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SCIENCE PROGRESS

IN THE TWENTIETH CENTURY

A QUARTERLY JOURNAL OF SCIENTIFIC WORK & THOUGHT

EDITOR

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IN CONSULTATION WITH

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SCIENCE PROGRESS

RECENT ADVANCES IN SCIENCE

ASTRONOMY. By R. W. Wrigley, M.A., Royal Observatory, Edinburgh.

The Lowell Observatory, in a circular dated March 13, made the important announcement of the "Discovery of a Solar System Body apparently Trans-Neptunian." The search programme for such an object was commenced in 1905, in accordance with the prediction of Dr. Lowell in his *Memoir on a Trans-Neptunian Planet*, published in 1914. This was necessarily based on the outstanding residuals in the motion of Uranus, for Neptune had not been observed for a sufficient length of time to enable the unperturbed elements to be obtained. Two possible solutions emerged, in which the longitudes of the predicted planet differed by about 180°, and both regions of the sky were subjected to an exhaustive search. Last year the new Lawrence Lowell telescope, specially designed for the work, was brought into commission, and on plates taken with this instrument Mr. C. W. Tombaugh, by means of the Blink Comparator, found an exceptional object. It was first recorded on a search plate on January 21, and, following a slow retrograde motion, its position on March 12 was given as R.A. $7^h\ 15^m\ 50^s$, Dec. $+22° 6' 49''$. The longitude therefore showed agreement within about 3° with the prediction of Lowell, but the brightness (15th magnitude) was considerably less than was expected, and no indication of a planetary disc could be detected. Since then the object has been observed both photographically and visually at numerous observatories. At the time of writing the latest orbit comes from Flagstaff, and is given in *I.A.U. Circular*, No. 271. The elements given are truly remarkable. The new body apparently moves in an ellipse with the high eccentricity 0·909, semi-major axis 217 units, perihelion distance 19·64 units, time of perihelion passage 1900·5, inclination 17° 21′, longitude of perihelion 12° 52′, ascending node 109° 21′, period 3,191 years; present distance from sun 41·3 units. It is stated that some revision will probably be necessary, especially in the eccentricity, owing to the shortness of the arc at present available. If the values given are correct, the object should have been of

about the 12th magnitude when at perihelion in 1900, and should therefore be readily found on old photographs of the region in question. In *I.A.U. Circular*, No. 273, Dr. A. C. D. Crommelin gives the heliocentric position in 1900·50 as R.A. $1^h\ 13{\cdot}2^m$, Dec. $-10°\ 54'$, and in 1909·36 as R.A. $4^h\ 1{\cdot}6^m$, Dec. $+7°\ 1'$.

In *Monthly Notices R.A.S.*, vol. 90, no. 1, H. Spencer Jones describes his revision of Newcomb's famous memoir "Researches on the Motion of the Moon, Part II" (*Astronomical Papers of the American Ephemeris*, No. 9, 1912). Newcomb utilised observations of occultations extending from 1672 to 1908, while Hansen's Tables, with certain corrections to reduce the elements to what he termed "the provisionally accepted theory," were used as a basis for computation. The completion of Brown's Tables of the Moon has made a revision desirable, and in the present work Spencer Jones has introduced longitude corrections to take account of the principal terms of long and short period omitted by Newcomb, in addition to correcting those depending on the figure of the earth. An extension in the number of the occultations has also been made possible through the publication of the Cape series up to 1922. The more rigorous comparison between theory and observations has produced certain small corrections to various constants. Brown's values of the longitudes of the moon's perigee and node are found to need corrections of $-1{\cdot}64''$ and $-1{\cdot}78''$ respectively; Newcomb's equinox at epoch 1850 is too high by $0{\cdot}047^s$, but his declination system and proper motions in declination in the neighbourhood of the Equator are closely confirmed. Agreement is shown with Brown's adopted value of the coefficient of the principal term in latitude. The derived coefficient of the principal term in the parallactic inequality is $-125{\cdot}023''$, corresponding to a value for the solar parallax of $8{\cdot}796'' \pm 0{\cdot}004''$, while for the Cape observations alone the value is $8{\cdot}804'' \pm 0{\cdot}005''$, giving a mean of $8{\cdot}799'' \pm 0{\cdot}003''$. The agreement furnishes a strong argument in favour of the accuracy of this method of determining the solar parallax. The revised differences in the moon's longitude as between theory and observation are not substantially changed. The systematic differences between the occultation values and those derived from the Greenwich meridian observations therefore still persist, and are well shown in a large diagram.

Brown has called attention to an apparent correlation between the minor fluctuations in the moon's longitude and these differences. The latter are most pronounced between 1788 and 1830, and during this period the observations of the sun, theory minus Greenwich, show a very similar fluctuation. From 1830 onwards the differences between the

occultations and the meridian observations are much smaller, rarely amounting to 1″, and Brown's correlation becomes less marked. Jones considers that the large differences observed for the earlier period are due to systematic errors in the Greenwich observations which have also affected the observations of the sun, and that the correlation with the moon's minor fluctuations is purely accidental. On the other hand, Brown has stated that in his opinion the key to the whole question of the hitherto unexplained discordances between theory and observation in the motion of the moon may lie here. Having regard to the uniform excellence of the Greenwich observations it is difficult to believe them capable of serious systematic error, and it is unfortunate that there is no other similar series of lunar meridian observations with which they might be compared. Meanwhile, the mystery of the apparent irregularities in the earth's rotation and in the length of the day still remains unsolved. Brown suggests that the cause may lie in changes in the earth's crust, in which case geology as well as astronomy may be expected to make a contribution to the problem.

An interesting problem, towards the solution of which numerous investigators are now working, is presented by the remarkably high receding velocities of the extra-galactic spiral nebulæ. These bodies, generally supposed to be galactic systems like our own, are situated at distances from the sun reaching to 50×10^6 light years, and are found to have motions away from us ranging from 300 km. per sec. to well over ten times that velocity. According to Humason (*Proc. Nat. Acad. of Sciences, Washington*, vol. 15, no. 3) the radial velocity of N.G.C. 7619, corrected for the solar motion, is + 3,910 km. per sec., and in the same publication E. Hubble demonstrates results which establish a roughly linear relation between the measured velocities of recession and the distances of the nebulæ. This corresponds approximately to 500 km. per sec., as shown by the red-shift of the spectral lines, for an increase in distance of 10^6 parsecs., though there are large deviations when the nearest nebulæ are considered. This velocity-distance relation is to be expected in the de Sitter cosmology, in which a line shift in the spectra is produced both by an apparent slowing down of atomic vibrations and by a real tendency of material bodies to scatter in space. In the *Astrophysical Journal*, vol, 69, no. 4, R. C. Tolman examines whether the de Sitter conception of the universe leads directly to Hubble's linear relation. He finds that while it implies a Doppler effect which tends to be positive and to increase with distance, the observed linear correlation between the two can be secured only by making additional assumptions regarding the distribu-

tion of the nebulæ. A purely geometrical explanation on the assumption that the universe follows de Sitter's solution of Einstein's equations seems to be beset with difficulties. Another suggestion involving a new effect of masses upon light, a sort of gravitational analogue of the Compton effect, is put forward by F. Zwicky in *Proc. Nat. Acad. Sci.*, vol. 15, no. 10. According to the relativity theory, a light quantum $h\nu$ has an inertial and a gravitational mass $\frac{h\nu}{c^2}$, and it should be expected that a quantum $h\nu$, when passing a mass M, should not only be deflected but should also transfer momentum and energy to M. During the process, the light quantum would naturally change its energy and therefore its frequency. The light of very distant objects would thus become reddened during its long journey through space, the amount depending on both the distance and the density of matter in space. A similar effect should also be perceptible in the case of the globular clusters, for in their case the much larger density inside the galactic system should compensate for their much smaller distance away. This has been investigated by P. ten Bruggencate (*Proc. Nat. Acad. Sci.*, vol. 16, no. 2). He finds that there does exist a linear correlation between the observed velocities of the globular clusters and their galactic latitude, similar to Hubble's correlation between the velocities and distances of the spirals, and states the possibility that it may express a physical relation between red-shift of the spectral lines and the total amount of matter between us and the clusters. Further, the amount of the red-shift seems to be of the general order required by Zwicky's formula. This result would seem to be in definite contradiction to any purely geometrical theory such as de Sitter's, and renders less likely the interesting possibility that from these observations numerical data may be secured for a discussion of the general curvature of space.

The progress in the construction of the projected 200-inch reflector for the California Institute of Technology in Pasadena cannot fail but be of intense interest to all astronomers. If this giant instrument can be brought into commission it will be ten times as powerful as the Hooker 100-inch, now the largest in the world, and will be able to explore a volume of space thirty times that already sounded. In the *Proceedings of the American Philosophical Society*, vol. 69, no. 1, Elihu Thomson, of the General Electric Co.'s research laboratory, gives an account of the preliminary work which is preparing the way for the construction of the great mirror. For a surface area four times that of the 100-inch and for a weight approximating to 30 tons, it has been found that fused silica is a much more suitable material than glass by reason of its very

small coefficient of expansion for a change of temperature. The body of the mirror is to be a comparatively rough mass of quartz, and is to be covered with a surface layer of clear silica glass. This is sprayed on by means of an oxy-hydrogen flame into which high-grade silica is introduced, and by this method a surface layer of any desired thickness, beautifully clear and transparent, can be readily deposited. The subsequent grinding, polishing, and figuring are then practically identical with the processes used for glass, with the great advantage that the work can go on steadily without the interruptions for equalisation of temperature necessary hitherto. Satisfactory mirrors up to two feet in diameter have already been produced by this method, and it is intended to increase the size by degrees until at last the great mirror itself is reached. Meanwhile, each of the intermediate mirrors will be a valuable acquisition for astronomical work. The completed instrument is to have a focal length of 55 feet, representing a ratio of F3·3, with a possible combination of mirrors giving F2.

In this connection reference should be made to the publication, under the auspices of the Société Astronomique de France, of a pamphlet by G. W. Ritchey describing his new principle of building up large cellular mirrors from comparatively small plates of low expansion glass, pyrex, or fused quartz. He claims that mirrors so constructed are optically permanent, can be supported in the telescope without perceptible flexures, and, through having forced ventilation, will suffer no change of figure when exposed to the temperature variations incidental to a night of observing. The proposals as to mounting are equally interesting, and aim at enabling the observers to work always in a stationary and comfortable position. It is claimed that an 8-metre "Fixed Universal" telescope would present no serious difficulties in construction, and that it would render possible photographs of star fields and spiral nebulæ under a magnifying power of 37,000 diameters. The pamphlet is illustrated by reproductions of photographs of star clusters, nebulæ, and star clouds taken with telescopes constructed by Dr. Ritchey. These are of an excellence surpassing anything of this nature already published, and cannot fail but engender firm confidence in the ability of Ritchey and the firm of Saint-Gobain to implement their claims. It is greatly to be hoped that circumstances will permit of the erection without undue delay of one of these super-telescopes in a suitable climate and latitude. Ritchey himself advocates Desert View, Arizona, for its location, and supplies a picture of the proposed observatory as visioned on the edge of the Grand Canyon.

In the publications of the Astronomical Society of the Pacific, February 1930, Dr. Knut Lundmark announces his intention of compiling a new catalogue of nebulæ, to supersede the well-known N.G.C. of Dreyer, which includes comparatively few observations based on photographic work. The aim of the new catalogue is to connect the visual and photographic series of measures, and to establish a reference system as accurate and as homogeneous as possible, and it is intended to include about 35,000 objects. The data will include the size, brightness, form, degree of concentration, and type of each object, together with other characteristics of interest, such as the length of the spiral arms, the area of dark regions, and the light of secondary nuclei. The catalogue is to be kept at the Observatory of Lund, and brought up-to-date in accordance with future observations, while information will be provided, when requested, to any workers in this field. Dr. Lundmark specially invites suggestions and criticisms in connection with this important project.

PHYSICS. By L. F. Bates, B.Sc., Ph.D., F.Inst.P., University College, London.

The Abundance of Isotopes.—One or two points of great importance are raised in a paper by F. W. Aston (*Proc. Roy. Soc.*, **126**, p. 511, 1930) on the photometry of mass spectra and the atomic weights of krypton, xenon, and mercury. He points out that, whereas it is impossible to obtain a mixture of elements which may be considered a fair sample of cosmic matter, it is almost equally impossible to obtain a mixture of isotopes which is not a fair sample of a complex element, and that, therefore, the study of the relative abundance of isotopes may yield results more directly applicable to the fundamental problem of the evolution of matter than the study of the relative abundance of elements on the earth is likely to do. Two methods have so far been used in the study of the abundance of isotopes. Sir J. J. Thomson used a form of positive ray analysis, and caused the parabolas produced by particles of different masses to fall one after the other upon a parabolic slit behind which was a Faraday cylinder. Assuming the discharge to be maintained constant, the charge collected by the cylinder in unit time was a measure of the number of particles in the parabola, and, therefore, of the relative abundance of a particular isotope. A similar device was used by Dempster in his researches on the isotopic constitution of magnesium and other metals, where he balanced the charge carried by the rays of specified mass against an adjustable ionisation leak. Here again the constancy of the discharge conditions played an important part, but the values of the

atomic weight found in these experiments agreed well with those found by purely chemical methods, and abundance ratios were obtained for the isotopes of lithium, potassium, calcium, magnesium, and zinc. In order to retain the photographic adjustments of his mass spectrograph, Aston did not pursue the Faraday cylinder method in the present investigations, but took advantage of recent improvements in optical photometry to determine the relative abundance of isotopes by observations on the optical intensities of the lines produced on a photographic plate. The rays only affect the surface of the plate so that a strong and rapid developer had to be used. The micro-photometer used to measure the density of the image was based on Dobson's principle of balancing opacity against a standard wedge by a mill method. The difficulties caused by changes in the discharge tube were overcome by producing two separate images of each isotope line on the plate. This was done by altering the electric deflecting field by one-half per cent., so that each isotope line was slightly displaced to a new position without pronounced alteration in intensity. By successive application of the two slightly different electric fields by a commutator device, one field being maintained for a longer duration than the other, two images corresponding to a definite ratio of exposures, more or less independent of discharge fluctuations, were obtained for each isotope. Krypton, which possesses many special advantages, was used for the initial survey. It was found advisable to record six or seven spectra on the same photographic plate in order to get a reliable mean value of the image densities from the photometer measurements. In this work Aston introduces the term isotope moment of an element, which he defines as the sum of the products of the abundance of each particular isotope and the displacement of this isotope from the mean mass number on the mass scale. This product is zero for a simple element, and unity for a complex element with two equally abundant isotopes two units apart. In the case of Krypton the isotope moment was found to be 0·87 and the mean mass 83·77 ± 0·02 on the chemical scale, which differs considerably from the generally accepted value from other sources. Xenon gave the high isotope moment 1·71 and an atomic weight 131·27 ± 0·04, which again is much larger than the value actually in use. The experiments with mercury were very helpful, and gave confidence in the above findings, as in this case the isotope moment was 1·40 and the atomic weight 200·62 ± ·05, which is in excellent agreement with the latest mean value 200·61 (1925). Moreover, the isotope moment of mercury has been determined by an independent investigation, by Bronsted and Hevesy (*Phil. Mag.*, 43,

p. 31, 1922), on the separation of the isotopes of mercury by the free evaporation method, the same value being obtained. It is yet too early to draw exact conclusions from the abundance ratios so far measured, but it is clear that an important method of attack on an interesting problem has been initiated.

Contributions from Leiden.—In a paper on the law of magnetisation of solid crystals, Becquerel and de Haas (*Commun. Leiden*, **193**, p. 3, 1930) preface their remarks with an interesting historical review of the problem of the Faraday rotation of the plane of polarisation, and they show decisively that such rotation depends on the paramagnetic properties of the medium through which the light passes in the magnetic field. We know that, as a rule, diamagnetic substances give a positive rotation and paramagnetic substances a negative one. We know, too, that when circularly polarised rays are passed through crystals of the rare earths, that a dissymmetry exists in the absorption of the oppositely polarised rays in the neighbourhood of their absorption bands, and that this dissymmetry and the magnetic rotation of the plane of polarisation, which has opposite signs at the two sides of a band, have been for some time now ascribed to a paramagnetic orientation of groups of electrons. The great rotatory power of crystals of the rare earth group is caused by absorption bands in the ultra-violet, and the sign of the magnetic rotation which we observe is negative, apart from local disturbances produced by absorption bands in the visible region of the spectrum, because we deal with wave-lengths on the long wave side of the band in the ultra-violet. Now, paramagnetism becomes increasingly pronounced as we lower the temperature of a body, so that Becquerel and de Haas carried out experiments at the temperature of liquid helium to magnify the effects associated with paramagnetism as much as possible. They used crystals of tysonite because it possesses the greatest magnetic rotatory power so far recorded, because it is very transparent, and may therefore be used in thicker plates than other crystals, and because its absorption bands in the visible region of the spectrum are, with one exception, not very active, and are separated by large intervals in which their influence is negligible.

The crystal section was placed in a cryostat consisting of three coaxial vacuum vessels. The innermost vessel contained boiling helium, the middle one boiling hydrogen, and the outermost one, boiling nitrogen. The vessels were obviously blown with great skill, for the light had to pass through twelve glass walls and five layers of liquid, the outer wall of the nitrogen vessel having a diameter of 15 mm. The cryostat

was placed between the poles of an electromagnet, and light from an arc was projected through holes in the pole pieces to traverse the crystal parallel to the lines of force. The crystal section was cut perpendicular to the axis, which was set parallel to the lines of force. The light incident upon the crystal was polarised with its electric vector vertical. A doubly refracting analyser of iceland spar was placed before the slit of the spectrograph, which received the transmitted light, so that two spectra, corresponding to horizontal and vertical electric vectors, were obtained in juxtaposition. A comparison spectrum from an iron arc was also simultaneously recorded by the spectrograph. On switching on the electromagnet, magnetic rotation occurred, and as the field increased, numerous black lines were displaced in the adjacent spectra from the violet towards the red. A particular line was chosen from the iron spectrum, and the field was determined when the black lines passed through the chosen iron line, so that the rotations, $n\pi$ in the spectrum of the horizontal vibrations and $(n\pi + \pi/2)$ in the spectrum of the vertical vibrations, were thus found. On plotting the rotation as a function of the field for the temperature 4·21° K., it was found that the rotation was not directly proportional to the field, and at 1·7° and 1·4° K. the deviation was considerable. The results obtained at 1·7° K. showed that the rotations for the same wave-length were proportional to the thickness of the plate, and that the law of variation of the magnetic rotatory power as a function of the field was independent of the wave-length. Efforts were made to determine whether the Langevin law of para-magnetism, which Woltzer and Kamerlingh Onnes (*Commun.* **167 C**) found to hold for powdered crystals of gadolinium sulphate, held for transparent crystals of tysonite. Actually, the rotation produced by the crystal was given by $\rho = \rho_\infty \tanh(C \cdot H)$. This meant, on the assumption that the rotation is directly proportional to the magnetisation of the crystal, that the law of magnetisation along one of the principal directions in a crystal is that given by Lenz (*Phys. Zeit.*, **21**, p. 613, 1920), and elaborated by Ehrenfest, namely $\overline{m} = \tanh\left(\frac{\mu H}{kT}\right)$, where $\overline{m}$ is the observed magnetic moment of a molecule of the crystal and μ the theorical value of the moment. This is a very important result. The magnetisation of tysonite was later found to be due to one particular ion, and two experimental values for C, gave $\mu = 0{\cdot}968$ and 1·026 Bohr magnetons respectively. The saturation constant, ρ_∞, was found to decrease with temperature for a given value of H/T. For a given value of H, however, it increased down to a temperature of 1·39° K., this being due,

presumably, to a dependence of the absorption bands on the temperature, which would explain why the rotation in weak fields or at higher temperatures does not exactly follow Curie's law, $\overline{m} = \frac{\mu^2}{3k \cdot T} \cdot H$, which is a limiting case of the more general law stated above. The general law is really equivalent to the statement that in a crystal the elementary magnetic particle may set either along or oppositely directed to a fixed direction in the crystal in the absence of a magnetic field. It would be unwise to generalise the law after the single example of tysonite, for it is shown (*Commun.*, **199**, p. 21, 1930) that the mechanism of magnetisation is the Ce^{+++} ion, whose single electron, in an incomplete layer, may be responsible for this particular behaviour. Magnetisation and spectroscopic data give the moment of Ce^{+++} greater than two Bohr magnetons, whereas these experiments give one Bohr magneton, so that we may here be dealing with direct evidence of the reversal of the sense of the magnetic moment of the elementary particles or electrons.

In *Commun.*, **199**, de Haas and Voogd describe experiments on the supraconducting alloy Bi_5Tl_3. They found that magnetic fields affect its properties as a supraconductor surprisingly little, *i.e.* they found that very high fields were required to restore the resistance of the conductor when at a temperature below the transition point. For example, at 3·4° K. a field of 5,300 gauss did not disturb the supraconductivity, and they consider that a field of even 9,000 gauss would be unable to do so at a temperature of 1·3° K. Hence, with wires of this alloy we should be able to produce fields of this magnitude without the production of heat. It is interesting to note that gallium has been found supraconducting below 1·07° K.

Whilst dealing with supraconductors we must take the opportunity to make a brief reference to the work of Meissner and Scheffers on the change of the resistance of gold crystals when exposed to a magnetic field at temperatures of liquid nitrogen, liquid hydrogen, and liquid helium. This work was described in the last issue of SCIENCE PROGRESS, and it will be recalled that the results were interpreted as being contrary to the hypothesis which Kapitza has made to explain his results; *viz.*, that the action of a magnetic field is to produce the same effect as imperfections in the crystal lattice. Kapitza (*Proc. Roy. Soc.*, **128**, p. 683, 1930) now shows that, when interpreted on the basis of his formulæ, the results of Meissner and Scheffers confirm his initial assumptions. His answer to the criticism that supraconducting lead at 7° K. has a resistance, calculated by extrapolation from Grüneisen's

formula, one million times less than that of an ideally pure metal, is very interesting. By plotting the ideal resistance data for lead above 7·65° K. against the logarithm of the absolute temperature, he shows that the ideal resistance of lead may be expressed as proportional to the nth power of the absolute temperature, where n continually increases as absolute zero is approached, so that by extrapolation it would be quite easy to obtain a very low resistance indeed for lead at low temperatures.

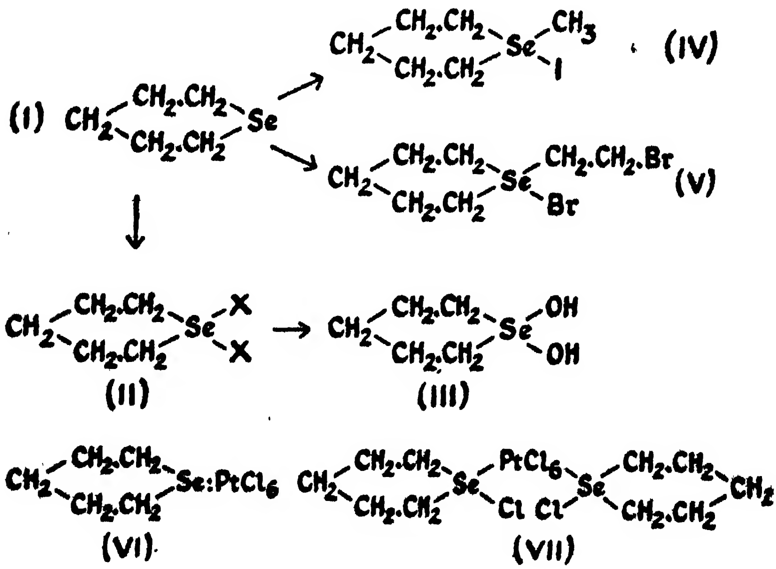
The Measurement of Dielectric Constants.—Two papers have recently appeared on the measurement of the dielectric constant of water and aqueous solutions. In the first, Drake, Pierce, and Dow (*Phys. Rev.*, **35**, p. 613, 1930) describe a method of using standing electric waves, in which the effects of stray field are avoided by confining the field to the space between a central wire and a surrounding metal tube. The usual Lecher wire system, then, is replaced by a brass pipe with a coaxial copper wire running through it, the pipe containing the liquid under investigation. A valve oscillator, standardised by a piezoelectric standard, is used to excite the system. A movable brass plunger, making spring contact with the wire and pipe, permits accurate settings at the positions of maximum reaction, *i.e.* at potential nodes, which are, of course, half a wave-length apart. From the known frequency supplied to the system and the wave-lengths in the dielectric, the velocity of the waves in the dielectric is computed. This method has been used to measure the temperature coefficient of the dielectric constant of distilled water and of KCl solutions. It is found, after due allowance for conduction effects, that the dielectric constant only slightly depends on the ionic concentration of the solution, and is nearly the same as that for pure water.

In the second paper J. Wyman (*loc. cit.*, p. 623) describes a method adapted to the measurement of the dielectric constant of a liquid, of which only quite small quantities are available. A small rigid resonator is connected to an oscillator whose frequency is varied until it corresponds to the natural frequency of the resonator, first in air and then when completely immersed in the liquid. If w_1 and w_2 are the natural periods in these two cases respectively, then the dielectric constant is given by $k = w_1^2/w_2^2$. The results for the dielectric constant of distilled water by this method agree very closely with those in the first paper. The resonators used possessed periods ranging from $1{\cdot}4 \times 10^{-8}$ to $81{\cdot}0 \times 10^{-8}$ sec., and the dielectric constants of liquids with conductivities one hundred times that of water should be measurable with resonators of period 10^{-8} sec., without appreciable error.

ORGANIC CHEMISTRY. By J. N. E. DAY, M.Sc., A.I.C., University College, London.

*cyclo*Selenopentane.—Morgan and Burstall (*J.C.S.*, 1929, 2197) describe the preparation and properties of *cyclo*selenopentane, thus completing the series of analogous ring compounds containing the oxygen-sulphur group of elements.

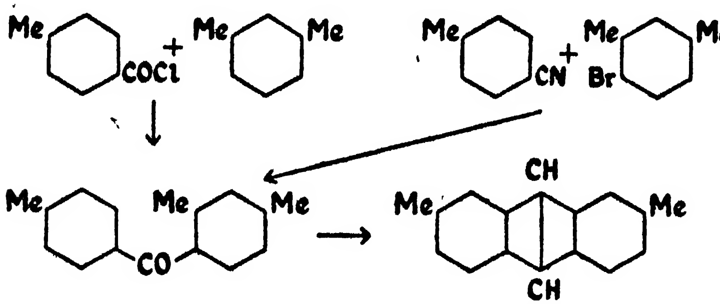
*cyclo*Selenopentane (I) was obtained from sodium selenide and αε-pentamethylene dibromide, and had b.p. 158°; with chlorine (bromine and iodine) it gave *cyclo*selenipentane 1 : 1-dichloride (1 : 1-dibromide and 1 : 1-di-iodide) (II, X=Cl, Br, I); the dichloride and dibromide, when treated with silver oxide in aqueous solution, gave *cyclo*selenipentane 1 : 1-dihydroxide (III). With methyl iodide, *cyclo*selenipentane 1-methiodide was obtained (IV), and, with ethylene dibromide, 1-β-bromoethyl*cyclo*selenipentane 1-bromide (V). Chloroplatinic acid and *cyclo*selenipentane 1 : 1-dichloride, in cold dilute solution, gave *cyclo*selenipentane 1-chloroplatinate (VI), and in hot more concentrated solution bis-1-chloro*cyclo*selenipentane 1-chloroplatinate (VII).



Anthracene Homologues.—Morgan and Coulson (*J.C.S.*, 1929, 2203; 2551) have now described the synthesis of 2 : 6- and 2 : 7-dimethylanthracene and 2 : 3 : 6-trimethylanthracene, required for reference purposes in the identification of anthracene homologues from the anthracene fraction of tar distillates.

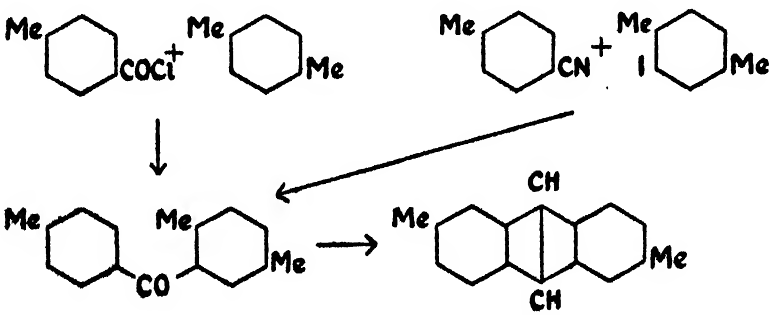
2 : 4 : 4′-trimethylbenzophenone (VIII) was prepared by condensing *m*-xylene with *p*-toluoyl chloride and also (in order

to prove the structure) by treating *p*-toluonitrile with the Grignard compound of 4-bromo-*m*-xylene. At its boiling-point, 2 : 4 : 4′-trimethylbenzophenone is converted (by loss of water) into 2 : 7-dimethylanthracene (IX).



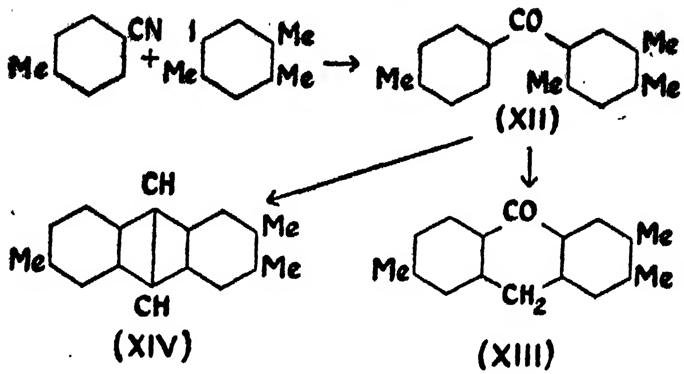
(VIII) (IX)

2 : 5 : 4′-trimethylbenzophenone (X) was prepared similarly by condensing *p*-xylene and *p*-toluoyl chloride (in the presence of aluminium chloride) and also (for the purpose of confirming its constitution) from *p*-toluonitrile and the Grignard compound of 2-iodo-*p*-xylene; at its boiling-point it is converted to 2 : 6-dimethylanthracene (XI).



(X) (XI)

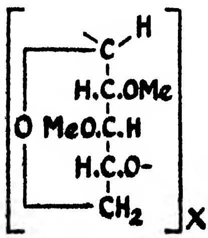
A similar series of reactions was used to prepare 2 : 4 : 5 : 4′-tetramethylbenzophenone (XII) (*a*) from 5-iodo-ψ-cumene and *p*-toluonitrile; (*b*) from *p*-toluoyl chloride and ψ-cumene. Boiling this ketone for eight hours gave 2 : 3 : 6-trimethyl-9-anthrone (XIII); but by continuous boiling for five days the required 2 : 3 : 6-trimethylanthracene (XIV) was obtained.

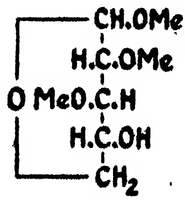


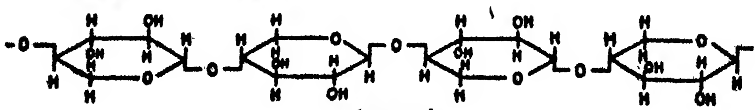
(XII) (XIV) (XIII)

Xylan.—In connection with the polysaccharides, the constitution of xylan is discussed in a paper by Hampton, Haworth and Hirst (*J.C.S.*, 1929, 1739). The xylan was extracted from esparto cellulose, and complete hydrolysis gave a 93 per cent. yield of crystalline xylose, indicating that xylan consists entirely of xylose residues, a point which until now had not been settled.

Improved methods of methylation gave a nearly quantitative yield of dimethyl xylan (XV) which, when hydrolysed with methyl-alcoholic hydrogen chloride, gave a dimethyl methylxyloside shown by the authors to be 2 : 3-dimethyl methylxylopyranoside (XVI). As therefore the hydroxyl groups in the 2 and 3 positions appear to take no part in ring formation or linking, the structure of xylan will be represented by (XVII).

(XV) 

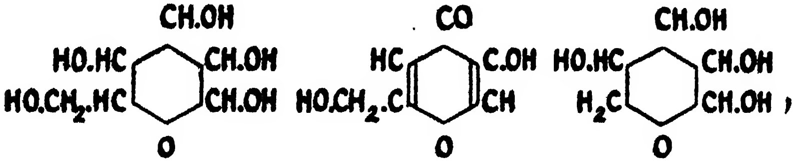
 (XVI)



(XVII)

Kojic Acid.—Kojic acid has been obtained by the action of moulds on sugars, and the similarity of the structure of this acid (XVIII) to that of glucose (XIX) has been pointed out. Less is known with regard to the action of moulds on

pentoses, and it is therefore of interest to note the work of Challenger, Klein and Walker (*J.C.S.*, 1929, 1498), who describe the production of kojic acid from arabinose (XX) and xylose by aspergillus oryzae.

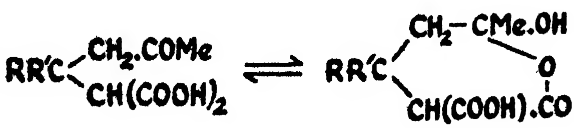


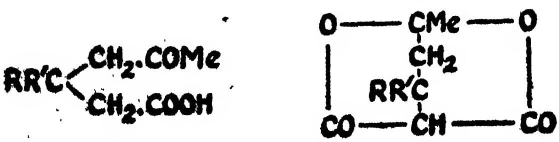
(XIX) (XVIII) (XX)

2 : 4-*Dinitrobenzaldehyde.*—Bennett and Pratt (*J.C.S.*, 1929, 1465) give an improved modification of the method for the preparation of 2 : 4-dinitrobenzaldehyde (condensation of 2 : 4-dinitrotoluene with *p*-nitrosodimethylaniline and hydrolysis) and point out the use of this compound as a reagent for condensing with methyl groups (see also Lowy and co-workers, *J.A.C.S.*, 1920, **42**, 849 ; 1921, **43**, 346 ; 1923, **45**, 1060), *e.g.* α-picoline gave α-dinitrostyrylpyridine ; 2 : 4-dimethylquinoline was condensed in two stages, giving 2-dinitrostyryl-4-methylquinoline, and, finally, tetranitro-2 : 4-distyrylquinoline.

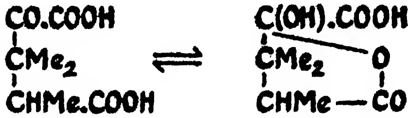
o-*Bromobenzoic Acid.*—The reactivity of the bromine atom in *o*-bromobenzoic acid, under the influence of copper or a copper salt, is discussed by Hurtley (*J.C.S.*, 1929, 1870) ; *e.g.* boiled with a solution of sodium and copper acetates, sodium *o*-bromobenzoate gave salicylic acid ; a powdered mixture of *o*-bromobenzoic acid and copper bronze, heated to 160°, gave cuprous bromide and cuprous benzoate ; ethyl malonate, copper acetate and *o*-bromobenzoic acid gave ethyl *o*-carboxyphenylmalonate.

Ring-chain Tautomerism.—In connection with the work of Thorpe and his collaborators on ring-chain tautomerism (see this Journal, 1925, **20**, 205) reference may be made to the work of Qudrat-i-Khuda (*J.C.S.*, 1929, 201 ; 713 ; 1913), who has studied the system (XXI–XXII).

(XXI)  (XXII)

(XXIII)  (XXIV)

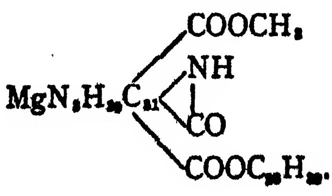
An example of this form of tautomerism is given by γ-keto-αββ-trimethylglutaric acid (Kon, Stevenson and Thorpe, *J.C.S.*, 1922, **121**, 650), which reacts in the two forms:



In the present papers, it is shown that some δ-ketonic acids exhibit keto-lactol tautomerism, and the effect of different substituents, in altering the carbon tetrahedral angle, has also been examined. The reactions of α-carboxy-γ-acetyl-ββ-dimethylbutyric acid cannot be entirely explained by the keto formula, and it is necessary to assume that the acid can exist in the lactol modification (XXI and XXII, RR′ = Me_2). When heated to 130°–140° the acid gave a mixture of the keto monobasic acid (XXIII, RR′ = Me_2) and the dilactone (XXIV, RR′ = Me_2) in the ratio 3 : 1. When, however, the *gem*-dimethyl group is replaced by the *cyclo*-hexane and *cyclo*-pentane rings, in the compounds cyclo-hexane-1-acetone-1-malonic acid (RR′ = $C_5H_{10}>$) and cyclo-pentane-1-acetone-1-malonic acid (RR′ = $C_4H_8>$) it was found that, as might be expected from previous work, in the *cyclo*-hexane compound, owing to the nearness of the carboxyl and carbonyl groups, this compound reacted more readily in the lactol form, the figures being 68–70 per cent., against 47–48 per cent. for the *cyclo*-pentane compound. The figures for *gem*-diethyl and methylethyl were found to be 45 and 32 per cent.

BIOCHEMISTRY. By P. Eggleton, M.Sc., University College, London.

Synthesis of Phytol.—The constitution of chlorophyll is not yet known, but a further advance towards knowledge of it has been made by F. G. Fischer and K. Löwenberg (*Annalen*, **475**, p. 183, 1929), who have synthesised phytol. Chlorophyll, it will be remembered, consists of the tribasic acid chlorophylline ($C_{31}H_{29}N_4Mg(COOH)_3$) esterified with one molecule of methyl alcohol and one of the alcohol phytol. The third carboxyl group is probably present in a lactam ring:



These authors reduced the ozonide of phytol with hydrogen, using palladium as a catalyst, and obtained glycollic aldehyde and the ketone $C_{18}H_{36}O$. The reaction was apparently:

$$\mathrm{C_{16}H_{33}\underset{\lfloor O-O-O \rfloor}{C(CH_3):CH}\cdot CH_2OH + 2H_2 \rightarrow \begin{array}{l} CH_2OH \\ | \\ CH_2OH \end{array} + C_{16}H_{33}\cdot CO\cdot CH_3 + H_2O}$$

It has been suspected on other grounds that phytol consists of condensed isoprene residues, and Fischer and Löwenberg attempted a synthesis on the assumption that the structural formula was:

$$\mathrm{(CH_3)_2CH[CH_2]_3CH(CH_3)[CH_2]_3CH(CH_3)[CH_2]_3C(CH_3):CH\cdot CH_2OH.}$$

Starting from ψ-ionone (1), they prepared the ketone (2), which was found to be identical with the ketone previously derived from phytol by partial destruction. This ketone was condensed with acetylene, and the product (3) reduced with hydrogen to the tertiary alcohol (4). The acetate of this alcohol underwent spontaneous conversion to (amongst other products) phytol (5):

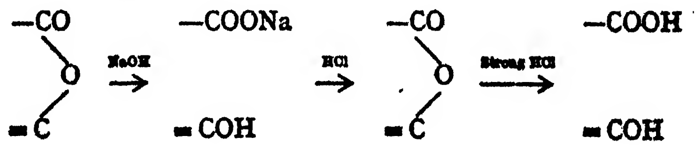
(1) $\mathrm{(CH_3)_2C:CH[CH_2]_2C(CH_3):CH\cdot CH:CH\cdot CO\cdot CH_3}$
(2) $\mathrm{(CH_3)_2CH\cdot[CH_2]_3CH(CH_3)[CH_2]_3CH(CH_3)[CH_2]_3 COCH_3}$
(3) $\mathrm{R - C(CH_3)(OH)C \equiv CH}$
(4) $\mathrm{R - C(CH_3)(OH)CH:CH_2}$
(5) $\mathrm{R - C(CH_3):CH\cdot CH_2OH}$

It remains now to ascertain the exact structure of the acid chlorophylline before the synthesis of chlorophyll can be attempted. Chlorophylline is known to bear certain superficial resemblances to hæmin, the red pigment of hæmoglobin: both, for example, contain substituted pyrrole rings. Three of the four nitrogen atoms of chlorophylline are probably in this form.

The Ovarian Hormone.—It is now eighteen years since the researches of Fellner (*Centralblatt für Allgemeine Pathologie und Pathologische Anatomie*, **23**) and others first clearly demonstrated that normal œstrus could be induced in ovariectomised female rabbits by injection of extracts of ovarian tissue. Fellner judged from the solubility of the unknown principle in different solvents that it was of a lipoid nature. When quantitative studies of this new hormone had been rendered possible by the vaginal smear technique introduced by Allen and Doisy (*Journal of the American Medical Association*, **81**, 1923), work on this subject began to accelerate, and fairly pure preparations of the hormone were made by concentrating it in the fatty constituents of the tissue, and subsequently extracting the fatty material with dilute acid. Although

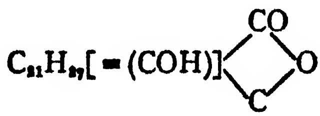
barely soluble in water, sufficient of it can dissolve to give reasonably potent solutions, which have the great advantage of containing no fat or protein. The use of such techniques permitted the demonstration of the presence of the hormone in the blood of pregnant females, and led naturally to its discovery in the urine of pregnancy (Aschheim and Zondek, *Klinische Wochenschrift*, **6**, 1927), which is now the best source for its isolation. Funk (*Proceedings of the Society for Experimental Biology and Medicine*, XXVI, 1929) has shown that the substance is a weak acid, forming salts soluble in ether. Its solubility in water is certainly less than 1 : 20,000 (Slotta, *Deutsche Medizinische Wochenschrift*, **53**, 1927). It is fairly resistant to high temperatures, but easily oxidised even by atmospheric oxygen. It is destroyed, though not readily, by saponification.

In the last twelve months the problem of the chemical nature of the hormone has been brought appreciably nearer to solution. Butenandt (*Deutsche Medizinische Wochenschrift*, **52**, 1929) succeeded in obtaining crystals of a very high physiological potency, and almost simultaneously Laqueur and co-workers (*Nature*, **125**, p. 90, 1930) achieved the same advance. Butenandt claimed a higher activity for his product, but the lack of standardisation in testing makes comparisons between the work of different laboratories difficult. Glimm and Wadehn (*Biochemische Zeitschrift*, **219**, p. 155, 1930) demonstrated faint acidic properties in preparations of very high potency, and suggested that the normal form of the molecule included a lactone ring rather easily opened by alkalis or strong acids :



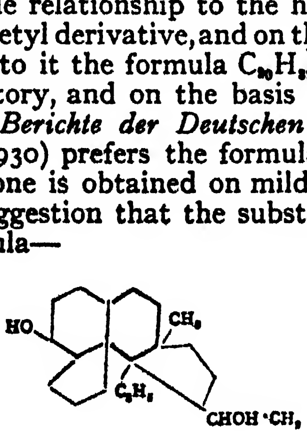
Marrian (*Biochemical Journal*, XXIII, 1929) utilised this weak acidic nature to effect a rapid purification of the hormone. He extracted it from aqueous solution with ether, extracting the ether solution with dilute aqueous potash. The potassium salt has a sufficient preferential solubility in water to make possible this procedure. By saturating the alkaline solution with carbon dioxide and again extracting with ether the process could be repeated if necessary. He obtained a product with an activity of seven million mouse units per gram. Laqueur has recently claimed (Laqueur, *et al.*, *Deutsche Medizinische Wochenschrift*, **8**, 1930) to have isolated crystals with an activity of eight million units per gram, these crystals contain-

ing 78·61 per cent. of carbon and 8·25 per cent of hydrogen, with no other elements save oxygen. Butenandt (*Naturwissenschaften*, **17**, p. 879, 1929) has introduced an additional method of purification in distilling the purest crystals at 0·02–0·03 mm. pressure, at which pressure the substance volatilises at 130–150°. The melting-point of his purest product is 243–245°, and it contains 78·31 per cent. carbon and 8·13 per cent. of hydrogen. He suggests the tentative formula—



which incorporates the lactone structure suggested by Glimm and Wadehn. In a recent paper (Butenandt and Von Ziegner, *Zeitschrift für Physiologische Chemie*, **188**, p. 1, 1930) he claims that the purest preparations to date have an activity of ten to thirty million mouse units per gram, as measured by the technique of Marrian and Parkes (*Journal of Physiology*, LXVII, p. 389, 1929).

An interesting sidelight on this subject is thrown by Marrian's discovery (*Biochemical Journal*, XXIII, p. 1090, 1929) of a sterol-like substance in the urine of pregnancy. It has no physiological activity, but its absence from other urine seems to suggest a possible relationship to the hormone. He found that it formed a diacetyl derivative, and on the basis of analytical figures he assigned to it the formula $C_{20}H_{32}(OH)_2$. Butenandt has taken up the story, and on the basis of slightly different analytical figures (*Berichte der Deutschen Chemischen Gesellschaft*, **63**, p. 659, 1930) prefers the formula $C_{21}H_{34}(OH)_2$. He shows that a diketone is obtained on mild oxidation, and has put forward the suggestion that the substance is a secondary alcohol of the formula—



It is difficult to avoid the suspicion that this substance is related to the hormone, and the discovery of its structure may help materially in elucidating the structure of the latter.

In its present immature state this subject—particularly on the physiological side—is difficult to describe coherently, but in two respects matters could be made considerably easier. At present this hormone rejoices in at least nine

different names in the literature. Of these the name "œstrin" suggested by Parkes and Bellerby (*Journal of Physiology*, **61**, 1926), based as it is upon the one known and indisputable action of the hormone, is undoubtedly the simplest and safest; but whatever the name chosen, the eight others should be dropped in the interests of lucidity. The other unnecessary complication lies in the absence of universal standards of assay. Not only are there "rat units," "mouse units," and the rest, bearing only the vaguest proportionality to each other, but one worker's mouse unit may be very different from another's; for the borderline between a positive and a negative result is to some extent arbitrary. Again, a dose of the hormone exerts greater effect if injected in quarter doses every six hours than if given all at once—owing presumably to a fairly rapid destruction. If we throw in the known wide variability of individual animals in susceptibility to pharmacological agents, the plea for some agreed standard technique for universal use needs no emphasis. A valuable practical contribution to this aspect of the subject has recently been published by Marrian and Parkes (*loc. cit.*).

GEOLOGY. By G. W. TYRRELL, A.R.C.Sc., Ph.D., University, Glasgow.

Metamorphism and Metamorphic Rocks.—For a clear, though short, statement of the modern physico-chemical principles underlying metamorphism the petrological reader is recommended to Dr. C. E. Tilley's article on that subject in the new *Encyclopædia Britannica.*

Dr. P. Eskola's closely-reasoned paper on the rôle of pressure in rock crystallisation (*C.R. Soc. Géol. de Finlande*, 1929, **1**, pp. 5–16) is chiefly concerned with refuting Prof. J. J. Sederholm's recent statement that the so-called "volume law"—*i.e.* that pressure favours the formation of those metamorphic minerals whose production implies a reduction of volume—does not hold good in metamorphism. Eskola emphasises the unity of modern conceptions of magmatic and metamorphic processes of rock crystallisation, which are governed by the same laws. The rôle of pressure has been over-estimated in the past, and seems in most cases to be only indirect. Eskola shows that the distinction between stress and anti-stress minerals is not so sharp as was at first thought.

In his comprehensive description of the geology of Ceylon Prof. F. D. Adams (*Canadian Journ. Research*, **1**, 1929, pp. 425–511) discusses the ancient crystalline rocks of which that island almost entirely consists, and which, he says, closely resemble the Grenville Series of the Canadian Shield. They are mostly biotite-gneisses containing at least two thick intercalations

of white crystalline limestone, which is often dolomitic, and is associated with beds of quartzite and with sillimanite-bearing rocks of sedimentary origin. Charnockite and allied rocks are present in large intrusions. The petrography of these rocks is fully described and illustrated by a valuable series of chemical analyses. The paper represents a contribution to the study of what the writer has termed "plutonic metamorphism." In structure Ceylon is a portion of a great syncline, deeply eroded, closed on the south, but open to the north, where it plunges beneath a Miocene cover.

Intense contact metamorphism, resulting from the intrusion of granodiorite into argillites, quartzites, and limestones of the Belt Series in the Pend Oreille district of Idaho, is described by J. L. Gillson (*U.S. Geol. Surv., Prof.*, Paper 158–F, 1929, pp. 111–21). The metamorphism took place in three overlapping stages. During the intrusion general recrystallisation took place, siliceous sediments changing to adinoles near the contacts, and to plagioclase-bearing rocks at greater distances, while limestone was altered to marble. Later on, emanations from the cooling magma caused the deposition of high-temperature silicates in the sediments. Still later, after the solidification of the igneous rocks, sericite, chlorite, serpentine, magnetite, and sulphides were formed by low-temperature replacement due to hydrothermal solutions.

Describing a partially-fused quartz-feldspar rock from Iceland, in which monomineralic areas of quartz and feldspar are separated by glassy matter, Dr. L. Hawkes (*Min. Mag.* XXII, 1929, pp. 163–73) suggests that the phenomenon is due to dry fusion under the prolonged influence of contact metamorphism, assisted by intimate granular contact of the minerals concerned. This action, however, is too sluggish to take place in the short period of a laboratory experiment, as Dr. J. A. Smythe shows by experiment on some of Dr. Hawkes's material.

Dr. L. L. Fermor has described the phenomena at the contact of basalt with a coal seam in Skye, and has compared them with some Indian examples (*Rec. Geol. Surv. India*, LX, Part 2, 1928, pp. 358–62). The lava has had little effect on the coal, but dikes in Indian coals have caused great alteration for distances up to four feet from their edges. The explanation lies in the different conditions attending extrusion and intrusion. In the Skye case the hot contact was evanescent, and the heat was quickly dissipated in the formation of steam, which acted as a protective cushion to the coal beneath. A dike, however, maintains a high temperature and strong metamorphic effect for a considerable time.

In a paper on " Séricitoschistes des chaînes du Mont Blanc et des Aiguilles Rouges qui sont des mylonites recrystallisées

postérieures au granit," Dr. A. Michel-Lévy (*Bull. Soc. Géol. France* (4), XXVIII, 1928, pp. 255–60) shows that numerous occurrences of sericite-schists and chlorite-schists are of mylonitic origin, and contain original material representing the remains of the crushed rock, and new material due to secondary recrystallisation subsequent to mylonisation. The memoir is illustrated by a magnificent series of micro-photographs showing a gradual transition from protogine to sericite-schist.

Dr. C. E. Tilley describes melilite as an abundant constituent of the zone of reaction between intrusive dolerite and chalk at Scawt Hill, Antrim (*Geol. Mag.*, LXVI, 1929, pp. 347–54). The melilite occurs both in the hybrid rock and in the exomorphic contact zone. It belongs to the humboldtilite class of the melilite group of minerals. The mineral has been formed as the result of assimilation of calcite by normal basaltic magma. This reaction is strongly endothermic, hence the hybrid zone is limited and local in its distribution.

In his important paper on metasomatism in the Whin Sill, L. R. Wager (*Geol. Mag.*, LXVI, 1929, pp. 97–110, 221–38) distinguishes two types of alteration: (*a*) alteration to "white whin" by lead vein solutions; (*b*) hydrothermal alteration by juvenile solutions. In the lead vein solution type the metasomatising agent was essentially an aqueous solution of carbon dioxide. The pyroxenes are replaced by carbonates, feldspars by mica and kaolin, and ilmenite by anatase. The original quartz is slightly increased in amount, but the apatite remains unchanged.

In the hydrothermal type of alteration the active solutions have been aqueous, with carbon dioxide, and a little sulphuretted hydrogen. A chlorite-quartz rock is the extreme term of alteration of the original quartz-dolerite. Hypersthene is altered to bowlingite, of which elusive material a valuable detailed description is given. The alteration takes place chiefly along the walls of an early jointing system. It is suggested that the joints were filled at a high temperature with a gaseous phase which made its appearance at the close of the solidification process, and that the hydrothermal solutions were produced by condensation of this phase.

In a paper entitled, "Über des Bornitvorkommen von Saint-Véren (Hautes-Alpes)," Dr. C. E. Wegmann (*Z. f. prakt. Geol.*, 36, 1928, pp. 36–43) incidentally discusses the origin of glaucophane in the ophiolitic or greenstone rocks of the Western Alps. He regards the formation of glaucophane as, on the one hand, a uralitisation, and on the other hand, as a process involving the addition of soda parallel to the process

of albitisation. The associated ore-magmas (Spurr's sense) possess a small sulphur content in relation to their heavy metal content, and the formation of glaucophane is believed to be partly dependent on this fact; while the formation of spilites and adinoles is associated more with ore-magmas containing a high sulphur content in relation to their heavy metal content.

Two important descriptive memoirs on Alpine metamorphic rocks by L. Bossard have appeared ("Zur Petrographie der unterpenninischen Decken in Gebiete der Tessiner Kulmination," *Schweiz. Min. u. Petr. Mitth.*, IX, 1929, pp. 47–106; "Petrographie der mesozoischen Gesteine in Gebiete der Tessiner Kulmination," *ibid.*, pp. 107–59). Prof. P. Niggli has summarised the chemico-mineralogical aspects of these rocks ("Die chemisch-mineralogische Charakteristik der metamorphen Paragesteinsprovinzen an Südrande der Gotthardmassivs," *ibid.*, pp. 160–87).

Volcanology.—In volcanological literature there are many references to erosion, mud-flows, and landslides which have their causes in heavy rainfalls, the alleged source of which is condensed steam from volcanic eruptions. From a study of the Kilauea explosion of 1924 and the contemporaneous rainfall, Dr. R. H. Finch (*Amer. Journ. Sci.*, XIX, 1930, pp. 147–50) derives data refuting the above conclusion. He shows that light rains accompanying some volcanic explosions may well have had part of their source in condensed steam, but that the main source of heavy falls is to be found in the moisture of the surrounding air.

The mudstreams or "lahars" of the volcano of Gunong Keloet in Java, which are produced by the overflow of the crater lake during an eruption, are shown by J. B. Scrivenor (*Geol. Mag.*, LXVI, 1929, pp. 433–4) to have a remarkable resemblance to glacial deposits. Many of the larger transported stones look like erratics, and the tumultuous agglomerate contains stones with polished and scratched surfaces.

Falcon Island, the famous "disappearing" island of the Tonga Group in the Pacific, is now in a reappearing phase due to active volcanicity. Its recent history is fully described by J. E. Hoffmeister, H. S. Ladd, and H. L. Alling (*Amer. Journ. Sci.*, XVIII, 1929, pp. 461–71; *Nat. Geogr. Mag.*, 54, 1928, pp. 757–66). It is a pyroclastic cone built by explosive eruption. When the volcanic energy wanes, wave erosion reduces it quickly to a submarine bank. Its final fate will be to be covered by limestone and uplifted into land, as in many other Pacific islands.

A good history of Bogoslof volcano (Aleutian Islands), another famous shifting island, and an account of its recent

activity, is given by Dr. T. A. Jaggar (*The Volcano Letter, Hawaiian Volc. Obs.*, 275, April 3, 1930). Bogoslof appears to be notable for the number of stiff lava spines or domes which have been extruded within the last two centuries from its submarine vents.

Since the spectacular rise of the crater obelisk of Mont Pelée in 1902, volcanologists have repeatedly shown that volcanoes are able to force more or less solid lava upwards within their conduits, and extrude it in the form of a spine or dome from their craters. Dr. Howel Williams draws attention (*Amer. Journ. Sci.*, XVIII, 1929, pp. 313–30) to the remarkable development of these domes in the Lassen Volcanic National Park of California. Here there are no fewer than 13 domes, consisting chiefly of a glassy dacite, within an area of 50 square miles. Most of them are situated on the flanks of the old Brokeoff volcano. The largest is Lassen Peak itself, and the most recent is only 200 years old.

In an important paper on the precipitation of limestone by submarine vents, fumaroles, and lava flows, J. E. A. Kania (*Amer. Journ. Sci.*, XVIII, 1929, pp. 347–59) shows that the elimination of CO_2 from sea water necessary for the precipitation of $CaCO_3$ may be effected by the heating and agitation of the water by submarine lava flows. A flow 20 square miles in area and 100 feet thick, issuing at 900°, and accompanied by the usual gas emission both from the lava and the vent, is estimated to be capable of precipitating a mass of limestone equivalent to 191 circular lenses each 500 feet in diameter and 50 feet thick. Gases are the main factor involved, and a submarine vent in existence for a million years could precipitate a mass of limestone 1,000 feet thick extending over 7,000 sq. miles. The lenses of limestone which are often found intercalated in lava flows may be thus explained.

Two important papers on chemical aspects of the emanations from the Valley of Ten Thousand Smokes, Katmai, Alaska, have been published by Dr. E. G. Zies (*Nat. Geogr. Soc. Contrib. Tech. Papers*, 1, Katmai Ser., No. 4, 1929, 79 pp.). In the first of these he has studied the fumarolic incrustations in their bearing on ore deposition. Incrustations in areas in which magnetite, molybdenum blue, fluorides, and sulphur were deposited have been specially studied. The analytical data show that nearly all the incrustations contain appreciable amounts of the following metallic constituents: lead, zinc, molybdenum, copper, arsenic, antimony, tin, and silver. In addition nickel and cobalt were found in the magnetite; thallium and bismuth in the molybdenum blue areas; selenium and tellurium in the sulphur areas. All these elements have the following property in common: their halides, sulphides,

or oxides have appreciable vapour pressures at the elevated temperatures found in the valley in 1919. No extensive ore body is likely to be formed, as the metallic constituents and acid waters can be readily carried into the drainage system of the area. The same metallic constituents were found in the fresh unaltered pumice of the region.

In the second paper the contributions of the acid constituents to the sea are considered. It is shown that approximately $1{\cdot}25 \times 10^6$ metric tons of hydrochloric acid and $0{\cdot}2 \times 10^6$ metric tons of hydrofluoric acid were exhaled into the atmosphere in a year's time from the Valley of Ten Thousand Smokes. At least three-quarters of these amounts are directly added to the sea. The hydrochloric acid alone is sufficient to make up 1 per cent. of the total deficit of chlorine in river waters, and the hydrofluoric acid supplied by the valley could contribute all the fluorine in the ocean in a period of eight million years. An interesting discussion of the geochemical history of fluorine is given, in which it is shown that great amounts of this element are removed from the sea through biochemical processes, and that even traces of fluorine can be removed from solutions through co-precipitation with calcium phosphate or calcium carbonate.

Sedimentary tuffs of Early Upper Cretaceous age have been found to occupy unexpectedly large areas in South-west Arkansas, South-east Oklahoma, and North-east Texas (C. S. Ross, H. D. Miser, L. W. Stephenson, *Prof. Paper* 154-F, *U.S. Geol. Surv.*, 1929, pp. 175–202). A few volcanic vents have been found in the region, including the famous diamondiferous peridotite pipe of Murfreesboro, Ark. Much of the tuff is of phonolitic and trachytic composition, and by intensive weathering some of the glassy varieties have been transformed into the valuable clay material known as bentonite. Near Murfreesboro there are a few restricted beds of peridotite tuff. These materials testify to widespread volcanic activity during the Cretaceous period over the south central States.

Thin strata, consisting almost entirely of turbid analcite crystals, occur on at least four horizons in the Green River formation of Utah and Colorado. The Green River formation is a series of Eocene lake beds that contain large deposits of oil shale. Other beds with less analcite than the above are clearly of tuffaceous character, and analcite is also plentiful in the oil shales themselves, accompanied by apophyllite, sanidine, quartz, and a little volcanic glass. These rocks, and others associated with the analcite beds, contain salt crystal cavities that strongly suggest the former presence of glauberite and anhydrite. According to W. H. Bradley (*Prof. Paper*,

158-A, *U.S. Geol. Surv.*, 1929, pp. 1–7) the field, microscopic, and chemical evidences point to the conclusion that the analcite and apophyllite originated in place at low temperature on the lake floor, by interaction between salts dissolved in the lake water and the dissolved fraction of volcanic ash that fell into the lake.

Prof. H. Cloos and E. Cloos have applied the senior author's methods of "granite-tectonics" to the determination of the structures and shapes of the Drachenfels and Wolkenberg intrusions in the Siebengebirge (*Z. f. Vulk.*, **11**, 1928, pp. 33–40, 93–5). By plotting the strikes and dips of the aligned sanidine phenocrysts of the Drachenfels trachyandesite, and the hornblende prisms of the Wolkenberg trachyandesite, the lines of magmatic flow were ascertained, and the Drachenfels shown to be a "quellkuppe" with steeply anticlinal flow-lines.

The same volume of the *Zeitschrift für Vulkanologie* contains valuable summaries of two Tertiary volcanic episodes, Austrian and Bohemian respectively (A. Winkler, "Der jüngtertiäre Vulkanismus im steirischen Becken," *Z. f. Vulk.*, **11**, 1928, pp. 1–32; H. Reck, "Zur Geologie der jüngsten Vulkane Böhmens, des Kammerbühls und Eisenbühls bei Eger," *ibid.*, pp. 96–109); a paper by Dr. Reck on "Zur Deutung der vulkanischen Geschichte und der Calderabbildung auf der Insel La Palma, *ibid.*, pp. 217–43; and a contribution to the petrography of the green tuffs of the Phlegraean Fields (in Italian) by A. Rittmann and E. Salvatore, *ibid.*, pp. 163–74.

ENTOMOLOGY. By H. F. Barnes, B.A., Ph.D., Rothamsted Experimental Station, Harpenden.

General Entomology.—The demand for a good textbook of general entomology has necessitated a second edition of A. D. Imms's book (*A General Textbook of Entomology*, etc., x + 703 pp., 607 figs., 1930, Methuen & Co., Ltd.). In this the chief alterations and additions are revised classifications affecting the orders Dermaptera, Isoptera, Thysanoptera, and Aphaniptera, while additional references have been added at the end of many of the chapters. Notes on recent advances are contained in the Addenda (pp. 668–72). *A Manual of External Parasites*, by H. E. Ewing (xiv + 225 pp., 96 figs., 1929, Ballière, Tindall & Cox, London), contains chapters on the Mallophaga or biting lice, the Anoplura or sucking lice, and the Siphonaptera or fleas. In addition there are chapters on mites and ticks, and one in which new genera are described. Keys are given to the families and genera which will make the book exceedingly useful to museum workers, general entomologists, and veterinarian officers. The size of the book apparently has prohibited the inclusion of more information

concerning structure, bionomics, and control measures. Mention must be made of the appearance of Part 1, Medical, of *Insects, Ticks, Mites, and Venomous Animals of Medical and Veterinary Importance*, by W. S. Patton and A. M. Evans (x + 786 pp., 374 figs. and 60 plates, 1929, published by the authors). Primarily written for medical officers, this book cannot but have a very wide range of use, and by publishing it themselves, the authors have been able to place a very moderate price on it. C. K. Brain has done a signal service to the farmers and entomologists of South Africa by his book, *Insect Pests and their Control in South Africa* (xx + 468 pp., 204 figs., 1929, Die Nasionale Pers Beperk, Cape Town). Every pest of importance to the grower and stock-raiser is successively considered, together with chapters on beekeeping, the relation of insects to human and animal diseases, and general principles of control. It is an excellent manual.

In an endeavour to solve the problem of the forces which determine the supply of oxygen to the tracheal endings, Krogh (1920) showed that the laws of diffusion of gases will explain the supply to the tissues of those quantities of oxygen which they actually consume, though in some cases it is supplemented by mechanical inspiratory and expiratory movements. He did not, however, deal with the tracheoles, but only with the tracheæ. This theory does not allow for any exigencies of respiratory mechanism, nor does it make provision for increased local requirements. V. B. Wigglesworth, in a very important paper (*Proc. Roy. Soc.*, B, **106**, 1930, 229–50), has put forward a theory which will provide for the increased demands for oxygen which arise locally in active tissues. If it be assumed that the terminal portions of the tracheal tubes are bounded by a semi-permeable membrane, then liquid will be drawn up the tubes from the tissues by capillarity until its progress is arrested by the osmotic pressure of the tissue fluids. During activity lactic acid, and probably other substances, will be produced, the osmotic pressure will rise, liquid will be absorbed, and air will extend down the tubes towards the active tissues. Experiments on the larva of the mosquito support this theory.

The prevalent idea that chitin causes the hardness and inflexibility of insect exocuticula is no longer tenable if the views of F. L. Campbell (*Ann. Ent. Soc. America*, **22**, 1929, 401–42) are maintained. This investigator, using a modification of the Van Wisselingh-Brunswick test, found chitin throughout the exoskeleton of several species of insects, in the peritrophic membrane of the American cockroach and honeybee, and in the blastoderm membrane of cockroach and grasshopper eggs; but no trace of it could be detected in

the chorion of these eggs, in the air sacs and attached tracheæ of the housefly and honeybee, or in the ootheca of the American cockroach. It is shown that only about 22 per cent. of the so-called "heavily chitinised" exocuticula of the American cockroach is chitin, while the "non-chitinised" endocuticula contains about 60 per cent. Further, the writer states that the hardening of the exocuticula is caused by a chemical or physical change in certain substances intimately associated with chitin, which are present with it in the cuticula when the hardening process begins.

G. C. Crampton, after making phylogenetic studies of the maxillæ (1923) and the neck and prothoracic sclerites (1926), has now (*Jl. N.Y. Ent. Soc.*, **37**, 1929, 453–512) made a comparison of the terminal abdominal structures of female insects throughout the orders. The formation of germ layers in insects is the subject of a critical review by L. E. S. Eastham (*Biol. Rev.*, **5**, 1930, 1–29). R. A. R. Gresson has made studies of the nuclear phenomena during oogenesis (*Q.J.M.S.*, **73**, 1929, 177–95) and yolk-formation (*loc. cit.*, 345–64) on certain Tenthredinidæ; these papers follow one by V. Nath and D. R. Mehta on the oogenesis of the firefly, *Luciola gorhami* (*loc. cit.*, 7–24).

Dorothy M. Needham, in a paper (*Biol. Rev.*, **4**, 1929, 307–26) on the chemical changes during metamorphosis of insects, describes the alterations in gaseous exchange and in carbohydrate and fat content of the organism. The nitrogen metabolism of this period is considered, the various fractions being followed as far as possible. The formation of silk and the differences in metabolism between insects forming large and those forming insignificant cocoons also receive attention. The question of the physico-chemical causes of the onset of hystolysis and of regeneration, and the question of the origin of the low respiratory exchange during the middle stage of pupation are discussed.

The biological problem of temperature is receiving more attention nowadays from entomologists, and at this juncture a review article, collecting the data concerning temperature coefficients, is particularly useful. J. Bělehrádek (*Biol. Rev.*, **5**, 1930, 30–58) deals with the reaction velocity in heterogeneous systems thus in protoplasm, showing that it obeys laws which are different from those established for reactions in homogeneous systems, and also that the velocity of biological processes depends on the rate of diffusion in viscous media. Bělehrádek further shows that the problem of temperature action in biology still remains an open question. In a further paper on factors associated with or producing cold hardiness, Miss N. M. Payne (*Ann. Ent. Soc. America*, **22**, 1929, 601–20)

shows that cold hardiness in the eggs and larvæ of *Hemerocampa leucostigma* Smith and Abbot bears a linear relationship to absolute humidity, and adds a note showing that the type of food affects the degree of cold hardiness exhibited by the Japanese beetle, in which insect absolute humidity does not play as important a part in cold hardiness of the adult as for the larvæ.

The effect of weather and climate in their relation to insects will always remain of vast importance. Two papers on this subject were read at the 1929 Conference of Empire Meteorologists, one by B. P. Uvarov (*Conf. Empire Meteorologists*, 1929, *Agric. Sect.*, **2**, 1929, 130–47) and the other by J. J. de Gryse (*loc. cit.*, 148–67). The latter contains observations made by officers of the Canadian entomological branch of the Department of Agriculture, while to the former is attached a short bibliography of publications referred to in the paper.

The attention of entomologists interested in parasitology must be drawn to a book, *The Cowbirds, a Study in the Biology of Social Parasitism* (C. C. Thomas, 1929, 421 pp., 29 plates, 13 text figs., reviewed by W. M. Wheeler in *Science*, **70**, 1929, 70–3), because of the numerous analogies with the parasitism of social insects. W. R. Thompson (*Bull. Ent. Res.*, **20**, 1929, 457–62) has examined in his usual way the part played by parasites in the control of insects living in protected situations. The advocates of biological control must be very gratified on reading the report by A. P. Dodd (*Commonwealth Prickly-pear Board*, 1929, 44 pp.) of the prickly-pear work being carried on in Australia. Very large areas are now being attacked by imported insects, and a considerable measure of control of this plant pest has been brought about. The advent of *Cactoblastis*, which is estimated to have destroyed at least 30,000 acres of prickly-pear chiefly in the last twelve months, has encouraged the board to believe that eradication of the plant will not prove to be so slow as was formerly thought. More than twenty years have elapsed since active work was begun to import from their natural homes and liberate in the United States of America the parasites and natural enemies of the gipsy moth and the brown-tail moth. A. F. Burgess and S. S. Crossman (*U.S. Dept. Agric.*, Tech. Bull. 86, 1929, 147 pp.) have now reviewed the results so far obtained, and, while admitting that the great value of parasite introduction has been demonstrated, stress the fact that much more work remains to be done, especially in correlating investigations carried out in the U.S.A. with corresponding work in other countries.

After five years' work on the insect visitors to fruit blossoms, G. F. Wilson has shown (*Ann. Appl. Biol.*, **16**, 1929, 602–29)

that, important as is the work of the hive bee as a pollinating agent, under certain conditions pollination may be carried out entirely by wild insects, principally humble and other wild bees (*Bombus* and *Andrena*) and flies (*Eristalis, Syrphus, Sciara*, Anthomyids, and *Calliphora*).

A very interesting account of the fauna of pitcher-plants from Singapore Island has appeared by C. Dover (*Jl. Malayan Branch Roy. Asiatic Soc.*, **6**, 1928, 27 pp.). More particular attention is paid to the resistance of mosquito larvæ to the action of the pitcher-plant. From experiments it would seem to appear that many of such larvæ living under these conditions possess a potential pepsin resistance; this, together with the fact that Culicid larvæ obtained from stagnant water only survive a few hours in pitcher fluid, indicates that environment plays an important part in changing the constitution of an organism.

E. A. Cockayne (*Trans. Ent. Soc. London*, **77**, 1929, 177–84) has collected together all the available information on the subject in a paper on spiral and other anomalous forms of segmentation in insects.

Orthoptera.—J. Zabinski (*Brit. Jl. Expt. Biol.*, **6**, 1929, 360–85), while studying the growth of *Periplaneta orientalis* and *Blattella germanica*, has found that, while the normal development cycle at 25° of the blackbeetle lasts about twelve months and that of the cockroach about three months, the blackbeetles cannot complete their cycle on a synthetic diet, although the cockroach is able to do so even when the only nitrogenous constituent is glycine. Even after prolonged inhibition of growth, the administration of normal diet allows these insects to resume growth and attain sexual maturity, including the production of normal young.

The thoracic mechanism of a grasshopper, and its antecedents, is examined in detail by R. E. Snodgrass (*Smithsonian Misc. Coll.*, **82**, 1929, 109 pp.).

Coleoptera.—A massive work by P. Luigioni, entitled *I Coleotteri d'Italia* (*Mem. d. Pont. Accad. d. Sci. I Nuov. Lincei*, Ser. 2, **13**, 1929, 1160 pp.) has made its appearance. It is a list of the beetles of Italy, including Corsica, Sardinia, Sicily, and the Maltese Islands, together with the literature dealing with them. One thousand one hundred and sixty-nine genera and 9,979 species are enumerated. This should be the standard work on the subject for many years.

A useful paper on the classification of beetles according to larval characters by A. G. Böving (*Bull. Brooklyn Ent. Soc.*, **24**, 1929, 55–80, 17 plates) is marred by several rather careless mistakes in the letterpress. A more important contribution to our knowledge of Coleopterous larvæ has been

made by A. W. Rymer Roberts (*Bull. Ent. Res.*, **21**, 1930, 57–72), who has constructed a key to the principal families in the larval stage. It may be pointed out that the last key of such a nature published was by Macgillivray in 1903. The larvæ of the sub-family Galerucinæ are described in detail, and a key is given for their identification by A. G. Böving (*Proc. U.S. Nat. Mus.*, **75**, Art. 2, 1929, 43 pp.); European species are included. Up to the present the genus *Niponius* has only been studied from the adult characters; now J. C. M. Gardner (*Bull. Ent. Res.*, **21**, 1930, 15–18) has described the early stages of *N. andrewesi* Lew.

In a study of the ecology of forest Coleoptera, after dealing with seral and seasonal succession in the Chicago area, O. Park (*Ann. Ent. Soc. America*, **23**, 1930, 57–80) discusses hibernation, suggesting that daylight intensity or possibly the relative length of day and night may play a part in this recurrent condition. F. A. Fenton and E. W. Dunnam (*U. S. Dept. Agric.*, Tech. Bull. 112, 1929, 75 pp.) have written up a detailed biology of the cotton boll weevil, based on work at Florence, S.C. In this bulletin the influence of climate on the seasonal cycle is especially studied. A very useful summary, with references, of the parasites of wireworms has been compiled by C. A. Thomas (*Ent. News*, **40**, 1929, 287–93).

The structure of the hind femur in Halticine beetles is described by S. Maulik (*Proc. Zool. Soc.*, Pt. 2, 1929, 305–8), and a useful list of jumping insects is appended. K. Mansour (*Q.J.M.S.*, **73**, 1930, 421–36) has made a preliminary study of the bacterial cell-mass (accessory cell mass) of *Calandra oryzæ*, the rice weevil.

Lepidoptera.—An important paper on the embryonic development of *Diacrisia virginica* Fabr. by O. A. Johannsen has appeared (*Jl. Morph. & Physiol.*, **48**, 1929, 493–526), and the following is taken from the author's abstract. The cells of the blastoderm, which are to form the serosa, are two- to four-nucleate; the smaller cells of the embryonic rudiment uninucleate. The amnion does not begin to form until after the serosa completely covers the embryo and yolk. The epithelium of the midgut arises from cells situated at the tips of the stomodæum and proctodæum. These cells are interpreted as part of the preprimordium of the endoderm. The amnion ruptures just before the larva begins to feed on the yolk, which still remains around it. The serosa is consumed before hatching.

Winefred M. Noyes has contributed a paper (*Bull. Ent. Res.*, **21**, 1930, 77–121) on moths attacking cocoa and confectionery—three species of *Ephestia*, viz. *kühniella*, *elutella*, and *cautella*, and *Plodia interpunctata* are the chief pests. Steps

are recommended for factory use. The report by J. W. Munro and W. S. Thompson on insect infestation of stored cocoa has also appeared (*Empire Marketing Board*, **24**, 1929, 40 pp.), and should be read in conjunction with the former paper.

A very wide range of aspects in the bionomics and control of the corn borer (*Pyrausta nubilalis* Hbn.) is covered by K. W. Babcock and A. M. Vance (*U.S. Dept. Agric.*, Tech. Bull. 135, 1929, 54 pp.), who give a brief summary and discussion of the results of four years' investigations (1924–7) in Central Europe. The fall army worm, *Laphygma frugiperda* S. and A. has been studied by R. A. Vickery (*U.S. Dept. Agric.*, Tech. Bull. 138, 1929, 63 pp.). This bulletin contains valuable information regarding this moth's parasites and bionomics. The biology of *Olethreutes cespitana* Hübner, together with that of *Anchylopera angulifasciana* Zeller, both small moths whose larvæ feed on clover, is the subject of a bulletin by L. P. Wehrle (*Cornell Univ. Agric. Expt. Sta.*, Bull. 489, 1929, 27 pp.). E. P. Breakey has contributed some notes on the natural enemies of the iris borer, *Macronoctua onusta* Grote (*Ann. Ent. Soc. America*, **22**, 1929, 459–64), in which he shows that various Diptera are the chief parasites.

Hemiptera.—J. G. Myers (*Parasitology*, **21**, 1929, 472–80), in a paper on facultative blood-sucking in phytophagous Hemiptera, gives a list of these insects recorded as attacking vertebrates, and, in the case of Homoptera only, other animals, together with observations on their behaviour, and notes on the alleged pathological effects attributed to Homoptera. A new coccid of the sub-family Lecaniinæ has recently been described, which has been obtained in two cases from the skin, causing an affection resembling scabies in one case and cutaneous myiasis in the other (Casazza, A. R., *Boll. Soc. med.-chir. Pavia*, 1928, review in *Rev. Appl. Ent.*, B, **17**, 1929, 47). This is claimed to be the first record of a Coccid occurring as a parasite of man or animals.

A valuable contribution towards a monograph of the Adelginæ (Phylloxeridæ) of North America has been made by P. N. Annand (*Stanford Univ. Publ. Biol. Sci.*, **6**, 1928, 1–146). After a short review of previous work, and accounts of the morphology and biology, the author enlarges on the taxonomic treatment of the group. All the known American species are described and well figured.

The dynamic aspect of the output of lac by the lac insect has been investigated by M. Venugopalan (*Indian Lac Assoc. Res.*, Bull. 3, 1929, 14 pp.). It has been found that the addition of complete manure generally gives better results than control plots, thus indicating the advantage of manuring to obtain

an increased output of lac. Further, although the duration of the life cycle of the insect varies from season to season, the period of vigorous lac production seems to be practically constant.

Two types of dorsal hypodermal glands in *Saissetia oleæ* are described by W. S. Marshall (*Trans. Wiscon. Acad. Sci., Arts and Letters*, **24**, 1929, 427–42). The function of the first type is the secretion of the wax covering the dorsal surface of the body, but that of the second type is not understood. The morphology of the repugnatory glands of *Anasa tristis* De Geer in the nymphal and adult stages has been worked out by D. L. Moody (*Ann. Ent. Soc. America*, **23**, 1930, 81–97). The morphology of the beet leaf-hopper, *Eutettix tenellus* Baker, has been considered, with special emphasis being laid on the digestive tract and its accessory glands, by G. F. Knowlton (*Utah Agric. Expt. Sta.*, Bull. 212, 1929, 24 pp.) with a view to elucidating the problem of transmission of beet curly-top virus disease by this insect.

R. M. Jones (*Ann. Ent. Soc. America*, **23**, 1930, 105–19), in a critical study of the life-history of the bedbug, *Cimex lectularius* Linn., using constant conditions of temperature and relative humidity, has found that the length of life varies inversely with the increase in degrees of temperature above 13° C., while relative humidities below 50 per cent. cause the death of the nymphs in less time than those between 50 and 75 per cent. He has also found, contrary to such workers as Girault, that the female will lay eggs before feeding in the adult stage.

Hymenoptera.—An interesting account, by L. O. Howard, of the travels of *Aphelinus mali* since it has been used for biological control work has appeared (*Ann. Ent. Soc. America*, **22**, 1929, 341–68). The bionomics of *Aphelinus jucundus* Gahan, an internal primary parasite of the geranium aphid, of *Aphidencyrtus inquisitor* Howard, an internal secondary parasite, and of *Asaphes americana* Girault, which is both an external secondary and an external tertiary parasite, have been studied by Grace H. Griswold (*Ann. Ent. Soc. America*, **22**, 1929, 438–52). Evidence is brought forward by A. D. Imms, in a study of the parasites of the frit-fly (*Parasitology*, **22**, 1930, 11–36), that at least two species, *Halticoptera fuscicornis* Walk. and *Rhoptromeris eucera* Htg., are of considerable importance. These two species, together with a third of less importance, *Loxotropa tritoma* Thoms., are described and figured in detail.

The plum sawflies, *Hoplocampa minuta* Christ. and *H. flava* L., are dealt with most thoroughly by L. Sprengel (*Zeits. f. angew. Ent.*, **16**, 1930, 1–86). The bionomics of the western grass sawfly, *Cephus cinctus* Norton, receives the attention of

C. N. Ainslie in a revisional bulletin (*U.S. Dept. Agric.*, Tech. Bull. 157, 1929, 23 pp.).

The Amazonian myrmecophytes and their ants are the subject of a contribution (*Zool. Anz.*, 1929, 10–39) by W. M. Wheeler and J. C. Bequaert. The latter gives detailed field notes and drawings, while the former gives a taxonomic account of the ants found. F. E. Lutz (*Amer. Mus. Nov.*, No. 388, 1929, 21 pp.) has made some interesting observations on leaf-cutting ants, *Atta cephalotes polita*, among which may be mentioned the fact that there is a correlation between the size of an ant and the weight of its burden.

Insular wasp faunæ, with particular reference to the Diploptera of the Bermudas, is discussed by J. Bequaert (*Ann. Ent. Soc. America*, **22**, 1929, 555–82). An important contribution to the ethology of the Meliponinæ by G. Salt (*Trans. Ent. Soc. London*, **77**, 1929, 431–70) is divided into two sections, the first consisting of notes on the nests and habits of Colombian stingless bees, and the second arthropods associated with Meliponine bees. On the causes of swarming in the honey bee is the title of a paper by D. M. T. Morland (*Ann. Appl. Biol.*, **17**, 1930, 137–49), in which the division of labour among bees of various ages is considered in its relation to the brood-rearing cycle, and a critical surplus of nurse bees is found to be associated with the formation of queen cells in preparation for swarming.

Diptera.—F. W. Edwards has now dealt with the family of non-biting midges, Chironomidæ, as found in Great Britain. Keys to, and brief descriptions of, all the species hitherto found in this country are given (*Trans. Ent. Soc. London*, **77**, 1929, 279–430). In a study of excessive abundance of the midge *Chironomus plumosus* at Lake Pepin, M. S. Johnson and F. Munger (*Ecology*, **11**, 1930, 110–26) found that some mud samples from the lake showed larvæ in excess of 7,000 per square yard, and that the great abundance seems to result from the enrichment of the lake bottom by pollution brought by the Mississippi River from the Twin Cities.

In the last *Advances*, C. W. Metz's work on sex ratio in *Sciara*, a Mycetophilid genus, was noticed. In a further paper (*Amer. Nat.*, **63**, 1929, 487–96) the cytological and genetic data are summarised, and the following conclusions drawn: (1) sex determination involves two distinct series of phenomena—(*a*) determination of the sex of the progeny, (*b*) determination of the sex of the individual; (2) sex is determined by the zygotic constitution of the females, presumably female-producing females are *Aa*, male-producing females *aa*, and males *aa* in constitution—*A* and *a* representing chromosomes or genes; (3) sex of the individual is determined in the ordinary

fashion by a pair of sex chromosomes (XX ♀, XY ♂); (4) *A* and *a* are probably " in " the X-chromosome or associated with it; (5) " unisexual " progenies are apparently the result of a selective elimination (inactivation) of sperms, not of zygotes; (6) the " precocious " chromosome is probably not a sex chromosome, and (7) the " sex limited " chromosomes are apparently not directly concerned in sex determination. In view of the rareness of records of " unisexual " progenies, it is interesting to see that H. F. Barnes reports it in *Rhabdophaga heterobia* H.Lw., a member of the Cecidomyidæ, a closely related family to the Mycetophylidæ (*E.M.M.*, **65**, 1929, 256–7).

A useful account of the bionomics of the clover-flower midge (*D. leguminicola* Lintner) by L. P. Wehrle has been issued (*Cornell Univ. Agric. Expt. Sta.*, Bull. 481, 1929, 35 pp.). A summary of the present knowledge of gall midges as enemies of aphids has been compiled by H. F. Barnes (*Bull. Ent. Res.*, **20**, 1929, 433–42).

J. N. Oldham (*Proc. Roy. Phys. Soc.*, **21**, 1929, 217–52) fills a gap in the knowledge of the detailed morphology of Tipulid larvæ by an account of the final larval instar of *Tipula paludosa* Meig. and *T. lateralis* Meig. The former is a purely terrestrial form, while the latter is intermediate between terrestrial and aquatic forms, as exemplified by *Pedicia rivosa* L., which the same author studied previously (1926). A comparison of the known species of Scatopsid larvæ has been made by Edith Lyall (*Ann. Appl. Biol.*, **16**, 1929, 630–8).

In Part 6 fascicule 1 (1929, 42 pp.) of *Diptera of Patagonia and Southern Chile*, which is being issued by the British Museum, H. Schmitz deals with the Sciadoceridæ, a new family, and the Phoridæ.

While the inheritance of morphological characters in animals has been more thoroughly investigated, the inheritance of biological and physiological ones has not yet received the attention that its importance warrants. W. W. Alpatov (*Jl. Expt. Zool.*, **56**, 1930, 63–71) has provided evidence that vestigial flies of stock used have a smaller size in the larval stage than wild long-winged ones, this difference being also observable in the pupal stage. He has also shown that slower development and metamorphosis take place in the vestigial flies than in wild *Drosophila*.

Having investigated the structure of the head and mouthparts of the Hippoboscidæ (1926) and the Nycteribiidæ (1928), B. Jobling (*Parasitology*, **21**, 1929, 417–44) had reinvestigated the Streblidæ, and compared his results with those reached in his former studies. These three families comprise the group Pupipara, all ectoparasites, on mammals and birds, bats and bats, with one exception on birds, respectively. The striking

resemblance in the Streblidæ and Nycteribiidæ, especially as regards the structure of the proboscis, is due to the phenomenon of convergence; the members of both families living under identical environmental conditions.

W. R. Thompson (*Trans. Ent. Soc. London*, **77**, 1929, 195–244) attempts to explain a number of the more obvious morphological characteristics of the larvæ of Muscoid Diptera as due to the interaction of relatively simple factors, susceptible at least in some degree to mathematical treatment, and discusses the general significance of the physico-mathematical method as applied to the problems of organic form and movement.

Other Orders.—A handbook of the Dragonflies of North America by J. G. Needham and H. B. Heywood (1929) has already been reviewed by C. H. O'D. in the January number. Methods of rearing dragonflies from eggs have been described by W. H. Krull (*Ann. Ent. Soc. America*, **22**, 1929, 651–8).

An ingenious piece of apparatus for the detection of substratum communication among termites has been devised by A. E. Emerson and R. C. Simpson (*Science*, **69**, 1929, 648–9).

The Collembola of Ireland have received the attention of H. Womersley (*Proc. R.I. Acad.*, **39**, Sect. B, 1930, 160–202). Useful keys for the identification of the 153 British species and 67 Irish ones are included. The Collembola fauna of South India has been dealt with by E. Handschin (*Rev. Suisse d. Zool.*, **36**, 1929, 229–62).

Parthenogenesis has been found to occur in the book-louse, *Trocles divinatoria* Mull., by O. W. Rosewall (*Ann. Ent. Soc. America*, **23**, 1930, 192–4). This is particularly interesting, as previously this phenomenon has been known only to occur in Hemiptera, Coleoptera, Hymenoptera, and Diptera.

ARTICLES

THE PRESENT STATE OF ASTROPHYSICS

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When, if ever, physics is an exhausted subject, and its history comes to be written, the present may or may not be recorded as its heroic age. We are prone to think so, but only the future can show how fundamental are the phenomena which remain to be discovered. Of one thing, however, we may be fairly certain. The present age will be described as an age of spectra. Band spectra, line spectra, X-ray spectra—these are the phenomena which have laid bare the detailed structure of the molecule and the atom. Chemistry gave us the constitution of the molecule, and made suggestions as to the grouping of its component parts in different compounds; but in the last resort it is relatively impotent to throw light on the dynamical connections. It was left to the analysis of band spectra—those beautiful systems of flutings which show up as sets of bright lines, spaced in marvellous regularity, when the compound is suitably stimulated—to ascertain the configurations and the dynamical binding of atom with atom by means of their attached electrons. The reason is clear. Chemistry studies only the cataclysm which accompanies the break-up of one set of compounds and results in the formation of another set. But by means of band spectra we may study the almost infinite variety of attitudes which the new-completed molecule can assume—its internal vibrations, rotations, and dislodgements of a binding electron from one niche to another. The study of the capacity of the molecule for heat—the quality of its energy-thirst—has, it is true, played its part, but it is concerned only with whole populations of molecules. For the structure of the molecule *per se* we must examine its light-thirstiness—we must examine those phenomena in which the molecule has scope for its self-expression. Similarly with atoms, the secret of the structure of which came first in historic development. Collision phenomena gave us Rutherford's picture of the hard core, the ruling sovereign with its court of satellite electrons. But X-ray spectra in the hands

of Mosely and line-spectra in the hands of Bohr were the phenomena which counted the electrons and allotted energies to them.

Atoms rarely soliloquise. Left to themselves they are the most taciturn of objects. But put them in relation with an æthereal society—quanta of radiation—and they become not only audible, but clamant, and not only clamant, but self-revealingly so. They are self-centred entities; each is concerned with singing the song of its own infinite variety. The spectroscope is the organ by which we listen, and listeners of insight have interpreted the song and recorded it in a special staff-notation. This notation expresses the notes the atom can sing, and the occasions on which and the intensities with which it bursts into song. The vocal anatomy of the atom is laid bare. Spectra of the most baffling complication, like those of iron and titanium, have been analysed in terms of energy values and electronic configurations, so that for each monochromatic absorption or emission of radiation which the spectroscope records, we know the alternative geometrical structures between which the atom is executing transitions. "La lumière, c'est moi." Our knowledge of the atom is practically entirely a knowledge of its reaction to light—visible, infra-red, ultra-violet, and Röntgen. Other aspects, such as the structure of its nucleus, less accessible to analysis by means of radiation, are largely an undeciphered document.

Such being the state of pure physics, it is of the highest significance that the first important step in the unravelling of spectra was made in connection with astrophysics. The stars are light-emitters, and it is only by analysis of their light emission that we can hope to determine their structure. We should therefore expect that this age of spectra would also be an age of rapid development of our knowledge of stellar constitution. So it has proved. The last century saw the descriptive phase of astrophysics; Huggins, Secchi, Vögel, Lockyer, Pickering, and many others described stellar spectra, and moreover used them to determine the velocities of the stars. But it is the present century which has transformed astrophysics into an exact science, exact not only in its measures, but also in the nature of its inferences and conclusions. The daughter of physics and astronomy, astrophysics has in this century attained her majority. Let us glance at her recent past and her probable future.

That fundamental spectroscopic step I have already mentioned was the discovery in 1860 by Kirchhoff that the dark lines in the spectrum of the sun coincided in wave-length with the bright lines in the spectra of terrestrial elements, and his interpretation of the coincidence as the presence in

the solar atmosphere of cool vapours (chemically a mixture of our known terrestrial elements), which absorbed certain constituents of the light attempting to pass from the heated interior. The chemistry of the sun, in Lockyer's phrase, was founded. We accept the chemistry, but we pass on to the physics, which is rather more complicated than Kirchhoff supposed. Imagine a star as a large globe of hot matter, dense in its interior, but thinning out near its confines as a gaseous envelope, ultimately of inconceivable tenuity. There is no discontinuity between the inner and the outer layers, no hard surface anywhere such as the earth possesses. When we speak of the atmosphere of a star, we must not think of it as the terrestrial atmosphere, nestling against mother earth. We use the term atmosphere simply to denote the semi-transparent outer layers, which shade continuously into the general fog which constitutes the interior layers. In these interior layers the atoms are broken down, owing to the high temperature and in spite of the high pressure, into free electrons and nearly bare nuclei, with negative and positive charges respectively. In some manner concerning which we are wholly ignorant, the interplay of these particles results in the liberation of radiant energy. We know this because we can measure the energy leaking out at the surface, and according to our calculations this energy-leakage goes on for incomparably greater periods of time than the mere cooling of the hot globe would permit. Even its reinforcement by gravitational contraction is hopelessly insufficient—there would have been far too short a time for the deposition of the sedimentary rocks in the earth's crust, still less for the measured age of the igneous rocks; arguments from cosmogony—the ages of double stars, clusters, nebulæ—show the need for still longer periods. Thus though the process of the radiation from the stars is essentially a cooling process, the loss by cooling is, practically, made good each second by atomic events in the far interior.

Consider now the outer layers of the star. They have a temperature of from 3,000 to 20,000 degrees, and we have no reason to believe subatomic energy can be liberated under these comparatively mild conditions. Hence the outer layers are mere transmitters of energy. Since they are approximately in a steady state in time, no energy accumulates inside them—they get rid, by radiation to outer space, of their gains of energy from the interior. We may draw net imports at any internal frontier and equal exports at the external frontier. But we can draw the interior frontier of these outer layers anywhere we please, provided we do not go too deep, for we have already seen that there is no layer of discontinuity. Consequently

the net imports are the same across any spherical surface concentric with the star, drawn not too deep. This is the fundamental property of a stellar atmosphere.

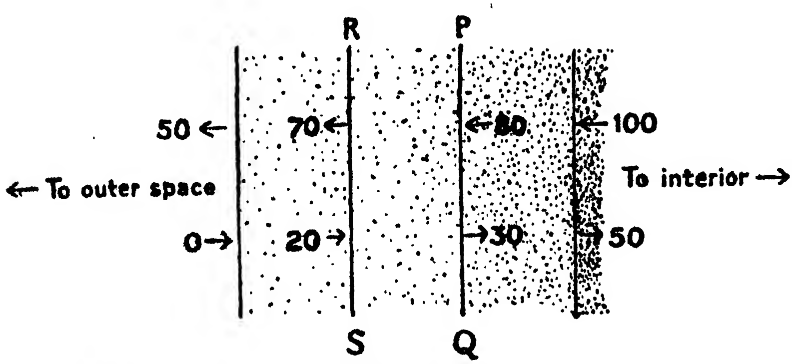
Consider next a single absorption line in the spectrum of the sun or any other star, say a line, due to the presence of calcium. In light of this wave-length energy is missing; here the sun is radiating less than it should. Kirchhoff supposed that the missing energy was simply abstracted from the outflowing stream by the cooler atoms of calcium; he had demonstrated that atoms can absorb the radiations they are capable of emitting. But he did not enquire as to the fate of the absorbed radiation. If it simply remained absorbed, the outer layers would be heating up, contrary to what we have seen. We now turn to the new atomic physics to help us out of our difficulty. In absorbing energy the atom springs from one electronic configuration to another of its other possible electronic configurations. But the energy which has been absorbed is held only momentarily—for some hundred-millionths of a second. The atom has been charged with energy; it now discharges. But it has no preferential direction of discharge, on the average. The energy is therefore emitted equally in all directions. But the initial absorption was on the whole an absorption of outward moving radiation. The net result is therefore to deflect backwards a certain portion of the outward stream. The outward stream is partially dammed back. I said earlier that the export of energy at the outer boundary of the atmospheric layer was equal to the *net* import at any internal boundary. Notice the word *net*. Here the gross imports exceed the net imports. The balance is made right by an export of energy at the internal boundary, the export representing the amount of energy deflected backwards by the absorbing and re-emitting atoms. There is at any fictitious internal boundary an excess of imports over exports, the state remaining steady because the excess, the net import at the inner boundary is exported at the outer boundary. Thus the presence of so-called absorption lines in a stellar spectrum is merely an indication of the trade-balance in the layer under consideration. No energy is actually absorbed.

Such a state of affairs is called "radiative equilibrium," and it is by analysis of radiative equilibrium that much recent progress in the analysis of the physical conditions in stellar atmospheres has been effected. The phenomena is at first sight paradoxical. The state is steady, yet energy trying to emerge is continually being deflected backwards. A numerical example will make the matter clear and carry our analysis a stage further.

Consider a layer of stellar atmosphere 50 kilometres thick.

Suppose this layer just foggy enough to be able fully to absorb the whole of the radiant energy falling on it from below (approximately true for the sun). Let this energy be represented by 100 units. Of this 100 units, when absorbed, half (50 units) is re-radiated outwards to space, and the other half (50 units) is re-radiated towards the stellar interior. The net amount leaving for outer space, the export at the outer boundary, is thus 50 units, and the net import at the internal boundary of the layer is 100 − 50 = 50 units; the two balance, and the state is steady.

Now consider a smaller layer, such as PQRS in the diagram, situated in the middle of the 50 kilometres layer already discussed. For a certain thickness of this smaller layer it may be calculated that 80 units enter PQ from the interior whilst



Radiative Equilibrium. Net flow of 50 units from right to left.

30 leave PQ for the interior, making a net import of 50; 70 leave RS for the exterior whilst 20 enter RS from the exterior, making a net export of 50, so that the trade-balance is maintained. The layer PQRS will be found to be about one-third of the thickness of the original layer. To see that this is consistent with all that has gone before, we recollect that the whole layer could absorb the whole of the 100 units incident on it. The smaller layer will therefore absorb one-third of what is incident on it. It thus absorbs one-third of 80 from the 80-beam and one-third of 20 from the 20-beam. Half of this total is re-emitted forwards and half backwards. Hence the beam of original strength 80 loses one-third of 80 but gains one-half of one-third of (80 + 20), leaving on balance 70, as originally stated. Similarly the beam of strength 20 incident on RS loses one-third of 20, but gains one-half of one-third of (80 + 20), increasing it to 30. We see that continuous backward deflection of energy is quite compatible with a net forward flow. Re-exportation is compatible with a steady transit of goods.

This numerical example shows us immediately a further point of great importance. At the interior face of our 50-kilometre layer we had 100 units moving inwards and 50 outwards. The total *density* of radiation here (the trade-activity) is thus 150. At PQ it is similarly (80 + 30) or 110; at RS it is (70 + 20) or 90; at the external boundary it is simply 50. We see that the radiation-density steadily decreases as we pass from the inner boundary to the outer boundary. In other words a radiation density-gradient exists. The net flow of energy is always down the density-gradient, and is proportional to the density-gradient. This gradient of radiation-density makes itself apparent as a temperature-gradient. Thus in radiative equilibrium the material becomes hotter as we penetrate more deeply. In our economic analogy we should say that there was an employment-gradient between the internal frontier and the external frontier.

The detailed analysis of radiative equilibrium on these lines, using the differential calculus to trace the beams through infinitesimal layers, was made by Schuster in 1905 and by Schwarzschild in 1906. The whole study of stellar atmospheres in the last few years has been dominated by their results. The reason for this is that what we actually observe is the net flow of energy through the outer layers of the star.

We can measure either total exports or the exports of one particular kind of goods. Suppose first of all we measure the *total* radiation of all colours, the heat radiated. This tells us first the average density of radiation in the neighbourhood of the boundary. The average density is one-half of the sum of the inward and outward beams, and since here the inward beam is zero the average density is one-half the observed total radiation. From this we can deduce the boundary temperature. If the star looks, say, the same colour as a carbon arc, and so mimics a radiator at 3,500 degrees, the actual temperature at the boundary will be 2,900 degrees, this being the temperature for which the density of radiation is one-half the density for 3,500 degrees. The point is that though the boundary is at 2,900 degrees the eye sees down, through the stellar fog, to the depth at which the temperature is 3,500 degrees. Secondly, we can infer from Schwarzschild's analysis something concerning the radiation density-gradient, and hence something concerning the temperature-gradient. This comes out as so many degrees "per unit of fogginess"—*i.e.* the temperature increase on traversing so much fog.

Next, we can measure the radiation of the star in some single colour. This enables us to convert the temperature-gradients into degrees per kilometre. The conversion clearly requires a knowledge of the intrinsic fogginess of the stellar

atmosphere. How to obtain this was a baffling question for many years. We are here concerned not with total exports, but with exports of a particular kind of goods. We have seen that in an absorption line the atoms offer great obstruction to the flow of energy, but we have seen that the obstruction is not perfect. The flow is reduced, but not to zero. The "dark lines" in the solar spectrum, called Fraunhofer lines, are not absolutely dark. Some leakage of light occurs through them—from one-tenth to one-half or even more of the light in adjacent wave-lengths struggles through. The line is darkest in its centre; in the "wings" it shades off until it merges into the undisturbed continuous spectrum where there are no selectively absorbing atoms offering special obstruction. The mode of variation of the outward flow in a line, from its blackest to its faintest, with varying wave-length, is called the "line-contour." As it were, bricks that are exported will be of various sizes; exports of bricks either larger or smaller than the average are less numerous than exports of bricks of average size. In the stellar case, exports of light at the average wave-length of a line are *least* numerous; they increase on either side as we depart from the line-centre.

The resources of photography and photometry have been called into service in the attempt to measure line-contours. Until the last few years astrophysicists were content to measure simply the *positions* of lines; now they are concerned with the more difficult question of the measurement of their intensities. Astrophysicists at many of the principal observatories are at present devoting the whole of their attention to this problem.

One principle that can be used to deduce the intrinsic fogginess of a stellar atmosphere is to compare the energy flowing through in an absorption line with that flowing through in adjacent unobstructed wave-lengths, and then to use the value of the intrinsic obstructing power of each atom provided by pure physics. Knowing the latter and observing the intensities we can infer a relation between the intrinsic fogginess of the atmosphere and the abundance of the element in question. By comparing different lines of the same atom, and lines of different atoms, in the same star, we evaluate the relative abundances of the chemical elements in stellar nature; by comparing the same lines in different stars we determine the absolute value of the intrinsic fogginess in stellar atmospheres, and so the absolute values of the abundances of the elements. In other words complete analysis of line-contours in stars will ultimately provide the absolute value of the opacity of stellar material near the surfaces of stars and the number of atoms of each species of element per cubic centimetre at

various depths. We can then calculate the temperature-gradients and pressures at various depths. The complete thermal, dynamical, and chemical structure of the atmosphere becomes known.

The following are a few results of this kind. The first table, recently published by Prof. H. N. Russell, shows the absolute amounts of various metals in the sun's atmosphere in units of 100 milligrams per column of one square centimetre cross-section, down to the average level to which we can see.

Lithium	0·0006	Chromium	25
Sodium	400	Manganese	40
Magnesium	1,500	Iron	1,000
Aluminium	60	Cobalt	25
Silicon	600	Nickel	60
Potassium	250	Copper	6
Calcium	200	Zinc	5
Titanium	8	Silver	0·001

The second table shows the mechanical and thermal structure of the atmospheres of the sun and Capella according to some recent calculations by the author.

Depth. (kms.)	Total fogginess to this depth.	Pressure (dynes).	Temperature (degrees).	Temperature gradient (degrees per km.)	Density (grams per cubic cm.)	Intrinsic fogginess. (cm.²gram.⁻¹)
			SUN			
—	0	0	4,830	0	0	0
12	0·009	6·7	4,840	4·5	$3·4 \times 10^{-10}$	75
21	0·103	23·2	5,000	40·5	11	222
31	0·582	64·5	5,650	113	28	359
41	1·650	140	6,590	141	51	388
48	2·854	222	7,310	147	73	385
			CAPELLA			
—	0	0	4,370	0	0	0
16	0·013	0·9	4,390	0·13	$0·5 \times 10^{-10}$	16
255	0·055	1·9	4,460	0·5	1·0	31
540	0·25	4·4	4,730	1·5	2·2	54
745	0·56	7·2	5,090	2·2	3·4	64
1020	1·26	12·7	5,700	2·7	5·4	68
1040	2·80	24·4	6,590	2·8	8·9	67

[The units in the second column are such that two-thirds corresponds to the average depth to which we see. The intrinsic fogginess is on a different scale. The basis on which this table is calculated is somewhat different from that used for the preceding. If, as suggested by Russell, stellar atmospheres consist largely of hydrogen, the depths would require multiplication by 10 and the temperature-gradients division by 10.]

When new methods are being developed, the earliest applications of them often lead to results of only inferior accuracy. So the foregoing tables must be regarded as pro-

visional. But in principle the problem of the steady state of a stellar atmosphere may be regarded as solved. The concept of a layer in radiative equilibrium combined with spectro-photometric measures of line-contours and of continuous spectra, determines the stratification. What importance has this result? In the first place it satisfies our curiosity. To know something of the nature of stellar atmospheres may be regarded as a legitimate end in itself. The fullness of our culture demands it. But it is not merely a closed chapter. The analysis of which I have given examples can be carried out completely only for stars of which we know the value of surface gravity, such as the sun. For stars whose mass is unknown this datum is missing. The real interest which attaches to the line of investigation is that it may be possible in the near future to *infer* the value of surface gravity for a star simply by photometric analysis of its spectrum, by reversing the analysis used previously. The intensities of the lines in the spectrum depend intimately on the mean pressure in the stellar atmosphere, and this in turn depends on surface gravity, since surface gravity determines the extent to which the various layers compress one another by their own weight. Surface gravity is not of great cosmogonic importance in itself, but it is a piece of evidence concerning the mass and radius of the star jointly. Additional information may permit these to be separated. A determination of the parallax of the star, for instance, fixes the absolute brightness; this combined with a determination of temperature (again from the spectrum) fixes the radius; and so the mass may be calculated. The masses of stars are known in general only for the components of double stars. Spectro-photometric observations will, however, ultimately produce evidence concerning the masses of single stars—a matter of great importance in studies of stellar evolution. The mass of a star decreases as it ages, since the outflowing radiation carries away mass, and hence the difference in mass between two stars is a measure of the difference in their ages. It is interesting to think that the grandest problems, of the birth of universes and the evolution of galaxies, are assisted in their solution by studies of tiny quantities of gas-producing faint markings in a spectrum. In such studies the whole of modern physics and the whole of observational astronomy are commandeered.

It is not in the studies of stellar atmospheres alone that spectro-photometric measures of line-contours have been applied. A question of great cosmic interest is the structure of the all-pervading calcium cloud in which we seem to be bathed. According to the theory of high-temperature ionisation the lines of ionised calcium should not be visible in the spectra

of the hotter stars; for their atmospheres are so hot that the ionised calcium atoms should be ionised still further, and therefore unable to give their usual spectrum. Yet such lines *are* visible in the spectra of the hottest stars. Calcium is therefore present somewhere in space between the star and the observer. The lines do not share the displacements of the other lines in the spectrum of the same star. For example, in double stars, with spectra in which the lines oscillate owing to the orbital motion of the stars, the lines of calcium do not share the oscillatory motion. For this reason they were originally known as "Stationary lines." It was shown by J. S. Plaskett that similar lines appear in the spectra of single stars, and that the velocity of the calcium source, so far from agreeing with the velocity of the star, reflects on the average simply the motion of the sun through the galaxy. It follows from these investigations that the source of the calcium is not in the stellar atmosphere, in agreement with the predictions of theory, and that it has in fact a separate existence independent of the particular star against which it is observed. This constitutes the discovery of the calcium cloud in space. By measuring the *intensities* of the "detached lines" of calcium (as they are now called), O. Struve was able to clinch the argument. He showed that the lines show up strongest in the spectra of the faintest stars. Now faintness is an indication of great distance, other things being equal. Thus Struve's result meant that calcium in space shows up most strongly when we used the most distant stars as a background against which to see it. Hence there must be more calcium between us and the distant stars than between us and the near stars, which shows that the cloud is more or less uniformly distributed through space.

We know of one calcium cloud much nearer home. This is the tenuous high-level envelope which surrounds the sun, visible at times of total eclipse as the chromosphere. Like the lower layers in the sun's atmosphere already discussed, this outer atmosphere is in radiative equilibrium, and it deflects downwards a considerable portion of the calcium light incident on it from below. In the course of doing this it experiences a reaction from the radiation, and it is this pressure (which only becomes appreciable at high levels) which supports the calcium against the downward pull of the sun's gravity. By balancing this radiation pressure against gravity, we are able to calculate the intrinsic opacity of the ionised calcium atom. Hence from Struve's observations on distant stars, by dividing the total observed calcium opacity by the opacity of the individual calcium atom we are able to calculate the number of calcium atoms between us and the distant stars, in other words the total number of calcium atoms in a column of the calcium

cloud. Dividing by the length of the column we obtain the density of the calcium cloud. Considerations of ionisation then show that once-ionised calcium atoms do not form the whole or even the greater part of the cloud. Each once-ionised calcium atom must be accompanied by about a million doubly ionised calcium atoms ; we observe only a tiny fraction of the calcium present, since doubly charged calcium atoms are transparent to the light we observe. Making allowance for this it is found that the density of the cosmic cloud is about 10^{-22} grams per cubic centimetre, or little more than one billion-billionth of the density of the earth's atmosphere.

How does the calcium get there ? It is probably being continually driven off from the atmospheres of the hotter stars by radiation pressure. Calcium atoms under the conditions of stellar atmospheres are extraordinarily sensitive to the pressure of light. We have seen that in the sun's atmosphere there is, to use Lord Asquith's phrase, a " precarious equipoise " between gravity and radiation pressure ; the two just balance. But substitute for the sun a hotter star, and forthwith light-pressure, much strengthened, wins the day. Calcium atoms must stream forth in an unceasing shower—an *upward* rain of calcium atoms. These congregate in space and build up the cosmic cloud.

Once again by the measurement, spectro-photometrically, of tiny quantities of an element giving rise to an absorption line, we have arrived at the discovery of a phenomenon of cosmic importance. We have come across something of compelling interest, a result in itself a reward to its investigators. But one of the symptoms of greatness in a science is its ability to use one major result to elucidate others. Astrophysics satisfies this test. We have seen how the study of stellar atmospheres will ultimately help to determine the masses of the stars they encase, and so throw light on general evolution. The same is true of the study of the calcium cloud in space. One of the major results of pure astronomy in the last few years is the discovery by Oort of the rotation of our galaxy as a whole. It is outside the scope of this essay to give an account of this discovery, but we may glance at its connection with the cosmic cloud of calcium. Struve, Gerasimovic, and J. S. Plaskett have reinvestigated the velocities indicated by the detached calcium lines, and find they share a velocity of general rotation just one-half the rotational velocity of the distant stars. This is just what we should expect if the calcium cloud is uniformly distributed in space ; for its *average* distance is then just one-half the distance of the distant stars. The whole galaxy swings majestically round its centre, and with it the all-pervading calcium cloud. Velocity-investigations of the

calcium cloud form therefore another weapon for attacking the problem of the structure of the universe.

I said earlier that atoms rarely soliloquise; not often, but sometimes. They are found soliloquising in the gaseous nebulæ. Again we know them by their spectra. It has been found by Bowen that lines of previously unknown origin in the spectra of the nebulæ can be attributed with certainty to atoms of oxygen and nitrogen in a high state of ionisation in a condition known as metastable. Normally, that is in terrestrial laboratories, they do not radiate; or rather, some other fate befalls them first. But in the vast rarities, the atomic silences, of the great nebulæ, they have long enough undisturbed periods in which to find utterance, and they ultimately give tongue, emitting the lines characteristic of nebulæ. Metastable atoms are taciturn by nature, but give them time, and they speak their message. The proof of the identification with nitrogen and oxygen rests on a calculation of the wave-lengths of the nebular lines from energy levels deduced by analysis of terrestrial spectra—a magnificent example of the interplay of pure physics and astrophysics. One of the remaining puzzles of observational astrophysics is thus solved. This has a significance both in pure physics and in astrophysics. Physicists had concluded that no new elements of small atomic number could exist, yet the presence of unidentified lines in gaseous mass was a challenge to their conclusion. Bowen's discovery confirms the physicist. The elements responsible for the nebular lines were old friends, oxygen and nitrogen, in quite familiar forms but in a different environment. In astrophysics also, this major discovery has had its applications. Bowen has shown how the observed stratification of the planetary nebulæ in light of different colours can be explained in terms of successive ionisations of oxygen, nitrogen, helium, and hydrogen. "Planetary nebulæ" is the name given to the curious soap-bubble-like nebulæ which surround certain stars, and their structure is now qualitatively clear. Most probably these spherical or spheroidal envelopes are supported by radiation pressure like the sun's chromosphere. They are also in rotation round an axis through the central star. The time is ripe for a mathematical attack on their structure, on the lines which have been successful for the sun's chromosphere and stellar atmospheres. A new field is opened for the application of the mathematical principles of radiative equilibrium.

Astrophysics is a science with the charm of youth. But it has the resources of the longer experience of astronomy, physics, and mathematics. It takes for its field the physical constitution of the heavenly bodies, and it seeks to answer one group of the eternal questionings of mankind. But it is a

giver as well as a taker. It is not a self-centred science. To astronomy it is gradually contributing new lines of attack on the problems of cosmical evolution, in ways we have been describing. Its method is to respond to every æthereal message the stars and nebulæ choose to send us. To physics it has contributed the discovery of helium, the knowledge of spectra terrestrially inaccessible, the proof of the correctness of the hypothesis of high-temperature ionisation, the absolute opacities of separate atoms and of ionised mixtures. To mathematics it has contributed the concept of radiative equilibrium; it has set for pure mathematicians new problems involving the solution of integral equations, now under investigation by leading mathematicians—just as a century ago in the hands of Fourier conductive equilibrium gave to mathematics new differential equations. It has still many more problems to set, more particularly problems involving non-steady states—radiative *lack of* equilibrium. To general science it has contributed justifications, if any were needed, of the hypothesis of the uniformity of nature. In the vastnesses of cosmic space, in the furnaces of high-temperature gaseous masses, it has proved that the laws of matter, or radiation, and of thermodynamics hold sway with uninfringed accuracy. And it has shown that the astronomically large can be studied by means of the submicroscopically small. As it is the youngest of the exact sciences, so it can look forward to a life longer than any, for its field is limited only by the extension of the cosmos itself.

CONCERNING THE STUDY OF PLANTS

An Inaugural Address by E. J. SALISBURY, D.Sc., F.L.S.
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The Quain Chair embodies a great historic tradition and has been occupied successively by three distinguished botanists, namely, Lindley and the two Olivers. The collective span of their careers represents a period of exceptional development in botany, as in other fields of science ; a period during which the content of the subject has vastly augmented and the centre of interest completely changed.

Lindley, the first occupant of the Quain Chair, has been described as an unconscious evolutionist, and it is no exaggeration to say that the modern classification of plants owes a great deal to the pioneer work of his versatile mind. But Lindley is more generally remembered as the man whose report secured Kew Gardens for the nation. It is gratifying to record that the happy relations between the Department of Botany at University College and the Royal Botanic Gardens, so auspiciously inaugurated, have continued ever since.

When Lindley was a young man botany had advanced but little from the purely descriptive phase. The plethora of new plants which were being brought in from all parts of the world resulted in a concentration of effort on the naming and classification of species, although the work of Stephen Hales in the early eighteenth century and of Nehemiah Grew in the late seventeenth had already directed the attention of botanists in this country to the physiological and anatomical aspects of the subject.

The elder Oliver was essentially a taxonomic specialist and of the first rank. Sir Joseph Hooker, in a letter to the distinguished American botanist Asa Gray, wrote : " The more I see of Oliver, the more I wonder at his marvellous knowledge. He has far the greatest knowledge of Phanerogams of any two botanists that ever lived. You cannot puzzle him." It is no reflection on Daniel Oliver's ability that he was once puzzled by a missing link from China that was sent by a native collector whose skill in grafting far surpassed his honesty.

The advent of my immediate predecessor marked the period of greatest change in botanical thought and teaching

when taxonomy became relegated to a less dominant rôle in the botanical curriculum. Of Prof. F. W. Oliver's work it may be said that of the many debts which British botany and British botanists owe him, his palæobotanical investigations on the seeds of the pteridosperms and his pioneer work on the dynamic relations of maritime vegetation, would alone suffice to ensure his adequate recognition by posterity. On my personal debt to his teaching and inspiration I will not here dwell.

Despite the fact that the content of botanical science has been vastly enlarged during the past hundred years, the popular conception of a botanist remains now much what it was then. To many people, even to-day, the apotheosis of botanical wisdom is to know the name of every plant "from the Cedar of Lebanon to the Hyssop that groweth out of the wall." There is a widespread impression that a botanist's chief qualification is an ability to name plants and more than a lurking suspicion that his task is completed when the label has been attached. This aspect of the subject is an important means to an end, but not an end in itself, though this scarcely justifies its neglect by many botanists of to-day.

When University College was founded the content of botany as already indicated was almost entirely descriptive, but if descriptive botany has ceased to occupy the predominant position which it held a century ago, it has remained an essential and important part of the subject. Criticisms are often levelled at the number of new species described, and indeed it is true that, to parody a well-worn cliché, there are systematists whose chief aim would appear to be to make two species grow where one grew before. Ignoring these smaller groupings, it is worth while to pause and consider what the taxonomist has accomplished. In Lindley's day the known number of flowering plants was about 90,000, whilst to-day it is probably well over 150,000, added to which the number of species of flowerless plants is far in excess of the flowering. It is no mean achievement that this vast assemblage of the most diverse types has been reduced to a system and order that is no mere artificial arrangement of pigeon-holes.

The specific names are, it is true, a shorthand method of designating an assemblage of individuals which resemble more or less closely a particular type specimen and description to which the name was first applied. But though based primarily on the organisation of the reproductive structures and on external features, these specific groups are composed of individuals which also approximate to one another in their anatomical structure, their chemical properties, and their physiological behaviour. This last is strikingly illustrated by the application in

recent years of the methods of serum diagnosis to the study of plants. A suitably prepared protein extract from a plant is mixed with the serum of an animal and a protein extract from another plant is added after a proper interval, when it is found that if the protein extracts be from closely related plants, a precipitate is formed, whilst if from unrelated species there is no precipitate. The results obtained by Carl Mez employing this method confirm in general the views as to relationships already arrived at on morphological grounds by taxonomists. Even if we accept in their entirety the criticisms of Barner and Burghard, the validity of the method for demonstrating relationship within a family is unchallenged and the results bear witness to the soundness of the morphological criteria of affinity.

Since such specific affinity implies so much more than mere external form, its importance to the student of pure science and for practical applications can scarcely be overemphasised. But though the larger groupings which the older botanists termed species usually suffice, where we are only concerned with the grosser distinctions of external form or internal structure, the specialised demands of modern requirements are continually emphasising the increasing importance of the recognition of the smaller aggregates. It is to such intensive study of species that we are indebted for the high-yielding strains of various crop-plants and the varieties immune from disease. It is subsequent to the recognition of such strains that the plant breeder endeavours by hybridisation to produce a race having all the virtues and as few as possible of the vices. To such endeavours to combine desirable characters in a single type we owe the high-yielding, disease-resistant wheats and sugar-canes of to-day.

But apart from the practical importance of such micro-species, some of the apparent contradictions, which the student of pure botany encounters, become resolved when the constituent strains of a species are distinguished just as the discovery of isotopes shed light on former obscurities in another realm of knowledge.

Sufficient has been said to emphasise the fundamental importance of taxonomy, an importance that is enhanced in proportion to its co-ordination with other aspects of the study of plants.

It is a natural consequence of the former dominance of taxonomy that the later developments also should at first have been descriptive in character. The early studies in anatomy were essentially an application of the methods of descriptive botany to the study of internal structure, and even such recent developments as the study of fossil plants and of cell structure were at first purely descriptive.

The stimulus to comparative studies which was a logical consequence of the acceptance of the doctrine of evolution, led naturally to attempts to correlate form and function. It is generally assumed that the modern period in botanical thought began with this changed point of view, but whilst this was undoubtedly the precursor, I venture to think that the real advance on modern lines was when investigators turned to study the mechanism of the plant's body. It was the discovery that the knowledge acquired in other fields of scientific thought could be applied successfully to the elucidation of botanical problems which so profoundly affected the study of plants. It marked the beginning of the introduction of quantitative thought into botanical philosophy to replace the purely qualitative point of view which had preceded it. It was entirely due to the fact that the methods of Mendel were quantitative that his results have served as the foundation on which the imposing edifice of the genetical knowledge of to-day has been built. Nearly half a century before Mendel's experiments John Goss in this country had in 1820 crossed different strains of peas and observed that the characters of the parents segregated out in the offspring of the hybrids, but his observations, like those of Koelreuter before him, were qualitative not quantitative. The fact that Mendel's results remained neglected till 1900 merely shows how recent is the recognition of the importance of quantitative data in biology.

From this time on the application of chemical and physical knowledge to the study of plants and the increasing use of quantitative methods have resulted in the development of a plant physiology that may already be said to have paid its debt to the sister sciences, and it is a lasting benefit to botany as a whole that plant-physiology has always remained an integral part of the botanical curriculum and did not become separated off as was the case with animal-physiology and zoology.

The studies of form and structure which engaged the attention of botanists at the end of the nineteenth century were inspired and stimulated by the prospect of reconstructing a genealogical tree of the vegetable kingdom, but whilst they have confirmed many already recognised relationships, the chief groups of plants remain as distinct as ever. The ferns and mosses, the mosses and algæ, still remain unconnected by any type living or fossil. The Charophyta are in splendid isolation and though the study of fossil plants, with which the Botanical Department of this college has been so intimately connected, has greatly enriched our conception of plant organisation, it has not revealed missing links, but indeed has brought to light other groups such as the Sphenophyllales, Pteridosperms,

Psilophytales, and Caytoniales, which but add branches or perhaps stems whose relation to the others is either remote or completely obscure. In the words of Dr. Scott, than whom no higher authority could be quoted, "We have indeed a wealth of accumulated facts, but from the point of view of the theory of descent they raise more questions than they solve."

The search for the missing link has proved elusive and has ceased to be a fertile stimulus for investigation. The presence of fully developed vascular plants of high complexity in the Lower Devonian rocks raises the question as to whether there are any adequate grounds for the assumption that these separate branches were ever joined to a common trunk. Evolution has perhaps been along many lines, parallel rather than divergent.

It is scarcely surprising that there followed a reaction from these abortive attempts to construct hypothetical genealogical trees, a reaction which manifested itself in the exclusive mechanistic attitude of the majority of those who were doing most towards the advancement of botanical knowledge.

The analytical method of the study of the constituent parts and processes of organisms has yielded valuable results in the past and it may be confidently anticipated will continue to be a useful method in the hands of the investigator of the future, but as Dr. E. S. Russell has rightly emphasised, the whole cannot be explained by a study of its parts in abstraction, although the parts may be explained by a study of the organism in its entirety. The mechanistic attitude has proved its value as a means of investigating mechanism, but only those whose mechanistic specialisation has atrophied their capacity for appreciating any point of view other than their own, will claim that this method of attack can do more than explain mechanism. The purely mechanistic outlook, whilst it has lost little if any value as an instrument of research, has probably expended most of its energy as a stimulus for new ideas.

The law of limiting factors applies with no less force to the extension of scientific knowledge than it does to the physiological processes of organisms, and there are indications that the progress of mechanistic analysis itself is hampered by the lack of a corresponding development in the study of the organism as an entity. The analytical processes fail to yield the results that might be anticipated because the progress of synthetic enquiry has become the limiting factor.

Eddington, dealing with the scientific reaction from microscopic analysis, has written: The artist desires to convey significances which cannot be told by microscopic detail and accordingly he resorts to impressionist painting. Strangely enough the physicist has found the same necessity, but his

impressionist scheme is just as much exact science and even more practical in its application than his microscopic scheme.

We may add that the biologist is similarly tending to revolt from the exclusively analytical concept of living matter, and there is increasing recognition that the unit is the organism, not the cell. In our meticulous analysis we have been so careful in "rubbing the gold-dust from the butterfly's wing," so careless of the pattern that we thus destroy.

During the latter part of the nineteenth century the development of the diverse branches of botanical study made rapid progress, but it was a development of independent parts rather than of a co-ordinated whole. This lack of integration threatened to bring about a severance of the constituent parts of Botany such as other subjects have suffered. It was the acute need of an integrating influence which accounted for the rapid development and success of ecology during the past twenty-five years.

Ecology has been defined as the study of the reciprocal relations between organisms and their environment, or in simpler language the study of the home life of the plant. But it has been rightly emphasised that ecology is a point of view rather than a subject. It is essentially a study of organisms as a whole which, whilst synthetic in attitude, finds place for all that the most rigid mechanist can contribute or the closest analysis reveal. This integrating influence is blending together the results of the physiologist, the anatomist, the morphologist, the palæobotanist, the geneticist, the taxonomist, and even the cytologist into one complete picture, and what this synthetic point of view has done and is doing for botanical science, ecology also bids fair to accomplish for the sister science of zoology.

The ecological approach is by no means a new one, for the foundation of ecology was laid in the physiognomic observations of Humboldt, if not actually earlier, but the modern content and application of ecology, with its rapid rise to prominence, is an outcome of the recognition of the need for a co-ordinating attitude of mind. One cannot therefore but regret that there are those who would unduly emphasise the sociological aspects of ecology and have indeed suggested the substitution of the designation phytosociology for that of plant ecology. To do this is to magnify the part at the expense of the whole. We can study the relations between plants and their natural habitats, considering them either as associated organisms or as individuals, but any undue emphasis on the one or the other is apt to obscure the fundamental aspect of both, their co-ordinating relation towards descriptive and experimental science.

Something of the complexity of the content and the multiplicity of the contacts of the modern study of plants is apparent if we attempt to obtain an insight into that most characteristic of British vegetation, the English woodland.

To understand the woodlands in Britain of to-day we must know more than a little of their past not merely during the historic period, but beyond. The accuracy of our information respecting these past conditions is in itself a striking tribute to the effective co-operation of various branches of knowledge. The investigations of plant remains, especially seeds and pollen at different levels in peat and other deposits, combined with the study of the evidences of prehistoric man and investigations by the zoologist and geologist, have together enabled us not merely to reconstruct the sequence of past epochs in the history of our country, but even to date their occurrence with a fair degree of probability.

Such studies indicate that during the period of maximum cold at the end of the last glacial epoch, and for a considerable period afterwards, there were no woodlands in Britain, but only shrubby vegetation of hardy and chiefly moorland species. Somewhere about 9000 years B.C., when palæolithic man the hunter was still in possession, the birch and willow spread in from the Continent. These were followed in turn by the pine between 8000 and 7000 B.C., the development of thickets of hazel and the arrival of the elm, the oak, and the alder some five centuries later. The cold climate had by now given place to a dry and warm period, which culminated about 5500 B.C., in the maximum forest development with birch and pine woods occupying the drier uplands, oak-hazel woods the moister and more lowland soils, whilst alder-willow thickets clothed the alluvial swamps. With the advent of the warm, moist Atlantic period, about 4000 B.C., we find the first evidence of neolithic man, the agriculturist who cleared forest for his crops and for fuel. It was probably his effective use of neolithic implements on the soft-wooded trees of the uplands that so hastened the replacement of the birch and pine by the moorland vegetation that has persisted to the present day. Beech and hornbeam perhaps did not appear till some 1500 or 1000 B.C., at the end of the Bronze Age, or early in the Iron Age, but certainly long before the coming of the Romans, to whom the introduction of these trees has so often been falsely attributed. This immigration of trees was accompanied by a similar immigration of herbaceous plants from the European mainland.

Thus despite the fundamental changes during the Ice Age, the dominant vegetation of these islands became once more part of the great deciduous forest-belt of Western Europe.

But an extensive study of the geographical distribution of our native trees shows that most of them are at or near their climatic limits in Britain, and it is probably for this reason that they exhibit a sensitiveness to local conditions which is not evinced in more congenial regions. Primarily owing to this the woodlands of Britain, as distinct from plantations, commonly present one kind of tree far outnumbering those others associated with it. To explain the predominance of Beeches on the slopes of the South Downs or the Cotswolds, the predominance of ashwoods on the limestone soils of the Mendips and the Peak district, the occurrence of alderwoods by the stream sides of Wales, or of the two types of oakwood each associated with particular soils, demands a knowledge of many botanical fields and not a few excursions into non-botanical realms.

We must first know how far the woodlands in question are due to natural causes or have been affected by the interference of man. Documentary evidence is sometimes available with respect both to the presence of woodland in early times and its composition, as where it is recorded that the Prior of Malton was fined in 1334 for cutting hawthorn and hazel in the Forest of Pickering for the purpose of kippering his herrings. But the best indication of the natural character of a woodland is afforded by internal evidence depending on the species of shrubs and herbs and their relative frequency. Here the aid of the zoologist can sometimes be invoked, since it has been found that certain animals, as, for example, the slugs *Limax tenellus* and *Limax cinereo-niger*, appear to inhabit ancient woodlands as distinct from old plantations.

That most trees, shrubs, and herbs will grow in a greater variety of soils and situations than they frequent in a feral state is witnessed by the wealth of our garden flora, and is sufficient indication of the importance of competition between species, the rigour of which is either removed or abated under conditions of cultivation.

Owing to competition and alterations of the physical environment, even natural woodlands are but seemingly stable and actually represent only a phase of varying permanence, so that we apprehend the plant communities of to-day, not as static assemblages of plants, but as phases in a dynamic succession brought about by the organisms themselves and by the secular action of climatic conditions. Evidence as to the nature and sequence of such change is to be sought in a comparative study of similar types of plant communities, in the chemical and physical constitution of the soil and in the indications of pre-existing vegetation of the same area. A study in which the student of the living and the fossil plant,

the soil chemist, the soil physicist, the archæologist, the historian, and even the student of place names may co-operate.

To elucidate the factors that determine the occurrence of any one phase in this sequence of woodland development it is necessary on the one hand to know the structure, life history, and physiological relations of the constituent species in the social assemblage of plants and animals, and on the other the nature of the physical environment which they occupy. By the application of knowledge acquired by the chemist and physicist it has been possible to show that the distribution of many plants is markedly affected by such features as the colloidal constitution of the soil and the hydrogen-ion concentration of the solution it contains, but above all by anything which affects the amount and freedom of movement of the soil water. Thus the botanist cannot be too intimately acquainted with the physical and chemical properties of the soil and constantly requires to utilise the data derived from their study.

Trees in common with all green plants are dependent for their growth upon an adequate supply of carbon-dioxide from the air, of water from the soil, and of radiant energy from the sun. The accurate measurement of light intensity in woodlands and its integration has been rendered possible by an adaptation and perfection of the photo-electric cell. The co-operation of the bacteriologist and protozoologist has led to the recognition that the supply of carbon-dioxide is determined by the numbers, proportions, and activity of the micro-organisms which decompose the organic debris on the forest floor and the expert forester now recognises that the preservation of the forest litter in a condition favourable to this teeming population is essential to the rapid production of good timber largely because it brings about an increase in the supply of this essential gas.

We have heard much in this country of recent years respecting schemes of afforestation, upon which the State can alone afford to embark owing to the low financial return which plantations yield and the large accumulated debt on costs of planting and rental. Ultimately no afforestation can be deemed satisfactory which does not admit of natural regeneration. The encouragement of naturally produced seedlings involves, however, an intimate knowledge of the life-history of the tree. We must know when and how often the tree fruits, the amount of seed produced, the enemies which prey upon the seeds and seedlings, and the conditions which encourage growth at every state. The mycologist has taught us that each of our forest trees is dependent for its proper nutrition upon the presence, in intimate association with its roots, of one or more fungi. But even if these beneficial fungi be present

and germination successfully accomplished the seedlings are subject to many fungal parasites and a prey to many herbivorous animals, so that unless the number of seedlings be great, this heavy toll may leave no survivors. A beech tree only fruits freely about once every seven years, and the chief enemies of its seedlings are the field mice, the numbers of which exhibit a marked periodicity also. Hence the successful regeneration of the beech is dependent on a high fertility, and the absence of coincidence between its mast years and field-mice epidemics.

As the tree seedling matures we find that its toleration for shade decreases and that it makes an increasing demand upon the water supply. The efficiency of a given rainfall will depend not merely on the water-retaining capacity of the soil, but on the resistance which the soil offers to the flow of water towards the tree's roots. There is also the resistance offered by the tree-trunk itself to the flow of water from root to leaves. These resistances depend chiefly on the structure of the soil and the detailed anatomy of the wood. The maintenance of the water supply depends initially on the rainfall, but of this a certain proportion evaporates from the foliage and branches, a proportion which varies with the kind of tree, the velocity of the wind, and the temperature and humidity of the air. Each kind of tree moreover requires to absorb a characteristic minimum amount of water for each unit of living substance that it produces, and this water-requirement is even found to vary with the different varieties of a species.

So that even to understand the seemingly simple relation of a tree and its water supply involves us in meteorology, in soil physics, in the detailed histology of the tree's axis, and in complex physiological problems such as the seasonal periodicity of absorption and the influence upon it of the ions dissolved in the water.

If it is a goal that has not as yet been reached, the endeavours of those who have travelled hopefully have often taken them along bye-paths to objectives of more economic value if less helpful to scientific progress. The large scale utilisation of forests for the lowering of the water-table and subsequent reclamation of swampy areas, or the planting of trees for the prevention of erosion are pertinent instances. But these examples serve to show how the environment itself is modified by the vegetation which it bears. So too the leaves which the tree sheds provide a constant manuring of the surface which retards the inevitable climatic leaching. In their decay both toxic and beneficial products result of which the relative proportions depend largely on the origin of the litter, the reaction of the soil, the proportion of calcium, and the presence or absence of earthworms.

How far forests modify the rainfall is a subject of some dis-

pute, but that forests influence the distribution of precipitation, if not its actual amount, there can be no reasonable doubt, whilst the importance of forests in regulating the flow of water to rivers cannot be overemphasised. Indeed it is perhaps no exaggeration to say that if this regulating action had been more generally recognised by those responsible for administration, the disastrous floods which the world has witnessed in recent years might have been avoided.

We can enquire into the mechanism of the bursting of the buds of the trees in spring or of the emergence of the herbaceous shoots. Precocious bursting of these buds can be induced soon after their formation in summer, or in late winter, though usually not in autumn. This can be effected by a remarkable diversity of treatments. Pricking, pinching, or even vigorous shaking of the buds is often efficacious. Treatment with hot water, dilute poisons, acetylene gas, or electric shock to the threshold of injury will produce the same result, whilst the confirmed smoker and the anti-prohibitionist will scarcely be surprised that the buds will also come out in response to the stimulus of tobacco smoke or the fumes of alcohol. It may be that even amongst this motley assemblage of stimuli there is a common factor in that all probably effect the permeability of the cell membrane, but without a study of the behaviour of the organism as a whole we are in danger of losing sight of the internal factors of the mechanism which are also variable. Under identical conditions one beech tree will come into leaf six weeks before its neighbour and its offspring share this peculiarity, whilst, as Prof. Diels has shown, definite types of periodicity with respect to the formation and duration of foliage may be characteristic of species having a common geographical origin.

We see the woodland in spring carpeted it may be with primroses and bluebells, or with wood anemones and lesser celandines, producing their leaves before the shrubs, and the leaves of the shrubs in turn expand before the canopy of green has clothed the tracery of branch and bough. We are impressed by the essential fitness of the organism for its place in nature whereby each stratum of vegetation is able to build up a store of food before the supply of sunlight which reaches it is diminished. A fitness which is further emphasised by the greater efficiency of the shade plants in weak light as compared with strong light, and of the plants of the wood-margin in bright light as contrasted with dull light. But when we seek to find the causes of such harmony between plants and their environment we are at once confronted with the question whether the plant is as it is because of its environment or is it merely where it is because of what it is? Further enquiry reveals that

there is a fitness which is inherent and a fitness which is imposed. Moreover, these two types of adaptation may present an identity of result that seems to belie the apparent diversity of their causation. Physiological experiments and breeding experiments are necessary for the resolution of this intricate machinery of causes and effects, but the partial revelation of the mechanism which they provide by study of its constituent parts must be supplemented by a study of the whole.

Many botanists, and amongst these some of the ablest, have during the past few decades abandoned the study of morphology and biology of plants because they regarded these as sterile lines of enquiry. Such an attitude can scarcely be wondered at in view of the purely qualitative character of most such investigations as compared with the quantitative results commonly furnished by experimental methods. The need of to-day is the application of the methods of exact science to the study of the structure and behaviour of plants; the substitution of quantitative for qualitative methods in those domains which are at present almost entirely descriptive.

To instance but a few examples, we know as yet almost nothing of a quantitative character regarding such fundamental phenomena as the reproductive capacity of plants, their rate of spread, the range in height of their vegetative organs or the variations in volume occupied by their root systems. How important such studies may prove for economic reasons, leaving on one side their value to pure science, is demonstrated by studies on the root system of Lucern, which have shown that strains of this important crop-plant with deeply penetrating roots survive in northern regions where the shallower rooted types succumb to the lifting action of the frost, and just recently Kokkonen in Finland has demonstrated that winter survival of rye is largely influenced by the extensibility of the roots.

Studies on the Scotch fir have elicited the fact that this species comprises a number of geographical races which differ as to the form of growth, the height they attain, and the root systems which they develop; and the failure of many plantations of this tree may well be due largely to the utilisation of unsuitable strains.

I have endeavoured by these brief glimpses from several points of view to convey the diversity of the content and relations of the modern study of plants. Many aspects have been ignored and I have intentionally refrained from emphasis upon the practical and economic applications.

One of the questions most frequently asked is, "What is the use of botany?"—not perhaps without more than a suspicion in the questioner's mind that it has little use whatever.

The intoleration of the so-called practical man for pure

science in general and botany in particular is due like most intoleration to mutual misunderstanding and lack of sympathy. In fairness to the considerable section of the intellectual community who have little acquaintance with the aims and methods of scientific pursuit, one must concede that they have an instinctive appreciation that the isolated fact is valueless. But the isolated fact remains valueless only so long as it remains isolated. Much of the scientific investigation that seems so fruitless to many is rationally so regarded because from their limited point of view the relation to other knowledge is not apprehended. For the man of affairs, who is commonly concerned with end results rather than their beginnings, it is difficult to realise that the apparently irrelevant facts acquired by patient research sooner or later serve as the bricks wherewith to build the superstructure of co-ordinated knowledge. The advocate of pure science could quote many instances where an apparently isolated fact, a stone which the builders rejected, has become the headstone of the corner. I make no defence of the half-ascertained facts, which serve only as the brickbats of controversy.

But if the mutual misunderstanding is to be dissipated, it must be recognised that the utility of science is not to be measured primarily in terms of its earning capacity or by the extent to which it ministers to our material comfort.

Investigators in the field of botany, or any other branch of science, may be likened to a number of workers digging for the buried pieces of a jigsaw puzzle. We are actuated by our faith that the pieces are parts of a single picture, not of many. Of those who dig, some break the pieces they recover so that their contributions do not fit and unnecessary trouble is involved in the recovery of the missing fragments. Some pieces are large and by themselves give a glimpse of the pattern of the whole. But every piece may help us to join up others, and, though many lie about neglected, sooner or later some one descries the relation between their apparently unrelated outlines and another part of nature's intricate pattern is revealed. If the game be worth the playing, no fragment of the puzzle can be neglected. The most severe critic of pure science is only too ready to accept the material benefits which the discoveries of botany have conferred, little realising that the very discoveries which he most prizes have often depended on the acquisition of knowledge which he would have condemned as trivial and useless. It is unnecessary here and before this audience to dwell on this theme, and the object of my simile has been rather to emphasise the inextricable relation of the obviously important and the apparently trivial. Moreover, just as each fact, each fragment of the

pattern, is necessary to the reconstruction of the whole, so too no aspect of botanical investigation can be neglected, nor its relation to other sciences ignored, without detriment both to itself and the progress of knowledge in general.

But botany cannot continue to fulfil its many-sided functions, still less to meet the increasing demands made upon it by forestry, agriculture, horticulture, and numerous important industries, unless it is provided with the necessary equipment to maintain in their integrity its complex ramifications. The need of the future is for botanical departments in the universities in intimate contact with the allied subjects of zoology, physiology, chemistry, physics, geology, and geography, equipped and endowed, with a personnel which shall include not merely specialists in all the more important branches, but biochemists, biophysicists, statisticians, and others whose function would be that of liaison officers maintaining and strengthening the links between subject and subject. The time has long since passed when the spirit of research itself needs to be actively fostered. In a world of increasing specialisation, the pursuit of such investigation is a necessary corollary. The function that the University is being more and more called upon to perform is one which can only be fulfilled by institutions where the diverse aspects of knowledge develop side by side. The University alone can maintain and intensify that unity of endeavour and synthesis of results without which the diversity of specialised enquiry, even within a single subject, is liable to degenerate into an accumulation of facts rather than an advancement of knowledge.

PROTOHYDRA, A VERY SIMPLE ANIMAL

(WITH ORIGINAL SKETCHES FROM LIFE)

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(A DESCRIPTION of a peculiarly interesting little brackish-water animal which is known in a few localities only and should repay any patient worker who can spare the time to study it in its natural surroundings throughout the year.)

Everyone knows Hydra, the little freshwater polyp which lives in ponds and streams. It is a tiny creature with a tube-like body, fixed at one end to a piece of stem or leaf and having at the other end a mouth surrounded by long retractile tentacles by means of which it catches living organisms. These tentacles are provided with elaborate stinging cells which are shot out and paralyse the prey. Having been successfully stunned, the food is then placed in the mouth by the tentacles themselves. There are green, brown, and grey Hydras, and one or more of these can usually be found in weedy ponds, each little animal being about half an inch or less when extended.

Protohydra (Fig. 1), the subject of this article, is a near relative of Hydra, but it is much less common. It was first discovered in 1868 by Greef and described by him in 1870. The only locality known then was the Oyster Parks at Ostend. These are enclosed areas where brackish water is allowed periodically to flow in and out. As far as I am aware it was not found elsewhere until about 1913, when Mr. R. J. Baker collected specimens up the River Tamar near Plymouth, in small brackish pools. These were given to Dr. Orton of the Plymouth Laboratory, who suggested searching up the other rivers in the neighbourhood. In 1919 it was found abundantly by myself in Chelson Meadow, the race-course of Plymouth. This is a large meadow drained by several streams and a main canal connected to the river by a hatchway; when this is open, water which may be quite salt is allowed to flow in (the River Laira at that point being tidal). Thus the water in Chelson Meadow varies greatly in salinity and may be almost fresh or very salt. In these streams such common brackish-water animals as the prawn *Palaemonetes*,

the amphipod *Gammarus*, many copepods, and the goby *Gobius microps* abound, and the bottom mud harbours many worms. By taking tow-nettings across the main stream with a fine-meshed net, samples of the minute life were collected, and the bottles containing this mixed with much fine mud were taken to the laboratory. The mud was placed in shallow glass dishes, covered with the water collected with it, and allowed to stand. After a day or two these were examined and Protohydra discovered. Since then it has been found frequently in the same place.

In 1916 Mr. Herbert Ashby found Protohydra in great abundance in the pools on the tidal marshes of the River Hamble near Southampton, so that this interesting little creature was discovered to be common in England in two distinct localities at about the same time. Writing about it in 1920, Prof. Sidney J. Hickson (*Quart. Journ. Micr. Sci.*) describes how it was found near Southampton in round pools not more than 4 or 5 in. deep, flushed with estuarine water only at spring tides, with Sea Lavender and Spartina flourishing between the pools. Films of diatoms over fine mud cover the bottom of the pools, and Protohydra lives in this diatom crust. Prof. Hickson states that it is *ca.* 3 mm. long, contracting to ½ mm., of a pale orange colour. On scraping off the crust at the bottom of the pools with a teaspoon and allowing it to settle in a bottle for twenty-four hours, Protohydra was found. Luther and Schneider (1921 and 1927) record it from Finland, and it has also been recorded from the Baltic and probably has been found in other countries, but as far as I know it has not been recorded from Britain except in the localities mentioned above. As this work was done more than ten years ago and apparently no more has been added since, it was thought that an account of Protohydra would not be out of place here, for it should occur in other brackish situations and invites researches of the utmost interest.

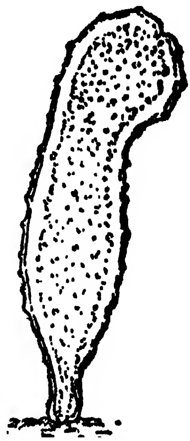

FIG. 1.—Protohydra.

Protohydra leuckarti, as this little creature is called, may reach a length of 4 or 5 mm. and may contract to as little as 0·4 mm. It is pale pinkish or yellowish in colour, shaped rather like a sausage, its body fixed at one end, its mouth situated at the other end. All over the body are small lumps which contain the stinging cells. There are no tentacles at all. Protohydra resembles a Hydra without any tentacles. If you look through the body it is apparent that it is made

up of two layers of cells surrounding the food cavity—an inner digestive layer and an outer protecting layer. In the outer layer are the stinging cells, especially plentiful near the mouth.

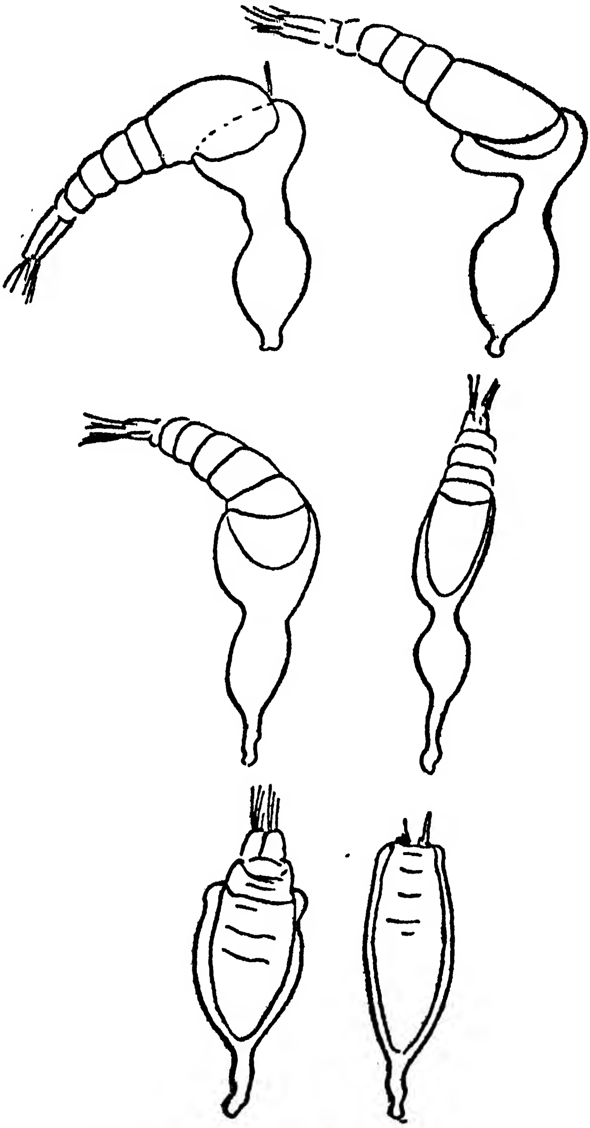

FIG. 2.—Protohydra eating a copepod.

The body is very elastic and can be quickly lengthened and contracted. It feeds on many things, but chiefly on s live animals, sometimes unicellular but more often such hi

organised forms as copepods and worms. Its food may be quite as large as the whole body or larger, or may be much longer. A worm many times the length of the body is sometimes taken but not always wholly eaten.

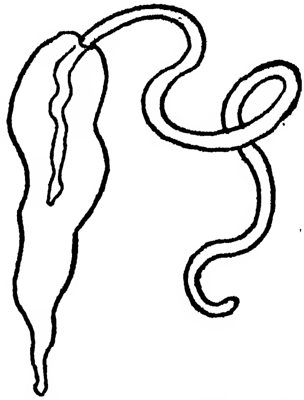

FIG. 3.—Protohydra eating a worm.

The process of feeding appears to be the following: A copepod or worm touches the body, which instantly reacts by sending out stinging cells probably containing some sort of poison, stunning the prey. The mouth then enlarges enormously, sometimes becoming as wide as the whole body width, and the food is gradually drawn into the food cavity by violent movements of the whole animal (Figs. 2–3). A copepod, swallowed head first for convenience, was completely engulfed in about a quarter of an hour and soon digested. After some time the indigestible remains were ejected by the mouth (Fig. 4*a*) and the body then contracted into a round mass in quiet content (Fig. 4*b*). The stinging cells, or nematocysts, are of immense value in feeding. There are two kinds, primary and secondary, much resembling those of Hydra. The secondary nematocysts are scattered irregularly all over the body, the primary nematocysts are arranged at regular intervals in the outer layer of the body wall, especially round the mouth (Fig. 5). At rest each one looks like a small bulb with a hair sticking out from the side. In this bulb is coiled a long thread with barbs surrounded by a fluid. On some living thing touching this outer hair the long thread is thrown out, barbs first, the barbs piercing the prey and probably carrying in some kind of poison, and the animal is stunned. Afterwards the swallowing seems to be fairly easy. The secondary nematocysts have no barbs and are of much simpler form with shorter threads.

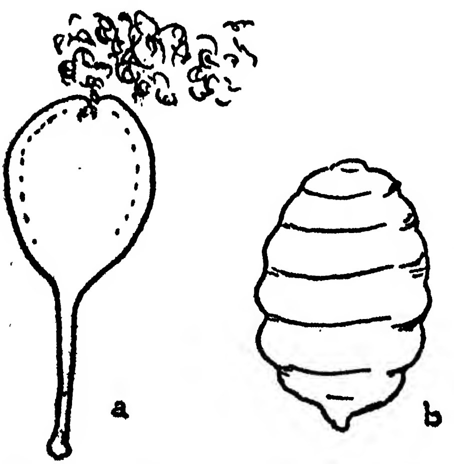

FIG. 4.—*a*. Protohydra ejecting indigestible remains. *b*. Contracted after feeding.

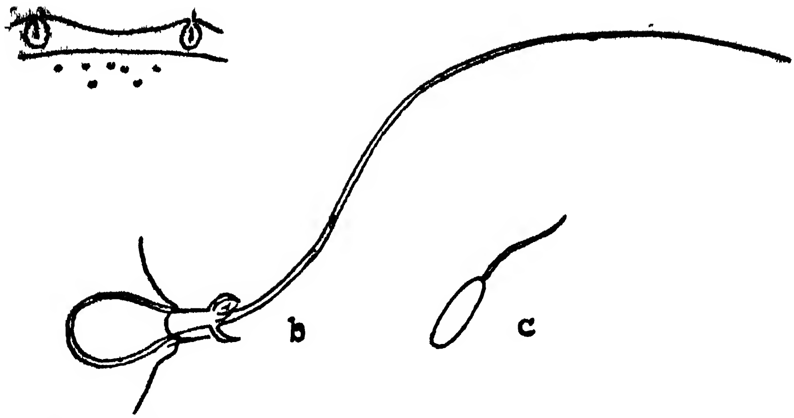

Fig. 5.—Stinging cells (nematocysts) of Protohydra *a* In outer layer. *b* Primary. *c* Secondary nematocysts.

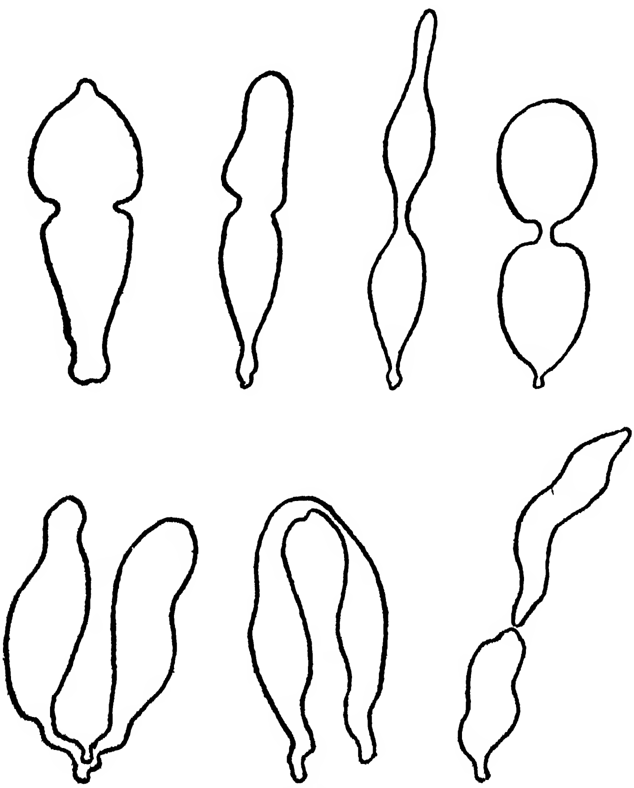

Fig. 6.—Protohydra dividing.

When Protohydra grows to a certain size it must reproduce. It is a curious and most interesting fact that only division of the body has been observed. No sexual reproduction has ever been seen, whereas in Hydra both sexual and asexual reproduction take place. In asexual reproduction of Hydra a bud appears at the side of the body and a new animal is formed, tentacles and all, exactly like the parent, which eventually constricts off, drops away, fixes itself and leads an independent existence. In Protohydra there is no lateral budding but, totally unlike Hydra, the division is transverse. A constriction appears in the body, generally about the middle, which becomes narrower until finally the connection breaks and two independent animals are seen (Fig. 6). This is the only form of reproduction known. It is quite possible that there is some kind of Medusa formed at certain times, but none is known. Here is a great opening for research. The animals have been kept in captivity for months and nothing more than this transverse division was seen. They have been collected at various times of year, still with no result. Prof. Hickson found none in July and August. My own observations show that it is plentiful in spring and early summer, but it was not collected during other months. Here is an opportunity for those living near brackish water to search for this interesting little beast and study it for a prolonged period, perhaps in one special pool or stream, throughout the year. It should be studied constantly both out-of-doors and indoors, and possibly some patient worker will be rewarded by finding out much that is new about Protohydra.

THE MECHANISM OF BIRD MIGRATION

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The subject of bird migration boasts an enormous literature. Scattered through this are numerous theories attempting to solve the riddle of the mechanism of migration. While they vary in value from the entirely impossible to the more rational, there seem to have been but few attempts to apply the experimental method to any of them. Watson and Lashley's experiments with terns concerned the homing instinct rather than the migratory. Pigeons have occasionally been the subject of investigation, but no more unsuitable bird could be employed. Were it not one of the most non-migratory and stay-at-home species it would not be the world's champion homer. The domestic product, at all events, has no migratory inclinations. The effects of the castrating of pigeons, for instance, could have no possible bearing on the correlation of sexhormones and migration.

Its possible mechanism is undoubtedly the most intriguing aspect of migration. It is one of the most complex examples of animal behaviour known among vertebrates, and involves so many aspects of biological science, that were an experimental analysis of at least some of its factors possible, the investigation might conceivably suggest general principles of considerable interest.

In the following brief account of an attempt to apply the experimental method to the subject, it should be pointed out that the investigation is as yet in its initial stages. Assumptions have had to be made, many of which will prove to be erroneous, but a blind alley that has been shown to be blind at least affords concrete information. When a majority of the blind alleys has been charted, the secret of the maze will emerge.

The first step was the formulating of a "working hypothesis." The general line of argument adopted was the following. Birds of the northern hemisphere migrate northwards in the spring in order to breed. They travel when their gonads are actively increasing. Once on the breeding-grounds they again become sedentary, and attend to nesting duties, while their

[1] The writer is greatly indebted to the Royal Society, the Directors of the Bache Fund, and the Elizabeth Thompson Fund, to Colonel John E. Thayer, and to Dr. John C. Phillips, for financing the later experiments on juncos and the crow experiment.

reproductive organs are functioning and at the peak of their cycle. Breeding over, there is at first a sudden regression of the organs, but this presently slows down, and the birds go south during the later phases of diminution. It might thus be inferred that the inclination to travel depended on (if it did not merely coincide with) certain stages in the growth and regression of the organs. The first point of experimental interest, therefore, was to discover some way of inducing recrudescence at a period other than the spring. The reproductive cycle in birds, if current opinion be correct, should be due to rising and falling temperatures. Both in scientific and popular literature temperature is assumed to be the stimulus. But this assumption appeared to the writer to be based on a misinterpretation of the facts, and it was therefore decided that elaborate aviaries equipped with heat-regulating apparatus would probably be useless. A more promising line of attack suggested itself in the fact that birds of the northern hemisphere go north when the days are lengthening and south when they are shortening. Gradually increasing daylight might, in fact, prove to be the stimulus inducing recrudescence of the gonads. The aviaries ultimately housing the experimental birds were therefore lit with electric light which could be turned on for increasing periods nightly in imitation of the spring days. Controls were similarly accommodated, but without artificial light. To assure the complete elimination of high temperatures, the aviaries were erected out-of-doors. Temperatures dropping to 30, 40, and even 50 degrees below zero F. at intervals could be relied upon during the months—November and December—when the experiment was in progress (− 52° F. was the actual lowest temperature recorded). The Junco, a common American migratory finch, was the bird used. In the winter of 1927–8, when the largest number of birds (120) was available, November turned out to be one of the coldest on record, and December the severest in thirty-two years. Yet before January 9, when the experiment terminated, the gonads of the experimental birds had attained their maximum (spring) condition, hundreds of times the bulk of the organs of their next-door neighbours, the controls, that had received no artificial lighting. Histologically, the experimental testes and ovaries were entirely normal.

A satisfactory method of inducing recrudescence in midwinter having been discovered, the next step was to liberate birds in batches whose gonads were at an approximately known stage of development and to record their behaviour.

Firstly, the controls. According to the hypothesis it should be possible to liberate these from November to February inclusive, without losing any birds. That is, they should not attempt to migrate, because their gonads would during that

period be in a resting phase—the winter minimum. Baited traps were constantly kept outside the aviaries for the retaking of returns, and all birds were ringed. During these months, eighty-four controls were released through three winters. Only four were not retaken, and it is as nearly certain as it can be without recovering the remains, that these were destroyed either by cats or the Great Northern Shrike.

According to the usual concept of migration, these birds, on gaining their freedom, should have departed for the south, since they were released in full winter conditions.

The experimentals did not behave so uniformly. They were liberated when samples picked at random showed that the gonads were in an intermediate stage of recrudescence, *i.e.* in the condition of spring birds on active migration. Under favourable weather conditions a large percentage usually departed. Those that failed to go were killed for examination. It was found that these had not reacted to the light treatment, and their gonads had remained small. Later on in the experiment the most advanced birds, those with fully or nearly fully developed gonads, swelled the number of sedentary individuals. In the extremely cold weather all birds stayed and were speedily recaptured.

Up to this point, the following conclusions may be reasonably based on the results. Increasing light induces the recrudescence of the reproductive organs regardless of low temperatures. Birds liberated with their gonads either at the maximum or minimum prove sedentary: those with organs in an intermediate stage, actively increasing, take advantage of their freedom, and depart except under certain weather conditions.

The significance of these facts is debatable on the following grounds. Departure and the condition of the gonads may be no more than coincidence, both possibly due to the daily increase of light. The former is by no means necessarily an outcome of the latter. However, this is a point that can in time be experimentally determined.

Birds of the northern hemisphere, such as the junco, go north in the spring, *i.e.* when their gonads are developing. Logically, it must be assumed that the experimental birds when they left us went northwards also, but we have no evidence of this, since not a single one of our departures has been retaken anywhere. This was only to be expected, for the junco is quite a small bird, and there are very few people in Western Canada sufficiently versed in bird-lore to detect the species either in season or out. For reasons outlined below, it was particularly desirable that this point should be ascertained.

Various modifications of these experiments were carried out concurrently with the above, but they do not concern us here.

The next phase planned was to repeat the experiments, but substituting for the junco some large species whose movements could be followed after liberation and individuals of which could be castrated. The last point, augmented with injections of testicular and ovarian extracts, promised to be of the greatest interest.

Various species suggested themselves, but were mostly subject to the objection that they were protected by law, and we could therefore not ask the populace in general to shoot and return them. It was finally decided that by far the most suitable bird was the common crow, a migrant with particularly regular habits. In Canada, as in most parts of the world, this bird is considered a pest—in Alberta it occurs in thousands—and it is shot or poisoned on a generous scale. It is, moreover, omnivorous, and likely to be able to fend for itself if turned out during the winter. At the same time, practically everyone in the West knows the bird when they see it, and as we have no crows in the country (except the odd straggler) from October to March inclusive, our birds would be sufficiently conspicuous to ensure their being noted. Opposed to these advantages was the general belief that it is impossible to catch an adult crow alive during the summer months.

Trapping was carried on during August. Numerous methods were tried, and most of them found to be failures. After four weeks of hard labour we had secured only 140 crows, less than a third of what we required. Our plans accordingly had to be seriously curtailed.

The birds were treated like their predecessors, the juncos. Lighting began on September 28, 1929. The experimentals when turned out on November 9 were very fit and strong on the wing, but there were but sixty-nine of them. The controls, which had received no lighting, and numbering only fourteen, were turned out at the same time. In the meanwhile, with the help of newspapers, radios, and the Department of Extension of the University of Alberta, the fact that the birds were to be liberated had been well advertised. Appeals were broadcast for everyone in the Province to keep an eye open for crows, and if possible to shoot and return them to the Department of Zoology at the University. Co-operation was willingly given in all quarters, even the Post Office joining in and giving precedence to all parcels marked "crow."

A full account of the experiment is appearing in the *Proceedings of the National Academy of Sciences*, and there is no need to give further details here except to summarise the results. To afford an understanding of them, it is necessary to make a brief comment on the geography of the Province. Although Edmonton is actually in the southern half of Alberta, it is nearly at the northern limit of the well-settled sections.

Fifty miles north-west of Edmonton the country gets relatively sparsely settled, and beyond that are large stretches of uninhabited muskeg and wilderness. Two hundred and fifty miles north-west of Edmonton is the Peace River country, again fairly well settled. It was originally planned to run the experiment about 100 miles south of Edmonton, in case the crows should actually go north-west (the supposed migrational direction of the birds in spring) and be lost to ken, but this involved various serious obstacles, and Edmonton was finally settled upon.

November 9 was a Saturday, and Monday, the 11th, a Dominion-wide holiday. The birds were surreptitiously turned out on the morning of the 9th, and the fact published in the evening papers and over the radios on Saturday night. No shooting is permitted on Sundays, so that the birds had forty-eight hours' freedom before the hunt commenced. Up to midday on Sunday practically all the birds seem to have spent their time moving up and down the river valley, in and out of the city limits, and then they appear to have sorted themselves out and those that did not return to the cages to have gone their various ways.

Returns came in rapidly at first, but soon diminished in frequency. Occasional sight records of crows were still being received (and one bird was shot) during March.

It is difficult to summarise the results on account of the many complicating factors involved which cannot be dealt with in a brief statement such as this, but many doubts could have been avoided had the experiment been run a couple of hundred miles to the south-west of Edmonton in the centre of well-settled country instead of at the northern border. Of the sixty-nine experimentals liberated on November 9, forty-one departed for good. The remainder were given more lighting and turned out again, under less favourable weather conditions, on November 24. Thirteen then departed, making a total of fifty-four birds, or 78 per cent. of the original sixty-nine. Of these fifty-four, twenty-five have been killed, leaving twenty-nine unaccounted for by the beginning of April. Six have been obtained south-east of Edmonton (the farthest in South Dakota, U.S.A.), and seven north and north-west (the farthest at Whitecourt, Alta, 100 miles N.W.) at distances greater than ten miles, the remainder all having been procured within ten miles of the town. Scores of sight records have been received. With one exception all reports of batches of crows exceeding two (rarely three) in number have come from the north-west, the majority from the Lesser Slave Lake country, up to 300 miles from Edmonton, from the latter half of November to January. All the evidence suggests that these must be our lost birds.

Of the fourteen controls liberated on November 9, six (turned out again on the 24th when all stayed) remained and eight departed. The liberation, in order to make use of Thanksgiving Day, was made at least two weeks earlier than was desirable, and it was anticipated that some of the controls would go, but they should have gone south. Six of the eight that left us have been killed, two on the outskirts of Edmonton and four more than ten miles to the south-east (the farthest about 200 miles). Two are unaccounted for. It seems unlikely that 75 per cent. (six out of eight) of the controls should have been recovered around Edmonton and to the south, and only 33 per cent. (eighteen out of fifty-four) of the experimentals if the latter had all gone the same way. Moreover, the birds that appeared in the Lesser Slave Lake country were first reported by the residents on the 18th (a flock of seven), nine days after our liberations and at least six weeks after the last crow had previously been observed there. That the birds were actually crows and not ravens is certain.

The results of this experiment, while quite unsatisfactory in many ways, are at least suggestive, and make it extremely desirable to repeat the attempt on a far larger scale and at a centre with at least 250 miles of well-settled territory in all directions.

In conclusion, there are some points of interest involved in the concept of migration that form the basis for these experiments to which allusion might be briefly made. When a bird leaves its northern breeding-grounds to winter somewhere farther south, it is safe to assume that it does not do so because it foresees the advent of a severe winter which entails a number of hardships and possibly death. The early migrants, those leaving northern latitudes in July and August, cannot even be aware of the existence of such a thing as a northern winter. They never experience it ; their parents have not experienced it ; countless generations before them have left the north in time to escape the experience. It can certainly not be foresight that takes these species south. One must assume, then, that the southward migration is the outcome of ancestral experience, that the tendency to migrate is now incorporated in the hereditary make-up of the individual, and that some releasing stimulus, occurring only at the appropriate season of the year, inaugurates the southward passage. If the shortening days are the environmental factor concerned, there must still be something additional, something within the bird that sets it going. It seems futile to say that the shortening days (or any other factor—cold, food shortage, etc.) send a bird south and to leave it at that. One must go farther. In the present case it is assumed that the reproductive organs,

responding to light conditions, at a given stage produce a hormone which affects the nervous mechanism of the bird in such a way as to arouse the instinct (or whatever you care to call it) that takes it south. The experiments themselves have been primarily concerned with increasing gonads and the opposite—the northern—passage. It has been assumed here also that the gonads produce a hormone, but in this case one that induces the bird to go northward. Far be it from me to define migration as a tropism, yet the double argument may be compared with the diametrically opposite tropistic tendencies exhibited by certain insects under various conditions, *e.g.* those that are negatively geotropic before feeding and positively geotropic after. In making these assumptions concerning the hormones of the reproductive organs it must be remembered that in the actual experiments all the ductless glands are sectioned and critically examined. That they may play a part either directly or by interaction has not been ignored. But a gonadal hormone, if a hormone be involved at all, is the more likely, and because the gonads can be removed by castration and extracts are obtainable, they offer the most promising starting-point for experimental attack.

Two other assumptions merit comment. One seems forced to assume that birds are in some way able to detect and respond to the earth's magnetic field. The evidence is extensive, and cannot be here discussed, but a single sample may be profitably cited. The adult American golden plover, *en route* for the Argentine, migrates south over the Atlantic, passing from Labrador to Brazil unless driven inland by storms. The young in the meantime, left behind by the parents, pass southwards from the Barren Lands where they have been hatched, across the prairies of Central Canada, drifting southwards towards Florida, whence they probably follow the Indies and the coast. They are unaware of the existence of the Argentine; they know naught of chart or compass; without experience and wholly untutored, they nevertheless terminate their journey on the wintering grounds of their species, the Argentine. It is not a case of mere wandering. They have the entire globe before them, yet they reach the correct area later than their parents, and having at first travelled quite independently some 2,000 miles to the west of the adult fly-line. Their migration covers an enormous front, extending right across the prairie Provinces. It is a drifting in the right direction rather than a passage over a well-defined track such as the southward flight of the adults. That they are "following the sun" (whatever that may mean), as suggested by various authors, seems improbable, since they travel mainly at night and, incidentally, cross the Equator on the road,

The marked wave of neo-Lamarckism at present affecting biological thought is, no doubt, an outcome of an ever-increasing knowledge of fossil forms and embryology. But all modern writers, adopting the Lamarckian viewpoint, stipulate æons of time in the establishment and inheritance of an acquired character. If this assumption is correct, it virtually eliminates the experimental method as a means of investigating the Lamarckian hypothesis. It would be impossible, in the lifetime of an individual, to induce a modification in a sufficient number of generations of a selected animal to demonstrate its ultimate inheritance. The endowment of an experiment of this nature so that it could be continued for a thousand years, as recently suggested by Pycraft, might be one solution, but there is an alternative. If a characteristic were selected for experimentation that could indubitably be shown to have been acquired, one could avoid the time factor, and begin the investigation half-way along, so to speak, by proving the selected characteristic to be inherited. I believe that it can be demonstrated beyond quibble that the migratory habit of birds must of necessity be an acquired characteristic. One of the *raisons d'être* of the crow experiment was an attempt to prove that it is inherited. If the spring migration could be invoked in the fall, and the birds induced to go northward in the face of the contradictory environmental conditions of autumn, it would surely demonstrate the inheritance of the habit.

Various critics of the hypothesis have assumed that if an internal secretion of the gonads induces migration in the junco or crow, then, since other birds presumably show a similar rhythm of the gonads and also develop interstitial tissue at similar periods, all birds should be migratory. But this is putting the cart before the horse. In the first place, interstitial tissue does not necessarily occur in the gonads of all birds. The little that is known of the histology of the gonads of gallinaceous species suggests that with them it is wanting and, incidentally, they are non-migratory. But the presence of interstitial tissue does not inevitably infer the migratory habit. Migration is the direct product of the environment and natural selection. The habit having once developed in any species, if it is to become inherent, occurring automatically at the appointed season, must become incorporated in the constitution of the bird. If the seasonal changes of the interstitial tissue ultimately afford the releasing stimulus, this supposes the pre-existence of these cells, not the habit. Various species possessing interstitial tissue may never have developed the migratory habit, and so could hardly be expected to migrate. If sexual behaviour depends, as is now generally held to be the case, on internal secretions of the

sex organs, it would be just as logical to expect the sexual behaviour of all animals to be identical as to expect all birds developing interstitial tissue of the gonads to be migratory.

On the same grounds one must explain degrees of the migratory habit. Some species, and certain strains within species, are still acquiring the habit. Annually, in northern latitudes, one witnesses the elimination of individuals that fail to go south. Natural selection is steadily doing the weeding that will finally establish a pure line of migrants. The annual survival of individuals of any species in Eastern Canada that winter north to any given latitude will produce a partially migratory race. But the same species, given entirely different conditions at the same latitudes in West Central Canada, may perish at those latitudes, and so will ultimately be represented in the west by a fully migratory race.

Another difficulty has already been discussed elsewhere. It involves such species as cross the Equator during migration. Such birds, having crossed the Equator, let us say in October, encounter days that are lengthening. Their gonads should enlarge under these conditions, and they should again turn north. But their organs fail to react. Early the following year they enlarge, and only then do the birds turn northwards. In its normal environment the breeding rhythm of the junco is annual, but experimentally it can be interrupted and re-adjusted almost at will. The rhythm is there, but annual periodicity has not become inherent, possibly because the migratory habit in the junco is of comparatively recent origin. But establish the annual periodicity, and the crossing of the Equator no longer forms a barrier. The gonads will fail to react to southern conditions. Storks, normally trans-equatorial migrants, do not breed in the southern hemisphere. Their gonads presumably, as with other species of similar habits, remain at their minimum during our winter—the southern summer. Yet storks kept in captivity for a number of years in the zoological gardens in Lima, Peru, ultimately bred there during the southern spring. The original periodicity evidently can be readjusted after a period of years in the south. The point is an interesting one from several angles, and would undoubtedly be worth investigation.

Equatorial birds undertaking an altitudinal migration into the mountains present yet another problem. Their environment is entirely different from that of birds of high latitudes. All the aspects of the case differ from the one that has been under consideration. There is no reason to suppose that this very special case invalidates the arguments outlined above, or that they should be expected to apply to it.

SOME RECENT ADVANCES IN THE CHEMISTRY OF IMMUNOLOGY

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When the animal body is invaded by micro-organisms it usually reacts to them in such a way that it is better able to kill them, or otherwise render them harmless. The actual process whereby the micro-organisms are killed is one which is at the present time rather outwith the bounds of chemical investigation. Accompanying, however, the actual destruction of the parasite in the body of the host, certain changes occur in the cells, and more particularly in the body fluids of the latter, which are comparatively simple in as far as they can be demonstrated with dead materials in the test-tube and which are, in all probability, dependent upon some physical-chemical mechanism. Thus, for example, during the course of recovery from an attack of typhoid fever not only do the bacilli disappear from the body, but the serum of the patient acquires properties which it previously did not possess. Even after considerable dilution it is able, when added to a suspension of typhoid bacilli, living or dead, to cause the agglutination, or clumping together, of these bacilli, and when it is added to certain suitably prepared extracts of the bacilli, a precipitate will be formed. These facts are commonly stated by saying that agglutinins and precipitins appear in the blood. Similar agglutinins and precipitins occur in the blood not only after an attack of the disease, but also after an injection into the body of killed typhoid bacilli, and it is further found that such an injection renders the treated animal, for some time at least, much less susceptible to the disease even though exposed to infection. It is thus clear that the development of agglutinins and precipitins in the blood serum is closely associated with the development of a real immunity to the disease, and therefore the investigation of the physical and chemical mechanisms of these phenomena of agglutination, precipitation, and allied processes is of the greatest theoretical interest and practical importance.

It has long been known that precipitins are formed not only

in response to the introduction into the body of micro-organisms or of their products, but also of other material of animal or vegetable origin. Substances, living or dead, animal or vegetable, in origin, which cause the formation of such precipitins or related compounds, have been called antigens and the hypothetical substances formed in the blood serum or other body fluids in response to their administration have been called antibodies. These two names, antigen and antibody, are somewhat difficult to define except in terms of each other, somewhat like the terms acid and base, in chemistry, an antigen being a substance which produces antibodies. The compounds, the antigenic properties of which have been most universally recognised, belong to the class of proteins, *e.g.* egg albumin, serum globulin, or haemoglobin. Certain proteins which are almost or entirely deficient in the aromatic amino-acids, as for example gelatine, are without antigenic properties. The presence therefore of aromatic groups, such as those in tyrosine, histidine, and tryptophan, are apparently quite indispensable in a protein antigen.

One of the most remarkable features of this phenomenon of a production of antibodies by antigens and their interaction is the very high degree of specificity which is observed. For example, the antibody formed by the injection of globulin from horse serum will not react with a solution of egg albumin or of the albumin from rabbit or guinea-pig, or other mammal apart from horse, or even the globulin of serum from any animal except the horse and possibly certain closely related species. The extraordinary specificity of these immunological reactions distinguishes them from the ordinary phenomenon of the precipitations of colloids and tremendously enhances their practical and theoretical significance.

The rôle played by non-protein compounds in the phenomena of immunity has long been a much-debated question. It is not possible here even to touch upon the many controversies which have raged over this matter, but we shall confine ourselves to certain recent work, the bulk of which has been carried on in America, and which has resulted in very real advances in our knowledge of these questions.

The work of Avery and Heidelberger and their collaborators on the immunological reactions of the pneumococcus stands out prominently as opening up a new field of investigation, which has already been subjected to very considerable exploration and yielded rich rewards. The various strains of pneumococci had previously shown themselves capable of being classified into three distinct immunological groups called Types I, II, and III, along with a fourth group called Type IV, in which the strains which did not fit into Groups I,

II, or III were classified. Antisera, that is to say sera containing antibodies, could be produced by the injection of Type I, II, or III pneumococcus which agglutinated that one, and only that one, particular type. It was found that the broth, free of bacilli, in which Type I had been grown was able, in very high dilutions, to give a precipitate with Type I antiserum, but not with Type II or Type III antisera. Extracts of the bacilli themselves could also be obtained which reacted specifically with the homologous antiserum, and similar solutions could be obtained from Type II and Type III pneumococci. These type specific soluble substances, which reacted in high dilution only with the homologous antiserum, were found to be comparatively stable substances capable of purification by the usual laboratory methods. The three substances when isolated proved to be of a carbohydrate nature, somewhat resembling certain vegetable gums. Type III and Type II substances like these gums possess distinct acidic properties, the former being a strong and the latter a weak acid. Both these carbohydrates were free, or practically free, of nitrogen. Type I substance on the other hand in its purest form contains about 5% of nitrogen and possesses amphoteric properties behaving both as a base and as an acid. These compounds formed precipitates with the homologous antisera in dilutions 1/2,000,000 to 1/6,000,000.

These carbohydrates, then, resemble the proteins in reacting with homologous antisera even in very great dilution. They differ sharply, however, from antigenic protein in one respect—they themselves are quite devoid as far as experiments have shown of the property of forming antibodies. They are, in fact, non-antigenic, but they have the power of reacting with the antibodies once they are formed. In order to produce these antibodies the whole bacterium must be used either dead or living.

The carbohydrates themselves are clearly not responsible for the immunological response to the pneumococcus. When the protein of the pneumococcus is investigated it is found that solutions of it too are able to form precipitates with anti-pneumococcus serum, but in this case the reaction is far less specific than with the carbohydrates. The protein from Type I pneumococcus reacts equally well with antiserum of any type, but the protein isolated from the bacterium although not type specific is by itself an antigen, when injected into animals it produces antibodies which react with it, but these antibodies produced by the injection of the protein do not act with the carbohydrates. To produce the carbohydrate antibody the whole bacilli containing both carbohydrate and protein in combination must be used.

In these carbohydrates we have, then, compounds which cannot be called antigens as they do not produce antibodies, but which react with antibodies once they are formed. Also the carbohydrates are unable to bring about the formation of these antibodies, yet they must be present if these type specific antibodies are to be produced. Such compounds have been called haptenes to distinguish them from proper antigens. Similar carbohydrate haptenes have been discovered in many other bacteria, in the Friedländer bacillus by Avery, Goebel, and Heidelberger, in the haemolytic streptococcus by Landsfield, in yeast by Mueller and Tomcsik, in the tubercle bacillus by Laidlaw and Dudley, and these do not exhaust the list. We shall only refer here briefly to work on the Friedländer bacillus.

Three well-characterised types of this bacillus have been named types A, B, and C respectively, and from each type, type specific soluble carbohydrate has been isolated which in high dilutions reacts with the homologous antisera. One very remarkable fact, however, was discovered. These type specific carbohydrates obviously belong to the same general class of acid gums as did the compounds isolated from the three types of pneumococci, and in its physical and chemical properties that from Type B Friedländer was not strikingly different from the Type II pneumococcus carbohydrate. It is nevertheless very surprising that although all these carbohydrates are strictly type specific within their own group, those from Type B Friedländer and Type II pneumococcus each reacts at high dilutions with the antiserum corresponding to the other. The reaction does not appear to be quite so sensitive or complete in the case of these cross reactions as with the proper homologous antiserum, but it is nevertheless very marked. The fact that the two carbohydrates resemble each other in the physical and chemical properties forcibly suggests that these physical and chemical properties play an important part in the mechanism bringing about precipitation.

The facts mentioned in the last paragraph suggested to Avery and Heidelberger that gums resembling the specific soluble substances, but of quite a different origin, might react in a similar way and give a precipitation with antisera. When samples of gum arabic were tested, certain of these were found to react distinctly with antisera for Type II or Type III pneumococci. Still more surprising, however, was the observation that when these samples of gum arabic were treated with hydrochloric acid in an endeavour to isolate the compound responsible for the reaction, the residue from the gum amounting to about 50% of the original material was found to possess an activity 100 to 150 times greater than that originally present

(Avery, Heidelberger, and Goebel). It appears that the treatment with acid must have altered the properties of the gum, presumably by partial hydrolysis, in such a way as to increase their resemblance to type specific carbohydrates. These observations further emphasise the fact that the specificity of the compounds is probably closely related to the physical and chemical properties and that the bacterial carbohydrates, although type specific, are not from the chemical point of view peculiar or exceptional compounds.

It is now quite clear that carbohydrate and protein play an important rôle in immunological specificity, and the question arises whether any other types of compounds also take part in such reactions. The other great group of natural compounds comprises the fats, or more generally the lipoids, the fat-like compounds of animal or vegetable origin soluble in acetone, ether or chloroform, the usual fat solvents. In this connection mention may be made briefly of the Forssman antibody and antigen. Rabbit serum does not normally haemolyse the red blood cells of the sheep, but if a small quantity of cat's tissue is injected into a rabbit, the rabbit's serum becomes haemolytic for sheep's red blood cells. This heterogenetic reaction as it is called has been extensively investigated by Forssman, Tanyguchi, and others. It has been found that the property of producing this heterogenetic or Forssman antibody haemolytic for sheep's red blood cells is possessed by the tissues or suitable extracts of the tissues of certain animals. These include the horse, cat, dog, guinea-pig, mouse, chicken, turtle, and some fish. The property is not possessed by the tissue of rabbit, pig, ox, goose, eel, rat or man, nor by the tissues of the sheep, although possessed by the red blood cells of the sheep. It has been found that from the tissues of the positive animals a lipoid can be extracted which itself reacts with the Forssman antibodies and that it is the presence of the lipoid in the red blood cells of the sheep that renders them susceptible to haemolysis in the presence of a Forssman antibody. But this lipoidal compound which we shall call the Forssman antigen is itself non-antigenic, it is in fact a haptene, capable of reacting with the antibody once this is produced, but itself not able to bring about its production. In one respect, however, it differs from the carbohydrate haptenes mentioned above. Neither pig serum nor the Forssman antigen when injected separately into a rabbit renders the serum of that animal haemolytic for sheep's red blood cells, but if the pig serum is first of all mixed with the lipoidal haptene and the mixture then injected, the Forssman antibody appears in the rabbit serum. In this case the union of lipoid with antigenic protein apparently takes place very easily on mere mixing and an anti-

genic complex is presumably produced. The serum of animals other than the pig may also be used, but it appears that pig serum works best. The work of Sachs and of Landsteiner on this subject is exceedingly interesting and is of particular significance in relation to more precise synthetic work by the latter author of which we shall now give a brief summary. It may be added here that although this haptene has been called lipoidal, this means only that it is found in extracts of tissues made with lipoidal solvents. There is some evidence, published by Landsteiner and Levene, that it may be largely carbohydrate in nature, resembling the bacterial haptenes, and that it is only associated with lipoids. This interesting point can only be decided by further experimental work.

The work which we are now to describe forms a most interesting extension and elucidation of the above observations, and sheds much light on the rôle of haptenes in the phenomena of immunity. It was shown many years ago by Aubermeyer and Pick that the products formed by the introduction of the iodo- or of the nitro- group into an antigenic protein resulted in the formation of a compound which was antigenic but specifically distinct. These modified proteins did not react with the antibodies produced by the original or by any other natural protein, but the antibodies produced by a nitro derivative of a particular protein reacted not only with this particular nitro-protein but also with other nitro-proteins. The introduction of the nitro-group had caused the disappearance of much of the original specificity, but at the same time had brought about the development of a new type of specificity. These observations have now been widely extended by Landsteiner, Van der Scheer, and their collaborators. These workers have developed a method for modification of proteins by the introduction of new groups which is capable of many modifications. It is well known that the hydroxyphenyl group in tyrosin and the iminazole group in hystidine are able to react with benzene diazonium chloride or derivatives of this substance with the formation of coloured azo-compounds. It is found that the property of coupling with derivatives of benzene diazonium chloride is retained by those amino acids even when they are in combination with other amino acids in the protein molecule. It is thus possible to form derivatives of proteins at will containing new groups of known structure. In this way a series of benzene azo-proteins has been prepared containing various substituents in the benzene nucleus in the ortho, meta, and para positions (Fig. 1). We shall first of all consider the cases where the substituent is methyl, nitro, chloro, bromo, or iodo, and not an acidic grouping. For each azo-protein a homologous anti-

serum may be prepared which forms a precipitate with this azo-protein. It appears, however, that the specificity is not absolute, in fact, azo-proteins having the benzene nucleus substituted in the para position react with antisera formed from other azo-proteins having the benzene nucleus substituted likewise in the para position though with a different substituent. On the other hand reaction is less marked with antisera formed to azo-proteins with a meta, and especially with an ortho substituent, even although that substituent is the same as that present in the para position in the original azo-protein. It appears, therefore, that the specificity is determined rather by the position of the substituent group than by its nature. This would appear to suggest that the shape of the molecule rather than its chemical properties, provided the latter were not too pronounced, was of the greatest importance. When azo-proteins are prepared which contain as a substituent

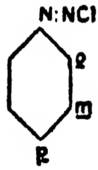

FIG. 1.—Benzene diazonium chloride (the letters *o*, *m* and *p* show the ortho, meta, and para positions in the benzene ring).

an acid grouping, *e.g.* CO_2H or ASO_3H_2, the specificity of antigen and antibody is more pronounced. These acid azo-proteins react very little, if at all, with antisera produced by the injection of the non-acid azo-proteins, and react much better to their own antiserum than to any other antiserum, even though this is produced from another acid azo-protein possessing a differently substituted benzene nucleus. In one respect this statement must be qualified. Two compounds can be prepared, each from different antigenic natural protein, by coupling with the same substituted benzene diazonium chloride. The antisera produced by these two proteins reacts each with both antigens so that the original protein, although necessary for the production of the antibodies, seems to play no essential part in determining their specificity. This specificity is almost or entirely determined by the benzene azo groups introduced. It is clear, therefore, that these benzene azo groups have a very close resemblance to the haptenes discussed above.

The extension of this work to groups of known constitution but of a more complex nature has yielded points of very great interest. The effect, for instance, of an optically active carbon atom has been investigated by Landsteiner and Van der Scheer. These authors have chosen phenylaminoacetic acid

(Fig. 2) (which contains an asymmetrical carbon atom), and in order to make it possible to condense this with proteins, have formed the *p*-aminobenzoyl derivative, namely phenyl (*p*-amino-benzoyl-amino) acetic acid (Fig. 3). This compound may be diazotised and coupled so as to yield an azo-protein. From the two forms *d* and *l* of the phenylaminoacetic acid there are obtained *d* and *l* azo-proteins and these yield the *d* and *l* antisera. It is found that a solution of the *d* azo-protein precipitates the *d* antiserum but the *l* very slightly, if at all, whilst the *l* azo-protein specifically precipitates the *l* antiserum. The specificity is thus complete, even although the two compounds differ from each other only in a position of hydrogen and carboxyl groups. It has been further found that

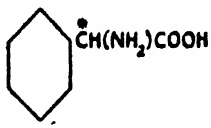

FIG. 2.—Phenyl amino-acetic acid.

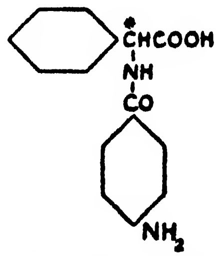

FIG. 3.—Phenyl (*p*-amino-benzoyl-amino) acetic acid.

(The asterisks in these and other Figs. denote the asymmetrical carbon atom.)

although *l* phenyl (*p*-amino-benzoyl-amino) acetic acid does not precipitate with the homologous *l* antiserum, it is nevertheless able when present to inhibit the formation of a precipitate between *l* antiserum and *l* azo-protein. The *d* acid inhibits not the *l* azo-protein reaction with *l* antiserum, but the *d* azo-protein reaction with *d* antiserum. It is quite clear, therefore, that the specificity is possessed by the crystalline compound quite apart from its combination with protein, and its resemblance to the haptenes as above described is further emphasised by the evidence that although it is not precipitated by the antiserum, it nevertheless combines with the antibodies as shown by its power to inhibit the formation of a precipitate.

These results have been confirmed and extended by Landsteiner and Van der Scheer by a series of experiments with azo-proteins prepared from diazotised amino-tartaranilic acid (Fig. 4). Three possible compounds can be made from *d*, *l*, and *meso*-tartaric acids respectively, as well as one from the inactive racemic tartaric acid, which, however, being a simple mixture of *d* and *l*, behaves as such and need not be further considered here. The *d*, *l*, and *meso* antisera are formed from

the *d*, *l*, and *meso* azo-proteins respectively, and each reacts specifically with its homologous antibody. It is interesting to note that the *meso* antibody and the *meso* antiserum react slightly more with the *d* azo-protein than does the *l*, although of course both reactions are slight. This fact, however, again

FIG. 4.—*p*-amino-tartaranilic acid.

emphasises the importance of the geometrical shape of the haptenes, since the *d* and *l* acids are more closely related chemically to each other than is either to the *meso* acid. Inhibition experiments with these compounds show specific inhibition by the homologous *p*-amino-tartaranilic acids, but it is interesting to note that complete inhibition was also effected when the homologous *p*-nitro-tartaranilic acid was used. This is, of course, not surprising, as in the antigenic azo-protein the group present is not the amino group but the azo group. It is more important, but not unexpected, to find that a certain amount of inhibition is also brought about by the homologous tartaric acid itself, though in this case the inhibition is only partial.

It is natural that Avery and his collaborators should have been interested to introduce carbohydrates into proteins in some manner such as that outlined above in the case of the tartaric acids. The technical difficulties in working with compounds such as glucose or other simple carbohydrates are formidable, but these appear to have been successfully overcome in the case of glucose and galactose by Avery and Goebel. These authors have prepared *p*-aminophenyl-*β*-glucoside (Fig. 5) and *p*-aminophenyl-*β*-galactoside (Fig. 6), and by the

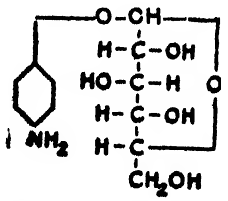

FIG. 5.—*p*-aminophenyl-*β*-glucoside.

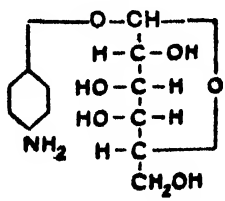

FIG. 6.—*p*-aminophenyl-*β*-galactoside.

usual process of diazotisation and coupling have obtained what may be called gluco- and galacto-azo-proteins. To these they have obtained the corresponding gluco- and galacto-antisera. These react specifically with the gluco- and galacto-azo-proteins and the specificity is independent of the original

native protein used. Thus the antiserum formed by the use of the gluco-azo-protein prepared from horse serum globulin reacts with the gluco-azo-protein prepared from egg albumin. Inhibition experiments show specific inhibition by the homologous *p*-aminophenyl-glucoside or galactoside, but in this case glucose and galactose themselves do not appear to possess inhibitory action.

It will be seen that this work by Avery and Goebel is in general agreement with, and forms a confirmation of, the work by Landsteiner and his collaborators, but it is of particular interest in view of the known existence of carbohydrates in nature which act as haptenes and play an important rôle in natural immunological reactions.

The work which we have described above relates almost entirely to the precipitin reactions. When we are dealing with whole bacteria the agglutinin reaction is much more important. One would naturally expect a close relationship to exist between the agglutination of bacteria and the precipitation of colloid material derived from them. Amongst other evidence of this relationship may be mentioned the work of Jones, who has shown that collodion particles which have been first treated with a solution of antigenic protein so that they are covered with a surface layer of this protein are then agglutinated when subjected to the action of the serum of an animal which has been immunised by a previous injection of the protein. The antiserum, of course, contains precipitins for the protein, and the decreased tendency of the protein to remain dispersed in water results in the ready agglutination of the protein-coated particles. The bearing of this work on the specific agglutination of the various types of, for example, pneumococci is demonstrated by the numerous observations which show that if the pneumococci of a particular type are cultivated in such a way as to lose their capsule of carbohydrate, then they are also found to have lost their type specificity.

The whole problem of the physical mechanism of agglutination is a difficult one and cannot be discussed fully here. Reference may, however, be made to the work of Northrop and de Kruif, who have shown that if bacteria are treated with a solution of inorganic salts, the electric charge is decreased so that contact of the particles becomes possible, but that if a certain minimum concentration of salt be exceeded, the bacteria no longer stick together, so that agglutination does not take place. If, however, agglutinating antiserum is present, this decrease in cohesion as the result of the presence of considerable concentration of inorganic salt no longer occurs, so that agglutination takes places whenever the concentration of salt is enough to cause a sufficient decrease of the electric charge.

The failure of colloidal particles to agglutinate in presence of high concentrations of salt, even although the electric charge is zero, is not limited to bacteria, as a similar phenomenon is exhibited by certain of the suspensions of lipoidal material used as Wassermann antigens (Kermack and Spragg, 1929).

It would appear from these and other observations that the liability to agglutinate in presence of antiserum will depend upon a decrease in the tendency of certain colloids to disperse in water, in other words upon the decrease in the lyophilic nature of certain colloids existing at the surface of the bacteria. This decrease in the lyophilic nature of these colloids might conceivably be brought about in various ways, for example by the neutralisation of particular lyophilic groups or by the increase in the size of the colloidal micellae. The facts given above would appear to demonstrate a very important rôle which the geometrical configurations of the groups protruding from the protein molecule appear to play in the determination of the specificity of the precipitin reaction. In the case of the simple non-acidic azo-proteins derived from aniline the position, but not apparently the chemical nature, of the group is of importance. In other cases the mere interchange of a hydrogen and hydroxy group, or the interchange of a hydrogen and carboxyl group, as in the case of the more complicated azo-protein mentioned above, is sufficient to alter completely the specificity of the antigen. The most natural assumption would seem to be that the antibodies, whatever they are, are able to fit on to the protruding groups. One might imagine that the molecules of antigen are carried throughout the body and become adsorbed on particular surfaces where these protruding groups would become surrounded by a molecular structure which fits them with a certain degree of precision. These antibodies are then presumably washed off into the blood stream, but when the antiserum is mixed with the antigenic material, the groups once more become covered with the structures which had been formed in the body so that their lyophilic nature is decreased partly as the result of the covering up and neutralisation of lyophilic groups (as carboxyl or hydroxyl) and partly owing to the increase in the colloidal unit. Such speculations are, of course, at best only elaborations of the general theory of antigen-antibody reaction suggested many years ago by Ehrlich, but the molecular groupings which he called haptophores can in certain instances, at least, be given a definite chemical configuration. It is a signal indication of his insight and genius that the remarkable chemical work which is now being done should fit in so well with the general ideas which he enunciated. There would seem to be no real incongruity between the explanations offered by the physical chemical

school of Arrhenius and Bordet on the one hand, and by Ehrlich and his disciples on the other. It would rather appear that it is only by the close co-operation of bacteriologists both with the physical chemist and with the organic chemist that these very difficult problems can be elucidated, and it is to be hoped that the great promise of theoretical development and practical achievement which the work described above appears to give will not fail to be realised.

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POPULAR SCIENCE

PROGRESS IN COLOUR PHOTOGRAPHY

By Captain OWEN WHEELER, F.R.P.S.

Last year was in the nature of a set-back to the advancement of photography in natural colours. With much trumpet-blowing an effort was made to exploit a revival of an old method by which all the three negatives required for colour-printing are produced at one exposure on a " tri-pack " consisting of three specially sensitised films packed together. By employing very rapid emulsions for the middle and rear films, and very transparent ones for the middle and front films, and by careful adjustment of the sensitiveness of all three to equalise the ratio of exposure, it is possible in this way to obtain very fair sets of negatives for three-colour printing. But tri-pack films must be used with discrimination, and to place them in the hands of any possessor of a pocket camera, and to offer a service of development and printing in colours at extremely low rates, was a rash experiment, and a disastrous failure resulted. Dazzled by glowing advertisements, the public eagerly bought " Colorsnap " films, exposed them, and sent them through their dealers to headquarters for treatment. As a rule, weeks of waiting followed, with an eventual intimation that the majority of the films had been so wrongly exposed that it was impossible to print from them. When prints were supplied, they were usually of poor quality, and sometimes mere travesties of the originals. After a brief and inglorious career the company responsible went into liquidation, and it will probably be some considerable time before a fresh attempt is made in this direction.

The tri-pack idea is not unsound, and another organisation has applied it with success to the production of negatives suitable for three-colour printing. But in this case the printing methods adopted, to which further reference will be made presently, have been of marked efficiency, and no attempt has been made to achieve popularity by impracticably low prices. Apart from the tri-pack system, and the ordinary plan of making three separate exposures behind the red, green,

and blue-violet filters, there are several ways in which sets of three-colour negatives can be produced, the simplest being the employment of one-exposure cameras, of which there are at least two recent types deserving attention. One is the René-Gilbert, which is having, it is said, a favourable reception in America, the other the Trichrome Photo and Films Co.'s camera, now being manufactured in North Acton. With these instruments excellent results can be obtained with very quick exposures—under favourable conditions instantaneously—but the extreme precision with which the mirror or prism systems on which they are dependent for splitting into three the image transmitted by a single lens renders them costly, and in any but small sizes they are necessarily somewhat cumbersome. Still, for those who can afford the outlay, the one-exposure camera is an attractive proposition, solving, as it does, the problem of securing three negatives behind three different colour filters both expeditiously and with due regard to the "ratio of exposure," which it is not easy to adjust correctly where fractions of a second are concerned.

Other methods of making the three exposures, not instantaneously, but sufficiently quickly for portraiture in a good light, are those in which a repeating slide with colour-filters the same size as the plate, or a rotating colour filter-holder after the fashion of the old "wheel" diaphragm, is employed. In the new Ross "Rotator," in the design of which the writer was concerned, the filters themselves take the place of the ordinary "stops," and, as they are sized to represent the ratio of exposure for the different colours, the same exposure is given in each case, a manifestly convenient arrangement. The "Rotator" is used in conjunction with either a simple repeating slide for successive exposures on three plates or in one long plate, or with one of the panchromatic film-packs now available.

It may be said that for time exposures the existing facilities for making sets of negatives for three-colour printing are fairly adequate, but the ideal, namely, the production at a single instantaneous exposure of a single negative from which colour-prints can be made by either a three-colour or other method is still to seek. There are two or three lines of approach along which some progress has been made, the most promising, perhaps, being the Finlay system, which is already in successful commercial operation, and with which results very striking in point of fidelity of reproduction are being obtained. The basic idea of the process is to make in the first instance a negative in which all the colours of the original are reproduced monochromatically with mechanical accuracy by exposing through a geometrically patterned tricolour screen similar to that

employed in the Paget screen-plate. The "colours" are then separated by means of two-colour screens of the same geometrical pattern, but with one colour stopped out. In this way, by careful registration, it is possible to produce sets of negatives fulfilling the conditions necessary for three-colour printing by either mechanical or chemical methods.

Another plan is to photograph the subject on an ordinary screen-plate such as the Autochrome or Agfa, and, from the colour transparency thus obtained, to make, by contact or enlargement, a set of three-colour negatives on panchromatic plates exposed to artificial light filtered through successive red, green, and blue screens. It is a roundabout method, and has the disadvantage of being both slow and uncertain, but it is practicable, and, if followed up with care and patience, might prove in many cases very serviceable.

A third system of direct colour photography, to which allusion may be conveniently made at this point, although it belongs essentially to the region of printing, is the Martinez process, by which it is alleged that colour prints can be made from ordinary negatives taken at a single exposure through, in the case of a daylight exposure, a single light filter capable of giving a suitable monochrome reproduction of all colours. A very special paper with a sensitive coating incorporating a urea derivative—alloxan—in combination with iron and other salts, is exposed under the negative, and it is claimed that, with a suitable developer, the colours of the original are reproduced—a truly magical performance. Some doubt exists concerning the practicability of the process, which in any case has the drawback that the colour reproduction effected in the first instance is only partial, the missing hues being "created" by after-treatment. The process was described by Dr. Martinez himself in the Colour Photography Supplement of the *British Journal of Photography* for April 4, 1930, but hardly in a manner calculated to carry conviction to workers with knowledge and experience.

Turning now to printing by three-colour non-mechanical methods, it is a little difficult to sort these out on any logical system of classification, since some of the most hopeful of them are combinations of entirely distinct processes. Progress is being made chiefly in those in which blues, reds, and yellows —the complementaries of the filter colours—are obtained (1) by toning, (2) by the employment of coloured "carbon tissues," and (3) by imbibition from dye-stained reliefs produced by exposing to light behind a negative gelatine films sensitised with an alkaline bichromate solution, and developing with warm water. In the first category a process formerly exploited by Colour Photographs, Ltd., with marked success,

but latterly, it is understood, abandoned by them, deserves to be mentioned as embodying some novel features. In this the prints were made on a material called cellophane, which is largely used commercially for wrapping purposes. It is a preparation of cellulose, obtainable in a tissue-like thinness, transparent, and, unlike celluloid, permeable by water. The prints were toned, respectively, red, blue, and yellow, and, by putting them together before cementing, the balance of colour could be judged and desired alterations made in the depth of tint. In this way some beautiful results have been attained, a special point being the goodness of the reds, which previously it had been impossible to procure satisfactorily by any known toning process. In a demonstration of the method under allusion before the Royal Photographic Society it was stated that the agent employed was a murexide, but no further details were given.

Turning to the second category, trichrome carbon has been largely supplanted by trichrome Carbro which, at the moment, is probably the favourite among British non-mechanical colour-printing methods, especially among amateurs content with a very limited number of copies. The process has been carefully standardised by the Autotype Co. and is capable of excellent results, its efficiency being emphasised by the fact that probably a better reproduction of the Ilford Colour Chart—a particularly severe test, since neutral tints as well as colours are included—has been made by it than by any other known method of chemical colour printing on paper. As in monochrome Carbro, prints are taken from the three negatives on bromide paper, and from the bromides carbros are made by chemical action, and transferred to waxed celluloids. Development in warm water follows, and the yellow, red, and blue films are successively imposed on transfer paper as in trichrome carbon. As the prints are laterally reversed an additional transfer can be made, if desired, by the employment of a soluble temporary support, but, of course, in the case of most still life objects and portraits this is unnecessary. The drawbacks to trichrome Carbro are the time taken to produce duplicates and the absence of any control of depth of colour. On the other hand, given good negatives, and the right kind of bromide prints from them, there is a very good chance of securing a right balance of colours, since the pigments of the tissues have been most carefully selected, and approximate closely to theoretical perfection.

In the relief processes the employment of dyes has led to development along the lines of both mordanting and imbibition, the colours in the former case being transferred to the final paper but with the help of a mordant, and in the latter by

simple absorption. There has, on the whole, been a greater advance in imbibition methods than in mordanting ones, and it is the writer's conviction that, at any rate for amateur purposes, this lead will be fully maintained. His own experimental work, which has been concentrated and prolonged, has demonstrated the possibility of formulating an imbibition process of great simplicity and considerable effectiveness by a selective combination of the methods of previous workers, coupled with a few additions, tending more particularly to increased economy and speed of production. This summary may, he ventures to think, be fitly concluded by a brief sketch of this process, which has not yet been commercialised, and of which at the time of writing no detailed description has appeared. It is called Collochrome, and resembles in some respects the writer's earlier process, Dyebro, which was put forward two years ago, and is now being exploited by the Autotype Company. There is, however, the essential difference that in Collochrome the reliefs are produced by light action, and not by chemical action as in Dyebro, which, as its name implies, is dye-printing from bromides, the chemistry being identical with that applied to the method of carbon printing from bromides known as Carbro. Moreover, in Collochrome the use of British dyes, which are much cheaper than the German dyes hitherto commonly used in photography, has been rendered possible by the discovery of a satisfactory treatment for the prevention of "bleeding," to which British reds and yellows are particularly subject.

The following is an outline of the operations in Collochrome. From the negative taken with the red filter a bromide print is made and toned blue with a mixture of ferric ammonia citrate, potassium ferricyanide, glacial acetic acid, and water. This is an old method which provides a very good blue foundation print if properly carried out, but is not suited to large pictures unless special precautions are taken to prevent uneven expansion of the paper base. From the green- and blue-sensation negatives prints are taken on a special very lightly coloured "tissue," sensitised with bichromate of potash, and exposed, as in the carbon process, with the guidance of an actinometer. The exposed tissues are then mounted on celluloid supports, stripped, and the images on the celluloids developed in warm water, and hardened in a solution of formalin. When dry the reliefs, or, as they have now become, print-sheets, are stained up in their respective red and yellow dye-solutions, and treated, before being printed from, with the anti-bleeding compound mentioned above. Printing is as usual in the imbibition processes, the washed print-sheet and the soaked support—in this case the blue-toned bromide—

being brought into contact under water, registration effected, and print-sheet and support squeegeed together and kept between damp cloth or blotting-paper till sufficient dye has been transferred. When, by lifting up a corner of the paper print, it has been ascertained that imbibition has proceeded far enough, paper print and celluloid print-sheet are stripped apart, and, if another dye impression is needed, the paper-print is dried and resoaked, and the print-sheet stained up afresh.

This process is not only extremely economical as regards cost of materials, but also, as chemical printing processes go, very expeditious. It is possible, if a good set of negatives is available, and blue-toned bromides have been made from that taken behind the red filter, to obtain a proof colour print in about three hours, and subsequently copies can be multiplied at the rate of three or four an hour. The cost of each copy, apart from time, can be reckoned in pence.

With these possibilities actually in view colour photography may surely be said to be making progress, and, although on none of the lines indicated above has finality been reached, there is a hopeful prospect that at a comparatively early date workers in colour will no longer be a small and rather timid band, but a numerous, self-assertive, and very competent community.

ESSAY

CAN FISH HEAR? (H. O. Bull, B.Sc., Biologist at the Dove Marine Laboratory, Cullercoats, Northumberland).

MOST keen anglers, if asked this question, will at once profess a positive belief—and yet, judging from the results, it has been found an extremely hard task by biologists to bring forward definite experimental evidence in support of it.

It may be that many fishes which periodically undergo an inshore and offshore migration, such as the plaice, salmon, etc., are sensitive to sounds emanating from land a considerable distance away. We cannot say. Nevertheless, it is possible that such fishes, if able to perceive these sounds and their direction, might be able to profit in no small measure by this perception. A directive influence of this nature, being, as it is, independent of seasonal fluctuations, appears more feasible than others which have been suggested, such as thermal, chemotactic, olfactory, for these are quickly obliterated by a stiff onshore breeze. On the other hand, it is conceivable that fishes habitually frequenting the littoral zone may have little use for auditory discrimination. Here, then, is one of many functional reasons which could be brought forward suggesting that some fishes may make good and profitable use of a sense of hearing which would be useless to others. The uncertainty prevailing upon this vexed question may, indeed, be due to this very real possibility; some fishes hear—some do not.

Another possible reason for the widely divergent opinions is that the methods employed have not been just the right ones to settle the matter. The methods group themselves into several well-defined types.

Primitive in conception, but common alike to ancients and moderns, is the anecdotal fisherman's story. In these the actual truth of the observation is not in question. Such an observation as that of Aristotle, that fish must hear, as they fly from the noise made by the splashing of oars, is fairly representative of this mode of inferring the auditory capacity of fishes. There is a failure to appreciate the great, or probably greater, importance of other stimuli present on the occasion.

Anatomical studies alone of the structures concerned in audition, and especially of their central nervous connections, have formed the basis for many denials that fish hear, or

do not hear. Undoubtedly, there is a definite correlation between the structural arrangement, and degree of development of the appropriate receiving centres in the brains of all vertebrates, and the degree of development of the peripheral sense organs with which they are associated. On the basis of comparative anatomy fairly reliable conclusions may be reached by this means, but such inferences demand support from physiological experiments. The ear in fishes, as in all vertebrates, is the organ of equilibrium as well as of hearing, and is almost certainly of more importance to them on this account, from the nature of the medium in which they live and from their mode of progression. Further, fishes are in possession of a complex cutaneous system of sense organs, totally absent in man—the so-called lateral line system. Formerly supposed to serve solely for the secretion of mucous, with which the skin is kept lubricated, it is now thought that this is specialised for the perception of slow vibratory movements. Further proof is, in my opinion, desirable, but if so it would seem that fishes are doubly provided with mechanisms for the reception of vibratory sensations.

Perhaps the most widely used means of ascertaining the auditory capacity of fishes is that of the *subjective* experiment. In these the fish may be free, restrained, or even blinded; the noise which it is desired to investigate is produced, and the response of the fish, or certain organs of the fish, is then noted by direct observation. The experiment may be repeated after actual removal of the auditory nerves, or the nerves of the acoustico-lateral system, and the response again noted. It is to me almost incredible that this procedure is seriously considered as a decisive test for sound perception. It is no more true to say of fishes under these conditions that they do not hear if they betray no bodily response, than it is to say this of a man sitting at his desk and ignoring the breakfast bell. Where sound perceptivity is *very* positive, valuable information may doubtless be obtained by this means.

The main object of this short paper is not, however, to enter into a discussion upon the reliability of methods, and for this reason references have been intentionally omitted, but to make known to English readers a very extensive research into this problem recently published by H. Stetter,[1] and carried out from 1926 to 1928 in the laboratory of Prof. K. V. Frisch at the University of Munich. His conclusions cannot fail to be of interest to any one dealing with fishes, but they are especially noteworthy as having been obtained by using the training

[1] H. Stetter, "Untersuchungen über den Gehörsinn der Fische, besonders von Phoxinus laevis L. und Amiurus nebulosus Raf.," *Zeitschrift für vergleichende Physiologie*, 9. Band, 2/3 Heft, 1929.

methods known as the formation of conditioned responses. That is, the fish is trained by the strict and unvarying association of some sound with the presentation of food, to perform a conditioned response involved in obtaining the food, such as swimming to a set place, coming out of a nesting bottle, or some other definite motor response.

For the greater part of his work he used the common minnow, *Phoxinus lævis* L. and, to a less extent, the catfish, *Amiurus nebulosus* Raf. The minnow is becoming the classical fish for all experiments on the sensory discriminations of fishes, on account of the ease with which it may be kept alive and the quickness of its response.

One individual is allowed to swim unfettered in a completely screened glass aquarium. In all experiments on conditioned responses it is desirable that the animal being investigated should be isolated. It is still further desirable that the registration of the response should be made in some way which precludes personal bias in interpreting the result. This is not actually the case in these experiments of H. Stetter, but by blinding his fishes before training he has aimed at the same thing. Parallel tests with fishes which were not so treated having given substantially the same results, he is probably right in saying that, in his own work, this precaution is of no importance. How extremely important is this strict isolation, in similar experiments with dogs, has been very strongly emphasised by Prof. Pavlov, and failure to observe due care in this matter has, over and over again, led to totally false conclusions. With all possible precautions of this nature, and a satisfactory apparatus for the generation and transmission of the vibratory stimulus, results are possible by this method which are not attainable by any other known method with such a high degree of reliability and accuracy when dealing with the animal in a healthy and living condition. H. Stetter's conclusions on the auditory sense of the fishes he has investigated are probably, therefore, the most accurate available, and should serve as an indication of what is to be expected elsewhere in fishes of a similar type.

The first step in the investigation has been to build up an association between one definite auditory stimulus, which is allowed to act for from 30 to 60 seconds, and the introduction of food at a definite place in the aquarium. To get the food the fish has to swim to that place, and to make various associated seeking movements of a characteristic nature. This is the "food reaction." After a certain number of these associations the sound, called by the author the "training, or food tone," brings about the "food reaction" by itself, even when not followed by the food. In Stetter's experi-

ments the smallest number of these associations necessary to form the conditioned food response was three: the greatest thirty-five. He records, for example, that one of the minnows was blinded (by removal of the eyes) on December 17th, 1925, and that he began the process of association to the note G of an organ pipe on January 8th, 1926. The striking point about this individual was that after only three associations of the sound with the introduction of food, the very characteristic "food reaction" was given when the sound was perceived. This is truly a very rapid learning and would even be regarded as such in much higher animals. Moreover, he has several instances where only five to seven associations were necessary. These figures are a striking confirmation of my own, obtained with many different types of stimuli, and are very close to those necessary for the formation of conditioned reflexes in dogs. Should these results be further amplified by similar work on other fishes, it seems that our present ideas on the working of the higher nerve centres may have to be altered quite considerably, for there can be no reasonable doubt, regardless of what future developments in nerve physiology may tell us of their true physical basis, that these responses are of essentially the same nature as the more familiar conditioned reflexes of higher animals. And yet there is nothing in the brains of these lowly bony fishes in any way comparable with that enormously complex mass of associational nerve fibre in higher animals—the cerebral cortex, and whose size and presence are significantly correlated with the degree of mental superiority. It would tend to lend support, in fact, to the contention of Dr. Coghill, of the Wistar Institute, that the total behaviour is built up through a process of analysis, rather than that it is gradually synthesised through the formation of more complex chain reflexes from the simple reflex.

The establishment of this association is proof of the fish's capacity to hear the sound used. In order to ascertain whether a fish so trained is able to distinguish that sound from any other sound of a contrasting or closely allied nature, the author has followed the procedure called by Pavlov "differential inhibition." The second, or contrasting sound, is associated with the introduction of food soaked in a strong solution of quinine, whilst the original note continues to be associated with good, wholesome food. The reaction to the food soaked in quinine is, of course, a vigorous swimming away from it, on account of its extreme distastefulness. This is the "caution reaction" and the sound associated with it the "caution tone."

All the fishes investigated, *i.e.* Minnow, Golden Orfe, Goldfish, Barbel, Miller's Thumb, and Catfish, are able to

associate with food the sounds of various wind instruments, such as the pitch-pipe, organ-pipe, or tin whistle; the sounds produced by vibrating strings, such as those of a guitar, a violin, whether plucked or played with a bow, or from a single string arranged with the aquarium as sounding board; or the note of tuning forks in direct connection with the aquarium walls. In addition, it has been shown that minnows can react similarly towards more complex noises, and many other types of sound.

The upper limits of sound perception vary greatly with the different species, and are by no means constant with individuals of the same species. It appears that the minnow can hear sounds produced by the above means up to the fifth octave, around 4,000 double vibrations per second. In the barbel the limit is somewhat lower at between E and G in the fourth octave, but in the catfish, with greater certainty, above G in the sixth octave. Of the lower limits of perception little can be said beyond that minnows react very definitely to sixteen double vibrations per second. Prof. G. H. Parker's conclusions that fishes are sensitive to very low vibrations thus receive neither confirmation nor denial from these experiments.

It is interesting to note H. Stetter's finding that minnows can easily differentiate between notes separated by two or three octaves without the differential training. Even with the use of the differential training it is only possible to get them to discriminate between notes separated by an octave or, in exceptional cases, by a minor third. Obviously their sense of discrimination is very poor indeed. With most fishes it is probably still less keen. A musical note can be distinguished from a mere noise, and with some of the minnows it was possible to bring about a discrimination between four to five separate tones, and of any of these from a noise. Different intensities of the same tone could not be distinguished, and in no case could it be established by use of the highly distasteful food soaked in quinine.

Although memory for these associations is apparently stable for a long period, H. Stetter is not inclined to attach any *biological* significance to the capacity of his fishes for perceiving and discriminating between these various sounds, but believes that their ability to do so is a latent power awakened into activity through the process of progressive conditioned training.

These fishes, then, appear to be endowed with quite a fair sense of hearing, and this would seem to justify the angler's belief. But it cannot be too strongly emphasised that what is true of one fish is not necessarily true of even another member of the same genus with possibly a nervous and bodily equipment indistinguishable from it.

NOTES

Obituary: F. V. Theobald, M.A., V.M.H., F.E.S. (H. F. Barnes, B.A., Ph.D.).

We regret to notice that this distinguished economic zoologist and entomologist died on the morning of March 6 at his residence, Wye Court, Wye, Kent. For some time past Mr. Theobald had not been well, and a serious attack of pneumonia in the spring of last year had undoubtedly lowered his resistance, so that an attack of bronchitis proved fatal. Born at Kingston-on-Thames on May 15, 1868, he was educated at private schools at his home and St. Leonards, and then went to St. John's College, Cambridge. He quickly became an Extension Lecturer in economic zoology for the University, and on the opening of the South-eastern Agricultural College in 1894 was appointed Lecturer in Agricultural Zoology at Wye. In due course he became Professor of Agricultural Zoology in the University of London, also Vice-Principal of the South-Eastern Agricultural College. From 1900–4 Mr. Theobald was responsible for the section of economic zoology at the British Museum (Nat. Hist.). He resigned the Vice-Principalship of the College when the advisory and research service of the Ministry of Agriculture was established, and he became Entomological Adviser for the south-eastern area of England.

His published works are at once numerous and diverse. In 1891, while he was still at Cambridge, *An Account of British Flies* appeared. The *First Report on Economic Zoology in the British Museum* (1903) and the *Second Report on Economic Zoology in the British Museum* (1904) bear witness to the widespread researches of the man, for these were the days before the *Review of Applied Entomology*. Systematic entomology also received his attention; witness his *Monograph on the Culicidæ* (1901–10). He published *Agricultural Zoology* (1898), still a standard textbook, and *Insect Pests on Fruit* (1909), a book of reference in great demand. Only recently, by the publication of the third volume, had he completed a monograph on the *Plant Lice or Aphididæ of Great Britain*. Besides such works, his annual reports, popular articles, and

pamphlets stand as lasting memorials to his far-reaching ability. To a few privileged persons he showed with boyish zeal his earliest work, a nature diary, the *Fauna of Sussex*, written and illustrated in colour when he was only about eight years old.

Although Mr. Theobald is perhaps more familiar to the younger generation as a leading authority on agricultural entomology, his great assistance to medical entomology must not be forgotten. When the discovery that mosquitoes were the carriers of malaria was made, it was imperative to place at once the whole group of mosquitoes upon a workable classificatory basis. Mr. Theobald was asked by the Trustees of the British Museum (Nat. Hist.) to do this. Only systematists can realise what a tremendous amount of work this entailed. Floods of collections came in to him from all over the world, sent by medical officers clamouring to know their identity and the possibility of their carrying disease: the journey from Wye to London was made several days a week, so that work could be carried on the more expeditiously. Mr. Theobald's *Monograph on the Culicidæ of the World* was the result, a stupendous work, done under high pressure at great speed. Nowadays, with our well-organised machinery for co-ordinating results, we know little of the difficulties extant in those days in dealing with such a little known family. This monograph has been criticised; Mr. Theobald's generic limitations have been altered, but now, after the first criticisms have grown old, various subgenera are being raised which run very close to the original genera. But this is beside the point. A man was required at short notice to deal with the Culicidæ. Mr. Theobald was chosen, and he fulfilled the difficult and thankless task. The groundwork was laid surely on a permanent basis.

Among the distinctions that were conferred on him were the Imperial Order of the Osmanieh, La Grande Médaille Isodore Geoffrey St. Hilaire of France, the Mary Kingsley Medal (Liverpool University), the Victoria Medal of Honour of the Royal Horticultural Society, and the election to honorary membership of the Société Nationale d'Acclimatation de France, the Société pour l'Étude d'Agriculture et de Zoologie de Bordeaux, the Société de Médecine Tropicale de Paris, the Association of Economic Entomologists, U.S.A.

Mr. Theobald throughout his life was an accomplished field naturalist, and his knowledge and experience of field pests were unrivalled and are likely to remain so. He was always a quick worker, but perfectly ready to change his views when convinced a mistake had been made. His decisiveness, his practical outlook, and his personality appealed to fruit-grower and farmer, staff and student alike. To those working under him he was an inspiration, ever ready to help, continually

spreading knowledge, never loath to take interest in all manner of outside affairs. Utterly unselfish, a true friend, an English gentleman, to old students "Freddie."

Cathodic Reduction of Sulphurous Acid (G. D.).

The reduction of sulphurous acid at the dropping mercury cathode has been studied by Gosman (*Collection of Czechoslovak Chemical Communications*, 1930, ii, 185), using the polarographic arrangement of Heyrovsky, who had previously observed that the current voltage curves obtained in electrolysis of solutions saturated with sulphur dioxide showed a "wave" at a cathode potential of about 0·20 volt. Reduction of undissociated molecules of sulphurous acid to hyposulphurous acid, $H_2S_2O_4$, occurs directly, since the reduction potential was found to be dependent on the concentration of sulphurous acid and of hydrogen ions.

Earlier observations that solutions of normal sulphites undergo no reduction at the mercury cathode were confirmed, but it was found that in neutral solutions a second reduction stage occurred at about −1·2 volt, when sulphoxylic acid, H_2SO_2, was believed to be produced.

Progress in Tannin Production[1] **(C. Scott Garrett, M.B.E., D.Sc., F.I.C.).**

Tannin extracts have mainly been derived from the barks of trees such as quebracho, oak, and hemlock, which are found wild or which are not grown specifically for bark purposes. Accordingly, there has heretofore been few, if any, attempts to control and nurture the tannin-producing properties so as to increase the yield and cheapen the cost of the tannin.

The easily accessible forests of quebracho are approaching exhaustion, and a time will soon arrive when artificial planting will have to be resorted to to maintain the supply of this tannin.

It is therefore of interest to note that scientific control has already been introduced into the tannin industry in the case of wattle bark, a material which is making rapid strides as a source of tannin extract.

The main source of this material is South Africa, and it is there that scientific control of production is most advanced.

The wattles or mimosas (genus acacia) are indigenous to Australia, where for a considerable time the bark of the wild trees has been used locally. About 1870, however, the black variety (*acacia mollis*) was introduced into Natal as an ornamental shrub or as a rapid-growing shelter for stock. It

[1] From "The Wattle Bark Industry," *Bull. Imp. Inst.*, 27, 169, 400 (1929).

quickly acclimatised itself, and it is now grown there and in other South African provinces, especially the Transvaal, as a commercial venture on account of the tannin value of its bark.

At first the bark only was exported, but factories for making the tannin extract have followed, and to-day at least a quarter of the value of production is from extract.

The industry is growing, and has attracted the benevolent notice of the South African Government. Overseas markets have been established in Europe, particularly Germany, and tanners in U.S.A. are beginning to recognise the potentialities of this source of supply of tannin extract.

That this advance will continue would seem certain, for owing to its rapid growth, wattle has a clear advantage over its competitors. Thus it shows a yield of about six tons of bark per acre, containing on the average 37 per cent. tannin, which is produced in from seven to ten years, whilst oak and hemlock crops require about eighty years for maturity, and give yields of four and eight tons of bark respectively with a tannin content of 12 per cent. Quebracho also requires a very long time to mature.

In wattle, therefore, the tannin industry has a material which grows rapidly in suitable climates, and produces a good yield of bark of a high percentage of tannin content. Such a fast-growing crop is much more suitable for scientific investigation towards improvement than its slow-maturing rivals, and already a comprehensive programme with this end in view has been embarked upon by the South African Department of Agriculture in conjunction with the Wattle and Timber Growers Association.

Experimental plots to record,—annual growth; maximum height and volume over the rotation period correlated with maximum yield; influence of spacing in planting; clean cultivation as opposed to cultivation in grass; effect of cleaning land by fire on the germination of the wattle seeds; influence of varieties of wattles; diseases and insect pests; fertilisers—these are some of the problems which will be attacked. Already the Department has shown that phosphate is the chief fertiliser required by the crop, preferably in the form of superphosphate, whilst the addition of potash and sulphate of ammonia is decidedly beneficial.

Finally, the economic success of the wattle industry is in great measure bound up with the profitable disposal of the by-products—the wood and the spent tan bark.

Heretofore the main outlet for the wood, where possible, has been for pit props, but absence of transport facilities precludes this as a general outlet. Alternatively, attention is

being paid to the possibilities of its destructive distillation. Wattle is a hardwood, and has been shown by tests at the Imperial Institute and elsewhere to give good yields of the normal wood distillation products. It also has possibilities as a commercial timber and for paper pulp.

The spent tan bark has proved on trial to be suitable as a raw material for straw-board and wrapping paper.

The British Adder (Norman Morrison, D.Sc., F.Z.S.).

The British Snake family consists of three species, viz.: the Ring Snake, Smooth Snake, and the Adder. Both ring and smooth snakes are non-poisonous, and are not indigenous to Scotland. The adder, on the other hand, is found in England and Scotland and is the only poisonous species we have in the British Isles.

The adder is a cold-blooded creature and might be said to be an intermediate type, or rather the missing link between birds and fishes, with more affinity to the latter than the former.

The British adder, or, as it is called, the northern adder, because it ranges over a wide area from Spain in the south to the extreme north of Europe, and through Siberia in the Far East, is absent from Iceland and Ireland, and some islands in the west of Scotland, notably Iona.

The average length of this species is about 21 inches, very seldom exceeding two feet, although specimens have been obtained measuring 27 inches, but these samples are extremely rare. The record for Britain at present is 28½ inches; this specimen is preserved in the British Museum.

The adder is stoutly built, the head rather flat, broadening towards the back of the neck. The tail differs in the sexes, that of the male being short and blunt while the tail of the female is long and pointed (see Fig. 1).

The marking of the adder is as follows: on the head is a V-shaped mark, pointing towards the proboscis. Immediately behind this angle, the adder is of a bright yellow colour, throwing into sharp relief the dark markings. Then proceeding along the back is a continuous dark brown zigzag line with sharp-pointed angles. This line of markings is continued to the tip of the tail. On each side of the body is a row of drab-brown lozenge-shaped blotches. Each spot lies opposite or rather between the points of the zigzag line running along the back as already mentioned.

The markings often vary in sharpness and shade, but the design is always present in the adder family, while on the other hand it is entirely lacking in the colour schemes of the

THE ADDER AT HOME. MALE AND FEMALE.

FIG. 1.—The distinction of the sexes is very apparent in this photo: the tail of the male is short and blunt, whereas the tail of the female is long and pointed.

FIG. 2.—The fangs of the adder magnified 13½ times (lengthwise). The opening through which the venom comes is seen near the pointed end on the outer side of the fang to the left.

ring and smooth snakes. Therefore, the adder can always be identified from the other species by the above markings.

The adder belongs to the viviparous species, that is, they produce their young alive, and have an average of seven. I once had a female adder in captivity which gave birth to seven young adders, measuring 4½ inches. They were fully developed with markings clearly defined, and if at large would have been quite able to glide about, and probably fit to look after themselves.

Sloughing—that is, casting off the old skin or epidermal covering—takes place twice a year. Whenever the adder's coat becomes ragged or frayed, Nature provides the creature with a resplendent new garment, which shows its colours and markings to perfection. The reptile has no eyelids, but the eyes are protected by a transparent covering resembling a watch-glass.

The adder hibernates during the cold season. About the first week in October it retires into holes in turf dykes and into cavities in the trunks of old trees, and beneath masses of decayed vegetation. In these winter quarters it remains in a comatose state till the advent of spring, say about the 5th of February. It is interesting to note that if one of these reptiles were disturbed during the winter sleep and brought back to consciousness, it would die immediately of exposure.

The principle underlying hibernation is simply to evade death from extreme cold. Biologically speaking, this state of inertia of reptiles and fish during the cold season means that the action of the heart is slowed down to a minimum—just sufficient to enable the living tissues to absorb oxygen and discharge carbon dioxide and water, producing heat and electricity ; and in this way the flickering candle of life is kept burning.

To refer to the adder sting is to repeat a common fallacy. The adder does not sting, and there is no danger to be feared from the fork-shooting tongue of the creature. The danger lies in the bite of the reptile.

The fangs of the adder are two slender, sharp-pointed teeth, barely a quarter of an inch long, curving inwards on each side of the upper jaw, and firmly united to the maxillary bone, which is freely movable. This mobility permits the fangs being switched from the horizontal to the vertical position. They are tiny tubes, with an opening near the point (see Fig. 2), When not in use they lie in a sheath on each side of the palate. The sheath also contains a pair of reserve fangs, ready for immediate use in case of accident to the other two, and also fangs in process of development.

The poison glands lie behind the eye, the broadest part of

the head. The poison secretion is conveyed by two ducts to the root of the fangs. When the adder is going to strike, it opens its mouth to an angle of nearly 90 degrees, erects its fangs, and darts its forked tongue out and in with a low hissing sound; then, swift as a lightning flash, the tongue is withdrawn and the sinister weapons plunged into the body of the victim. The action of the bite compresses the muscles of the poison glands, and drives a drop of the venom along the ducts, right up through the fangs, from the tip of which it is squirted into the wound. The mechanism of the poisonous apparatus works exactly on the same principle as the hypodermic syringe. If the victim receives a full bite, two small punctures show where the fangs have struck, but if the blow has been one-sided, there is only one puncture. The bite is followed by great pains and much swelling. Breathing becomes difficult and spasmodic, and the pulse weak and irregular. In some cases the patient for a time is completely prostrate.

The poison possesses a high ratio of blood-destroying or hæmolytic element. The chemical compounds of the venom are very complex. It is slightly viscid, a clear fluid with a specific gravity varying from 1·030 to 1·081. It coagulates immediately on exposure to air, and it contains at least two distinct toxic proteids but no bacteria.

It is interesting to note that the poison is harmless when taken into the stomach. It is only when it enters the circulatory system that it is injurious. It will therefore be obvious that the most effective means of curing adder bite is to prevent the venom from invading the blood stream.

The following instructions, if carried out immediately after the bite, will restrain the operation of the poison, and prove an excellent remedy.

First.—Without a moment's delay, stop the circulation by tying a ligature tightly above the wound—that is, nearer the heart.

Second.—Cross-cut the wound or punctures with a sharp knife and encourage bleeding by sucking hard with the lips if there is no broken skin about the mouth.

Third.—Apply to the wound a few crystals of permanganate of potash if they are at hand, and if not, burn or cauterise the wound with a red-hot wire, such as a knitting needle. Keep the patient quiet, give him or her a little stimulant, and send post-haste for the doctor.

Until science discovers or manufactures an anti-venomous serum, this is the simplest and most effective cure known for adder bite. The full bite of the adder, that is when one gets a maximum discharge from both fangs, is always serious, and even a healthy person would have a bad time of it for weeks; but fortunately fatal cases are very rare.

The natural food of the adder is mice, frogs, toads, lizards, and even rats. It is interesting to mention that adders can swallow objects of much greater diameter than that of their throats. I once found a full-grown rat in the stomach of a 22-inch adder. The explanation of this is that adders have no joint in their jawbones, as we understand that term. Their place is taken by elastic ligaments capable of a great amount of stretching.

Adders will not feed in captivity; consequently the cause of their death in captivity is usually starvation. Adders are also untamable. The only creature of our wild species in Britain which cannot be domesticated is the true wild cat of the Highlands, but, unlike the adder, this animal will feed in captivity. It is a well-known fact that the average person has a horror of serpents and snakes. These reptiles are looked upon as malignant creatures—the mortal enemy of man. Consequently they are ruthlessly destroyed. But in fairness to the poor adder, I must state that this belief is entirely erroneous; the adder is a most nervous creature, and will never assume the offensive, but glides quickly under cover from the presence of an intruder.

In conclusion, may I be permitted to add that I have once dined on an adder. I can affirm that the cooked reptile was by no means disagreeable to the palate. Neither did I experience ill effect of any kind after the unique meal. I can only describe the taste of the boiled adder as a combination of fish and flesh flavour, if one can imagine such a taste.

Notes and News

The list of the names of those approved by the President and Council of the Royal Society for election as Fellows was published too late for insertion in our April number. It is as follows: H. S. Allen, professor of natural philosophy, St. Andrews; E. B. Bailey, professor of geology, Glasgow; F. T. Brooks, lecturer in botany, Cambridge; P. A. M. Dirac, lecturer in mathematics, Cambridge; H. W. Dudley, chemist, National Institute for Medical Research, Hampstead; C. A. Edwards, professor of metallurgy, University College, Swansea; H. Eltringham, entomologist, Oxford; C. E. Inglis, professor of applied mechanics, Oxford; H. Spencer Jones, H.M. Astronomer Royal, Cape of Good Hope; E. K. Rideal, lecturer in physical chemistry, Cambridge; R. Robinson, chemist, Lister Institute; J. Stephenson, zoologist, Edinburgh; G. P. Thomson, professor of natural philosophy, Aberdeen; C. Todd, pathologist, National Institute for Medical Research; W. W. C. Topley, professor of bacteriology and immunology, London School of Hygiene and Tropical Medicine.

Sir Ernest Rutherford has been appointed to be Chairman of the Advisory Council of the Department of Scientific and Industrial Research in succession to the late Sir William McCormick as from October 1, 1930. Prof. V. H. Blackman will act as Chairman until October.

Sir William Bragg has been awarded the Franklin medal by the Franklin Institute, Philadelphia, in recognition of his work on X-rays and radioactivity.

The Cullum Geographical medal has been awarded to Dr. Hugh R. Mill, formerly director of the British Rainfall Association, by the American Geographical Society.

H.M. the King has approved the following awards by the Geographical Society: Founder's medal to Mr. F. Kingdon Ward for his explorations and botanical work in South-west China and North-east Tibet; Patron's medal to Mr. C. E. Borchgrevink, whose Antarctic Expedition in 1898–1900 was the first to winter in that region.

Prof. A. S. Eddington has been elected President of the Physical Society for the forthcoming session. Drs. Ezer Griffiths and Allan Ferguson will continue to serve as Honorary Secretaries. Dr. A. C. D. Crommelin will be President of the Royal Astronomical Society, with Drs. W. M. Smart and H. Dingle as Secretaries. Prof. W. C. M'Intosh has been elected President of the Ray Society; Dr. G. C. Clayton, President of the Institute of Chemistry, and Dr. J. T. Dunn, President of the Society of Public Analysts.

We have noted with regret the announcements of the deaths of the following men well known in scientific circles: Prof. J. O. Arnold, metallurgist of the University of Sheffield; the Rt. Hon. the Earl of Balfour; Prof. J. P. Borodin, botanist; Sir E. Brabrook, anthropologist; Dr. G. G. Chisholm, of Edinburgh, geographer; Prof. F. M. Exner, geophysicist, Vienna; Dr. A. R. Fee, physiologist, London; Prof. G. A. Gibson, mathematician, Glasgow; Prof. A. Henry, professor of Forestry, Royal College of Science, Dublin; Dr. D. H. A. Hutchinson, naturalist; Dr. Y. T. Mackay, anatomist, University College, Dundee; Sir William McCormick, Chairman of the University Grants Committee; Mr. E. T. Newton, palæontologist; Prof. W. Robinson, botanist, University College, Aberystwyth; Dr. R. F. Ruttan, chemist, past President of the Royal Society of Canada; Mr. A. A. Campbell Swinton, electrical expert; M. Armand Solvay, President of the Société Solvay; Prof. F. V. Theobald, biologist, Wye, Kent; Prof. E. G. R. Waters, entomologist.

General the Rt. Hon. J. C. Smuts has been invited to act as President of the British Association for the centenary meeting to be held in London during the period September 23–30, 1931.

The Royal Geographical Society celebrates the hundredth year of its foundation this autumn. The celebrations will commence on October 21, when H.M. the King will open the extension of the buildings in Kensington Gore. A series of historical lectures will be given, and a centenary dinner has been arranged for October 23.

The eleventh International Congress of Zoologists will be held at Padua during the week beginning September 4. Prof. P. Enriques will preside over the meeting. All enquiries should be addressed to Padova, Via Loredan, 6.

Prof. Donnan, professor of chemistry in the University of London, has been honoured by the creation of a Frederick G. Donnan Fellowship in Chemistry at the Johns Hopkins University, Baltimore. The Fellowship is open to graduates of any University in Great Britain and Northern Ireland.

The Rt. Hon. Sir Samuel Hoare, G.B.E., will succeed Lord Melchett as President of the British Science Guild.

The Council for Scientific and Industrial Research of the Commonwealth of Australia has established, on the recommendation of Sir Arnold Theiler, a Division of Animal Health, and Dr. J. A. Gilruth has been appointed Director.

Dr. A. T. Doodson, Associate Director of the Liverpool Observatory and Tidal Institute, has been awarded a Thomas Gray Memorial Prize of £150 by the Royal Society of Arts for his work on the Analysis and Prediction of Tidal Currents. Two prizes are offered in 1930: (*a*) a prize of £100 to any person who shall acquaint the Council of the Society of a valuable improvement in the Science or Practice of Navigation proposed or invented by himself in the years 1929 or 1930; (*b*) a prize of £100 for an essay on "The Training of Apprentices and Cadets with a view to their becoming efficient Officers in the Merchant Service." Proofs of claim for the first prize and essays for the second must reach the Secretary of the Royal Society of Arts, John Street, Adelphi, W.C.2, on or before December 31, 1930.

The use of gas-masks for respiratory protection against poisonous gases in industrial processes has been gradually increasing during the past ten years, their development having been advanced by the impetus given by war-time requirements; but for mine rescue purposes the self-contained breathing apparatus, carrying its own supply of oxygen or air, has been the only reliable protection from poisonous gases available in this country. With the discovery of hopcalite, a specially prepared mixture of manganese dioxide and copper oxide, which converts carbon monoxide into carbon dioxide, it has become possible to make gas-masks which give protection against afterdamp, provided there is sufficient oxygen present

in the air to maintain life, and in the United States of America a gas-mask, the "All Service" mask, has been approved by the Bureau of Mines, and has been used in mine rescue work with considerable success.

Close co-operation is maintained between the British Safety in Mines Research Board and the U.S. Bureau of Mines under a scheme which provides both for the carrying out of joint researches and for the exchange of investigators who are specially skilled in particular subjects. Under this scheme Dr. S. H. Katz, chemist-in-charge of the Bureau of Mines gas-mask laboratory, came to this country in April 1928, and for just over a year joined with a member of the Research Board's staff in carrying out investigations in the Board's laboratories at Sheffield with the object of designing a mask which would be as efficient against poisonous gases as the "All Service" mask, but would offer appreciably less resistance to breathing.

An account of these researches is now published by the Safety in Mines Research Board under the title of "Mine Rescue Apparatus: The S.M.R.B. Gas Mask," by S. H. Katz and C. S. W. Grice (*S.M.R.B. Paper* 57, H.M. Stationery Office, price 9*d.* net). The mask is in general arrangement similar to the box respirator used by the British Army in 1917 and 1918, but the corrugated tube, which connects the face-piece to the canister containing the absorbent materials, includes a check valve which operates a "timer" consisting of a small dial with luminous dots at the quarter positions and a luminous hand actuated by the breathing impulses. The hand completes one rotation in the allotted life of the canister, i.e. two hours with normal breathing. Air entering the canister passes through the following layers: a filter of turkish towelling, activated nut charcoal granules, absorbent cotton-wool, caustic soda fused on pumice granules, anhydrous calcium chloride, silica gel, hopcalite, anhydrous calcium chloride, cotton-wool, the whole being held in position by iron wire gauze at each end and between certain of the layers. The outfit weighs only 8¼ lb., while the usual oxygen equipment weighs 40 lb. Although the mask was designed primarily for mining purposes, it is hoped that it may prove suitable for the use of firemen and other workers who are liable to encounter poisonous gases in the course of their occupation.

The following table, taken from a paper by McLennan, Howlett, and Wilhelm in the *Transactions of the Royal Society of Canada*, 1929, contains a list of the pure metals and alloys which have been found to become superconductors, together with the temperatures at which their resistance vanishes.

Super-conductors. Pure Metals.	Trans-formation Tempera-ture °K.	Investigator.	Super-conductors. Alloys.	Trans-formation Tempera-ture °K.	Investigator.
Lead . .	7·2	Onnes	Au-Bi	1·93	deHaas, v. Aubel, and Voogd
			Au-Pb[3] .	above 4·21	
Mercury .	4·16	Onnes	(Cu-S) .	1·1	Meissner
Tantalum .	4·2	Meissner	Bi_5-Tl_3 .	6·5	de Haas and Voogd
			Sb_2-Tl_7 .	above 4·21	
Tin . .	3·7	Onnes	Cd-Tl .	2·50	
			Tl-Au .	1·92	de Haas, v. Aubel, and Voogd
Indium .	3·4	Onnes	Sn-Cd .	3·61	
			Sn-Bi .	3·8	
Thallium .	2·5	Onnes	Sn-Zn .	3·65	
			Pb-Ag		de Haas and Voogd
			Pb-Ca		
Thorium .	1·4	Meissner	Pb-Sb .	above 4·2	
			Pb-Bi		
Gallium .	1·07	de Haas & Voogd			
Ruthenium	2·04	McLennan, Allen & Wilhelm, 1930			

McLennan and his collaborators investigated a large number of metals and alloys, including molybdenum tantalum, hafnium, titanium, magnesium, tungsten, niobium, zirconium, uranium, sodium-lead alloy (Na_2Pb_5), the entectic alloy of cadmium and antimony, and misch metal (a mixture of rare earth elements), and showed that they do not possess superconducting properties.

The work of Sir C. V. Raman and his students has made the *Indian Journal of Physics* an essential item in the subscription list of a science library. Volume IV, Part V, contains the report of the Indian Association for the Cultivation of Science for the year 1928, and includes the Presidential address given by Sir C. V. Raman to the Indian Science Congress at Madras in 1929. Here Raman gives a remarkably clear account of the discovery and character of the effect which bears his name, and indicates the vast field of research which it renders accessible for the first time. The remainder of the Report contains an account of some of the forty-nine papers published by Sir C. V. Raman and his students during the year 1928; lists of publications, books, apparatus purchased and constructed in the workshop, and properly audited accounts showing, *inter alia*, valuations of the land, buildings, scientific instruments, and allowances for their depreciation. Among other papers in this issue of the *Journal* is one by Dr. Krishnan dealing with the bi-refringency of black soap films. The paper describes some experiments made by Raman with soap bubbles in

plane polarised light, and shows mathematically that although the results resemble effects obtained with uniaxial crystals, they can in fact be explained by assuming the films to be isotropic.

It is now a matter of a century since the railways began that revolution which diverted our national transport from the roads and canals to themselves. For long they emptied the roads, which fell into neglect, but now the whirligig of time is bringing road transport into its own again, and the railways are obliged to set their houses in order. In attempting to do this they find it difficult to live down their past, by which we mean not so much their own misdeeds as the mistaken restrictions with which Parliament sought to hedge them round, when it held them in tutelage. Motor transport, on the other hand, has been allowed to grow up unhampered, and it is only recently that our law-makers have turned their attention to its control. Our railways are unfairly beset with difficulties in their attempts to modernise their methods, in that practically every new venture of theirs requires an Act of Parliament, which is sure to be opposed by the road interests. Some of these difficulties are discussed by Mr. C. E. R. Sherrington in *Proc. Roy. Institution*, vol. 26, p. 33 (1929), where he outlines improvements in the systems under three heads: (*a*) Improvement in Track; (*b*) Signalling; (*c*) Speed and Punctuality of Service.

As far as safety is concerned, British railways probably present the smallest percentage of accidents to passenger-miles of all systems of transport in the world. Certainly, if there were as many casualties on the rail as on the road, there would be an outcry.

Mr. Sherrington's article gives an insight into the up-to-date methods of what too many people are wont to regard as an obsolescent transport system.

A useful summary of the present position of Architectural Acoustics is given by a *Circular* (No. 380) under this title, issued by the Washington Bureau of Standards. The subject has now reached a stage wherein one aspect of the design of auditoriums—that of obtaining the correct reverberation-time—is purely a matter of arithmetic. A list of absorption coefficients for common building materials and decorations being given, it is possible for the architect to take his proposed design and to calculate the number of "absorption units" which each surface will present, and find whether the total agrees with that which an ideal hall of the given size should have. If, as often arises, there is a defect of absorption units, there are materials of porous properties available, which will provide much absorption in a small space, and may be substituted for part of a harder surface in the proposed design.

Research should now be directed to improvements in these materials. Unfortunately, they are not cheap, and in many instances they are unsightly. Many suggestions for improving existing halls acoustically fall through from financial considerations, and some, possibly fewer, from æsthetic reasons.

It is surprising that, with such a large variation of absorption with pitch, data based on a mean pitch of 512 should be so effective. It seems to suggest that exact determination of the " absorption coefficients " (the fraction of the incident energy absorbed) is unnecessary, since, in practice, the incident sounds may cover the whole of the musical gamut. It is, of course, important to know how materials behave to notes of varied pitch, and, if possible, to choose those materials which give the most uniform response to incident sounds of all frequencies. Fortunately, the materials comprising the special absorbents conform most closely to this criterion; for they absorb the energy by friction in pores of the substance, whereas harder materials show special response to those frequencies which lie near the frequencies natural to them when vibrating as a drum. Instances are given in the pamphlet in which two such " drums " of hard material on opposite walls of a room caused prolonged reverberation due to the sound being bandied to and fro between them.

This pamphlet is an excellent summary of the practical aspects of the subjects treated, and may be commended to architects and to all who require a rapid grasp of the main principles involved. Its price is 5 cents, and it can be obtained from the U.S. Government Printing Office, Washington.

" A broadcasting station, to maintain its assigned frequency, must be provided with an accurate frequency standard. A piezo oscillator is the most suitable frequency standard thus far devised. It is a generator of radio-frequency current, the frequency of which is determined primarily by the dimensions of the quartz plate used. The Bureau's service to the broadcasting station consists of measuring the frequency of the piezo oscillator, or adjusting the quartz plate so that the piezo oscillator has the frequency to which the station is assigned, and issuing a test certificate."

W. L. Hall, in the *Bureau of Standards' Journal of Research*, vol. 4, p. 115 (1929), describes the method used in these tests, with full details of the apparatus. It is called a " beat tone " method. A more orthodox name for this would be " combination tone," or " difference tone." If one strikes the middle C and F on the piano loudly, one can distinguish a note low in the bass (F two octaves below) whose frequency is equal to the difference in frequencies of the two primary notes. Now the frequency of a piezo-electric oscillator (being in fact a small

crystal) lies above the audible limit. Combined with another note of a known high frequency, it will produce a difference tone whose frequency is sufficiently low to be accurately measured. To take an example given by the author, a crystal of 600,000 fundamental frequency, working with a valve-oscillator producing oscillations of 599,000 frequency, gives in a suitably connected telephone an audible response of 1,000 frequency. By tuning-up the valve oscillator it is possible to lower the pitch of the difference tone so far that (at about 30 vibrations per second) it ceases to excite the telephone. On noting the setting of the condensers of the valve oscillator—this having been previously calibrated—it is possible to tune-in to the crystal frequency, within this limit of 30 on either side. Full details of the apparatus, including a constant temperature room for the crystals, are given.

Temperature is an important factor in the control of the frequency, so that often the constant temperature cabinet, in which the crystals rest at the broadcasting station, is also submitted to the test, together with suitable slices of quartz.

These oscillators give a number of overtones as well as a fundamental frequency, which overtones can also be explored by the best method, and can serve as an additional check, as their relationship to the fundamental can be calculated. As a standard of frequency for all the determinations, a piezo oscillator, kept at a frequency of 200,000 by a thermostat, is employed.

The Monist for April contains an article by Lloyd Morgan on "The Bifurcation of Nature," which is a criticism of Whitehead's philosophy of nature with regard to his concept of a "concrete organism." Since this includes mental states, it inevitably leads to a bifurcation of qualities in nature, and Lloyd Morgan, of course, believes in one concrete nature, physical and mental.

C. Judson Herrick, in an article, "The Order of Nature," maintains that the modern concept of the order of nature is the legitimate offspring of the scientific method, and that there is no contradiction between instinctive faith in the order of nature and a consistent rationality.

Joseph Needham contributes the first part of an interesting paper on "Philosophy and Embryology," in which he surveys "neo-vitalistic" theories classed under the headings—Hormism, Finalism, Dynamic Teleology, Organicism, and Emergence.

In "Determinism and Modern Physics," C. M. Sparrow attempts to show that since we now rely on probabilities for the description of the behaviour of the ultimate constituents of the world, determinism has disappeared, with consequent necessity for a change in the philosophy of science. It seems,

however, that he is overstating the case when he says: "Determinism has been dispensed with as a necessity of rational thought in so far as the thinking of men like Bohr and Einstein can be called rational." Einstein still believes that nature is everywhere orderly, although our knowledge at present may only entitle us to description in terms of probability.

The Report of the University Grants Committee for 1928–9 (H.M. Stationery Office, price 3*s.* 6*d.* net) contains a review of the conditions in the Universities and their progress during the past quinquennium, together with a statement of "present needs and problems." The whole forms a statesmanlike and withal courageous document which should be read by all who are engaged or interested in University teaching. The total number of full-time men students has increased from 30,063 to 31,410 during the five-year period, while for women the figures show a small decrease from 12,962 to 12,899. In the pure science and agricultural groups the numbers remain practically unchanged; in the technological group there is a fall of 627, and in the medical group of 2,610. On the other hand, the arts faculties have increased by no less than 4,644, the increase being specially noticeable in Scotland. Nearly 50 per cent. of the students are known to be receiving financial assistance, the figures varying from 34 per cent. in London to 53 per cent. in Scotland and 67 per cent. in Wales. The statistical tables appended to the *Report* show extraordinary variations in the figures relating to different colleges and universites. Thus at East London College the total income per full-time student is £111, 50 per cent. of this coming from Parliamentary grants, and only 18·4 per cent. from tuition fees; at the Imperial College of Science the corresponding figures are £221, 52 per cent. and 23 per cent. respectively; at University College, London, £108, 35·6 per cent. and 33·6 per cent.; at Manchester University, £110, 34·7 per cent. and 25·6 per cent.; at Cambridge, £99, 25·6 per cent. and 27·2 per cent. (but these figures do not include the incomes of the individual colleges). Cambridge has over 5,000 students reading for a first degree and 260 research students; London, 5,600 first degree students and 690 research students (University College alone having 212).

A large part of the *Report* deals with matters relating to the staff, particularly the non-professorial staff. It is evident that the Treasury has commented unfavourably on the fact that in previous years the Grants Committee has laid repeated emphasis on the inadequacy of the salaries of the "Junior" staff and on the provision for libraries, and this in spite of large increases in the Treasury grant. During the last quinquennium the average salary of the lecturer class has risen

from £444 to £455 per year, so that the following sentence is only too well justified: "We feel, however, that the fact that such a criticism should have been possible deserves the attention of many Governing Bodies, since it lends force to our oft-repeated contention that universities and colleges would be wise to concentrate their efforts upon securing their academic foundations before attempting elaborations of their superstructure."

This advice is reinforced by the statement that "we shall be profoundly disappointed if, at the end of another quinquennium, we have once more to inform your Lordships that it is for expenditure upon these two items that additional income is in general most urgently required." At the instance of the Grants Committee the Treasury grant to the Universities has been increased by £250,000, and its expenditure will be watched with keen interest by the 2,000 lecturers and demonstrators in the Universities of England, Scotland, and Wales.

On the subject of research the Committee writes with moderation and wisdom: "We are inclined to agree that in the long run the teaching of a university is likely to suffer if its junior staff are led (*sic!*) to think that in matters of advances of salary or promotion to higher posts, a record of stimulating and successful teaching will count for less than an impressive list of published research work." This research problem does not only affect the juniors. It is obviously unwise to select as Head of a Department a man with a brilliant record of original work, and then to bring this work almost to a standstill by overloading him with administrative duties. The remedy, when such appointments are made, is obvious, though the writers of the *Report* do not give it. Perhaps they felt that they had gone far enough.

One other matter of urgent importance is touched upon—the question of the employment of the newly fledged graduate. It is tragic for a student to find at the end of his course that employment for which his training eminently fits him is forbidden to him on account of age. The number of graduates taken into Government and Municipal service is far too small. The man who obtains most of his education as junior clerk may know more of the detail of the work to which he is promoted at twenty-two than the graduate, but he is unlikely to prove the better man in the end, if only because he has not been selected with the same care, and has not had a tithe of the opportunities of broadening his outlook. Here is a matter on which university authorities might well bestir themselves more vigorously.

ESSAY-REVIEWS

BAND SPECTRA. By R. C. Johnson, M.A., D.Sc. Being a review of **Molecular Spectra and Molecular Structure.** [Pp. 611–954.] (London: The Faraday Society, 1929. Price 15*s.* 6*d.* net.)

In tracing the origins of experimental spectroscopy we are carried back almost a century. It is difficult to say exactly when the theoretical foundation of the subject was laid, but we should not be far wrong in regarding it as a post-war development. The modern theory of band spectra may be dated from about 1918, when Heurlinger's well-known dissertation was published. To Kratzer also we owe many of the fundamental ideas which were produced in the years immediately following. The last five years have been a time of great activity by both experimentalists and theorists. In particular we owe to Prof. Hund theoretical views which have proved of far-reaching importance, the implications of which are still being worked out. The whole subject is thus at present in a state of rapid development. The broad foundations are not likely to be much disturbed by future investigation, but the whole superstructure is undergoing transformation, and there is much speculation in the air. It is difficult for the research worker to keep in touch with the increasing output of original papers; it is almost impossible for a specialist in another branch of physics or physical chemistry to do so. At the same time the study of band spectra, with the information it provides of molecular structure, is throwing new light on many problems of physical chemistry—and is likely to do so increasingly. Under such conditions the present volume is particularly welcome, containing as it does contributions from some of the most distinguished workers in this field. These were read at a conference held under the auspices of the Faraday Society at the University of Bristol last September. A volume such as this lacks the unity of a textbook, the critic is disposed to see in it parts of very unequal value, and it is in no sense an exposition for the uninitiated. It has, however, compensating advantages in that it is up-to-date, and that the various articles represent for the most part the views of experts on their own subjects. The student of this volume can thus obtain a fair

appreciation of the ideas and interests which are at the present time engaging the attention of experts in the field of band spectra.

Some thirty-nine papers are included in the volume, and for convenience they are grouped under the three headings, " Band Spectra in the Visible and Ultra-Violet Regions," "The Raman Effect," and " Infra-red Spectra." In addition, Profs. Garner and Lennard-Jones have contributed a useful general introduction and a concluding summary.

In Section I, Prof. Richardson contributes a paper on nomenclature, a subject which has recently aroused a good deal of discussion. There now appears to be widespread acceptance of most of the important symbols, and we may shortly anticipate a long-overdue agreement on the subject through the activities of Prof. Mulliken of Chicago. It is understood that final decisions are to be published in the Physical Review.

Prof. Mulliken contributes a paper on band spectra and atomic nuclei, which reviews and interprets the information available by a study of alternating intensities (including as a special case alternate missing lines) in band spectra. The article is rather difficult reading. Prof. Hund writes on chemical binding in a brief but very interesting paper discussing the extent of the validity of London's theory of valency. The author deals in particular with combinations of the hydrogen and helium atoms. The problems touched on in this paper are fundamental to synthetic chemistry.

Other theoretical papers include one on the Electronic Structure of Diatomic Molecules, by Prof. Lennard-Jones, and on the Determination of Heats of Dissociation by means of band spectra by Prof. Birge. The latter paper is not merely a summary of the previous well-known work by Birge and Sponer, but embodies new developments of considerable interest. (In reading this paper equations 4 to 6 should present no difficulty if it is remembered that ΔE_{ω} represents the increase in E_{ω}, due to a change of *unity* in ω. This point for some time confused the reviewer.) One of the most interesting features is the existence of a discontinuity in the $E : \omega$ curves of molecules. Prof. Birge's paper should lead to a fuller knowledge of heats of dissociation, and should stimulate more research activity in a direction which yields valuable information from the standpoint of molecular structure.

Among the special molecules of which the band spectra are discussed are H_2 by Prof. Richardson, He_2 by Prof. Curtis, and CO by Dr. R. C. Johnson. Of all the known diatomic molecules these are the most prolific in band spectra, and the schemes of electronic energy levels are perhaps the most

nearly complete. To the papers of Prof. Richardson and Prof. Curtis are appended useful bibliographies.

In the case of CO the existence of quintet levels is discussed, replacing, if substantiated, levels previously believed to be triplet. The first part of this paper reviews elementary deductions from the data of band spectra, and should be useful to those who are not specialists.

Dr. Bengtsson and Prof. Hulthén write on the band spectra of metallic hydrides, dealing particularly with the structure of these molecules in the light of the Hund-Mulliken views on detailed electronic structure.

The Isotope effect in band spectra is represented by two papers of considerable interest. Prof. Birge writes on recent work which has been successfully directed to the discovery of isotopes present in a very small proportion in elements hitherto believed on the evidence of the mass-spectrograph to be single. The first impetus to this work was given by Giauque and Johnston, who, using data of Babcock on the atmospheric oxygen bands, obtained evidence of two isotopes of oxygen of masses 18 and 17. The approximate abundance of these species compared to O_{16} are 1 in 1,250, and 1 in 10,000 respectively—a remarkable illustration of the sensitivity of the spectrographic method, and in particular of the range of intensities recordable on the photographic plate. Birge and King have now found evidence of a carbon isotope of mass 13 by a careful examination of certain band spectra of C_2, CO_2, and CN.

Profs. Patkowski and Curtis describe and explain the vibrational isotope effect in ICl. This presents features of unusual interest on account of the relatively large changes in vibrational quantum number observed. The half-integral character of vibrational quantum numbers, required by theory, is supported by the experimental evidence.

Messrs. Kondratjew and Leipansky write on the recombination spectra of the halogens. Absorption spectra are discussed in several contributions. Messrs. Goodeve and Stein write on chlorine dioxide; Drs. Herzberg and Scheibe write on methyl, halides, and other derivatives in the far ultra-violet and Schumann region, and Mr. Barratt deals with the interesting spectra of metal molecules. Mr. Rawlins writes on the crystalline salts of the rare earths, and Prof. Henri deals with polyatomic molecules—in particular with the exhibited phenomenon of predissociation.

Considering the recent character of the Raman effect, the literature in this field has been voluminous. Some nine papers are presented on this symposium, including an introductory address by Prof. Raman himself, entitled "Investigation

of Molecular Structure by Light Scattering." This outlines the origin and progress of the investigations culminating in this discovery, and summarises the principal directions of subsequent enquiry. The paper is attractively written, is full of fruitful ideas, and exemplifies Prof. Raman's characteristic modesty and enthusiasm. It would be difficult to find a better introduction to this subject. Prof. Wood writes on methods of excitation of Raman spectra. The paper is of an essentially practical character, and the details of technique should be valuable alike to those who are doing or contemplating doing research in this field.

An interesting paper by Prof. Cabannes gives a résumé of the wave theory of diffusion spectra and the quantum theory of the Raman effect. There is a further paper by the same author on the polarisation of Raman radiation.

Prof. Daure writes on the Raman effect in some of the more easily liquefied gases: HCl, NH_3, CH_4, C_2H_6, C_3H_8, C_2H_4, and C_2H_2 dissolved in acetone. Prof. McLennan deals with the effect in liquid oxygen, nitrogen, and hydrogen. This work provides experimental proof of the theoretical view that hydrogen at low temperatures should be a mixture of two molecular types—the symmetrical and anti-symmetrical variety.

Prof. Allen has a brief note on the existence of Raman lines in the spectrum of the electric discharge, and Prof. Taylor on the Raman effect associated with some AX_4 groups. Dr. Menzies writes briefly on the plane polarised character of Raman radiation, and the production of such spectra by light scattered from powdered crystals. This latter possibility, which was independently discovered by Prof. Baer of Zurich, should provide an interesting direction for further research.

The third section of this volume deals with infra-red spectra. Prof. Schaefer presents an account of the infra-red spectra of solid bodies, and Prof. Taylor gives detail of some work on infra-red absorption spectra of salts containing the group AX_4. A second paper deals in particular with $KMnO_4$.

Dr. Lecomte contributes the introductory paper on the infra-red spectra of liquids. This is certainly a field of work of importance for a knowledge of molecular structure, although the problem of interpretation is not an easy one. One of the chief needs seems to be for experimental work with instruments of much higher dispersion and resolving power. Prof. Bonino of Bologna writes on the infra-red band spectra which arise from a CH linkage in organic molecules. Prof. J. W. Ellis discusses in an interesting paper the absorption of liquids below 3μ.

Sir Robert Robertson introduces the important subject of infra-red spectra of gases with an historical survey and a résumé of the information which can be derived in this field. Profs. E. F. Barker and C. F. Meyer contribute a valuable paper on their high dispersion work on gases. Other papers are by Dr. Rideal on chemical structure and infra-red analysis, by Mr. F. I. G. Rawlins on the form of the CO_2 molecule, by Prof. Taylor on the probable infra-red spectrum of sulphur vapour, and by Mr. C. P. Snow on vibration-rotation spectra of diatomic molecules. Prof. Mecke and Mr. R. M. Badger, in a brief paper, give the correct interpretation of the infra-red ammonia spectrum.

The Faraday Society are to be congratulated on the organisation of the Bristol Conference and on the publication of so useful a volume.

THE INFLUENCE OF PRESSURE ON THE COMBUSTION OF GASEOUS MIXTURES. By W. Payman, D.Sc., Ph.D. Being a review of **Gaseous Combustion at High Pressures.** By William A. Bone, D.Sc., Ph.D., F.R.S., Dudley M. Newitt, Ph.D., D.I.C., and Donald T. A. Townend, Ph.D., D.I.C. [Pp. xii + 396. With 14 plates and 148 diagrams.] (London: Longmans, Green & Co., 1929. Price 42s. net.)

The pioneer work of Professor Bone and Drs. Newitt and Townend on gaseous combustion at high pressures, the main results of which have already been published in the *Philosophical Transactions and Proceedings of the Royal Society*, is well known. This volume gives a connected account of their researches, together with a description of the equipment and experimental methods employed in the High Pressure Gas Research Laboratories of the Imperial College, where the investigations are in progress. It will be assured of a generous welcome from the ever-increasing number of research workers interested in gaseous combustion and in high-pressure reactions, and it will also have a wide appeal to chemists and physicists in general.

It might be expected that an extension of research on gaseous combustion into the realms provided by high initial pressures would help to throw light on some of the obscure but familiar phenomena observed at normal pressures, but more often such an extension introduces disturbing factors or new complications. This has been largely the outcome here, but the new problems are of absorbing interest. The most striking example is the discovery of the activation of nitrogen during the combustion of carbon monoxide in air at high pressure, an activation which is not produced to any appreciable extent at initial pressures below 10 atmospheres. Substitution of the nitrogen in a $2CO + O_2 + 4N_2$ mixture by oxygen or carbon monoxide caused a great increase in the

rate of development of pressure, although these gases have much the same density and heat capacities as nitrogen, and therefore their effects, as diluents added to the $2CO + O_2$ "detonating mixture," would be presumed to be the same. The maximum pressure attained was not altered to any marked extent, so that dissociation, after-burning, or the formation of oxides of nitrogen could not be the cause of the comparatively low rate of development of pressure when nitrogen was present. Interesting evidence is advanced in support of the authors' suppositions that: (i) Before being burnt, each molecule of carbon monoxide must itself be "activated" or "excited" quantum-wise by absorption of the radiation emitted during the combustion of another carbon monoxide molecule, and that an activated carbon monoxide molecule is instantly burnt, emitting the same radiation again; and (ii) that nitrogen molecules, by absorbing quantum-wise the same radiation and "holding it up," slow down the rate of the carbon monoxide combustion. The extraordinary resemblances in the physical properties of nitrogen and carbon monoxide indicate that the two gases might act "in resonance" in the manner suggested.

The effect of activation is more marked at higher pressures, the increased density of the medium aiding the absorption of radiation. Thus the effect of increasing pressure is to decrease the rate of development of pressure in a $2\ CO + O_2$ mixture when diluted with nitrogen, but to increase the rate when undiluted, or when diluted with gases other than nitrogen.

The occurrence or non-occurrence of activation in other mixtures is examined partly by observation of this effect of pressure variation and partly by an examination of the cooling curves. The authors regard the first as the more reliable method, but unfortunately it does not give consistent results. Thus increasing initial pressures of 10, 50, 100, and 150 atmospheres gave times of development of pressure of 0·10, 0·18, 0·36, and 0·53 second for the mixture $2CO + O_2 + 3{\cdot}76\ N_2$, implying activation of nitrogen at the higher pressures; whereas times of 1·22, 0·36, 0·25, and 0·22 second were obtained in a corresponding series of experiments with the methane mixture $CH_4 + O_2 + 3{\cdot}76\ N_2$, implying absence of activation. In another series of experiments with the methane "Knallgas" mixture $CH_4 + 2O_2 + 7{\cdot}52\ N_2$, initial pressures of 10, 50, and 125 atmospheres gave times of 0·08, 0·15 and 0·17 second, which would suggest that nitrogen may also be activated in methane-air explosions at high pressures.

An interesting point emerges when these results with methane are examined. According to the hydroxylation theory of hydrocarbon combustion, the primary oxidation of methane involves "a non-stop run" through a dihydroxy stage, which

yields, by thermal decomposition, carbon monoxide, hydrogen and water.

$$CH_4 + O_2 \rightarrow CH_2(OH)_2 \rightarrow CO + H_2 + H_2O.$$

Yet at atmospheric pressure the air mixture containing these combined proportions of methane and oxygen is not inflammable, the pressure having to be increased to 10 atmospheres before it does become inflammable. The reason for this should prove interesting.

Among other interesting matters dealt with in the book special mention may be made of the descriptions of the work on the production of nitric oxide during explosions of carbon monoxide-oxygen-nitrogen mixtures, and on the spectrographic investigations of the ultra-violet radiations emitted during explosions.

The chapters on the gaseous medium at maximum pressure and on the analysis of time-pressure curves are valuable, but it is to be regretted that more attention has not been paid to the consideration of the period of rising pressure, for, despite any dissociation, after-burning or nitrogen activation which may take place, the main part of the gaseous combustion, with which the book is primarily concerned, takes place during this period. It might be pointed out that the value t_m given throughout is not the true time of attainment of maximum pressure, but the time to maximum pressure from the instant at which the pressure-gauge begins to record, and neglects the time during which the pressure is rising too slowly to affect the gauge. Incidentally it may be noted that attainment of maximum pressure in 0·005 second is regarded as instantaneous. In a vessel 7·6 cm. in diameter this implies a speed of flame of only 15 metres per second, and it is not usual to regard a rate of flame movement of even 2,800 metres per second as instantaneous.

Enough will have been said to indicate the interest and importance of the varied matters dealt with in this book. The excellent descriptions and illustrations of the research equipment and the experimental methods employed will be of the greatest value to other workers engaged on high-pressure reactions, and students of combustion will be glad to have the results collected together in such a convenient form.

THE CONQUEST OF SUPERSTITION BY SCIENCE. By H. STANLEY REDGROVE, B.Sc., A.I.C. Being a review of **Devils, Drugs, and Doctors**, by HOWARD W. HAGGARD, M.D. [Pp. xxii + 405 + 16 plates.] (London: William Heinemann. Price 21*s.* net.)

"THE history of medicine is at once the history of human wisdom and the history of human credulity and folly." Many

years ago I wrote these words in reviewing Wootton's now very well-known *Chronicles of Pharmacy*. I am constrained to repeat them, since this splendid book by Dr. Haggard so forcibly emphasises their truth. It is, indeed, an amazing story Dr. Haggard has to tell of stupidity and superstition and gross callousness towards the suffering of others, slowly conquered by the scientific spirit.

A considerable and by no means the least interesting portion of his book is devoted to the question of childbirth and the gradual change which has taken place in the treatment meted out to the childbearing woman. "The position of woman in any civilisation," he justly remarks at the very opening of his book, "is an index of the advancement of that civilisation; the position of woman is gauged best by the care given her at the birth of her child." Judged by this criterion, mankind has been civilised a very little while. For centuries, physicians refused to have anything to do with the business of childbearing. It was beneath their dignity. Those who so far forgot themselves as to assist a woman at this critical period of her life were subject to condemnation. Care of the childbearing woman was entrusted to midwives, who were always woefully ignorant and frequently vicious as well. It was typical of the spirit prevailing during the Middle Ages "that attempts were made to form intrauterine baptismal tubes, by which the child, locked by some ill chance in its mother's womb, could be baptised and its soul saved before the mother and the child were left to die together." Society was too intent on dealing with the supposed ills of the soul to bother much about the ills of the body. Moreover, everything concerning sex was hedged about with superstitious taboos engendered by the teachings of the Church. It was right that the woman should bring forth in sorrow since she had, no doubt, enjoyed the act which led to conception, and all instinctive pleasures were sinful.

Indeed, we have not yet quite escaped from this atmosphere of superstition, as is evident from the arguments advanced to-day against the practice of contraception, the hindrances placed in the way of knowledge concerning the subject being disseminated, and the hostility directed towards much-needed research.

But we have evolved far, and in Dr. Haggard's book we may read of Paré's reintroduction of podalic version, of Semmelweiss's conquest of puerperal fever, and of the use of anæsthetics in connection with childbirth. It was not only the inherent difficulties of the problems which faced them with which the pioneers in scientific medicine and surgery had to contend, but the indifference or, worse, hostility of

the multitudes who resented any advance in knowledge as an affront to Authority.

Writing in the latter part of the eighteenth century, Gibbon could, with justice, describe the period from A.D. 96 to 180 as that during which the human race was most happy and prosperous. It is true that during this period mankind was afflicted with two visitations of the bubonic plague, and that malaria, diphtheria, anthrax, tuberculosis and other diseases took heavy toll of victims; but it was no worse than the period from which the world had just emerged. Few folk, outside the medical profession, realise what immense benefits they derive to-day from the discovery of antiseptics and aseptic surgery.

Dr. Haggard groups all the methods used to treat disease under three heads—faith healing, drug cures, and hygienic therapy, of which the last is undoubtedly the most important. In connection with faith healing, his book contains a criticism of the pretensions of Mrs. Eddy and the "Christian Scientists," which is especially telling because he freely recognises the important part the mind plays in curing certain diseases, *i.e.* those having a basis in hysteria. It is, however, misleading, I suggest, to describe Mrs. Eddy's ideas as similar to those advocated by the philosopher Berkeley, since Mrs. Eddy denied the reality of sensations, whereas Berkeley, in denying the reality of matter, never questioned the reality of sensations, but denied only the reality of a hypothetical cause for them. Nor does it tend to clarity of thought to regard psycho-analysis as a form of faith healing. A fourth category of methods of treatment would seem to be needed for this. And finally, to finish what I have to say in criticism, it does not seem quite fair to certain preparations now on the market to assert that "vitamins are found more abundantly in . . . tomatoes than in the prepared compounds," which, therefore, owe what efficacy they possess to faith.

Dr. Haggard is not less severe in his criticism of those who place complete reliance in drugs than he is of those who would cure all diseases by faith. The story of the extraordinary hold the views of Galen obtained, so that they became invested with supreme authority, is very interestingly told. Galen's fantastic philosophy encouraged excessive drug-taking. A courageous genius, such as Paracelsus, was needed to challenge it; and, although many of the views of Paracelsus were not less, but even more, fantastic than those of Galen, his work of destructive criticism and his introduction of powerful and poisonous remedies of mineral origin constituted a turning-point in the history of medicine the importance of which cannot be overestimated.

It is in the department of prophylaxis that the science of medicine has made its greatest strides and has been of greatest service to suffering humanity. No longer do we fear the plague; but, as Dr. Haggard points out, were we to cease for a moment to guard against it, our great cities might suffer a devastation far worse than anything ever experienced in the past. Not only is "prevention better than cure," but prevention is often possible when cure so far eludes the skill of mankind.

Every member of the medical profession will read this history of the healing art with absorbing interest. But it is no dry-as-dust recital of bare facts. Dr. Haggard's book is one of intense human interest, and he has enriched his story of medicine and surgery with numerous illustrations of great historical value. Hence his book is essentially one for the "man in the street," who will learn from it the immense debt he owes to medical science and especially to preventive medicine. Certainly every woman ought to read the book, and every woman who does so will read it with the greatest interest and enjoyment, since it is the manner in which medical science has learnt to answer her peculiar needs which has been Dr. Haggard's especial care throughout.

EXTREMES OF WET AND DRY WEATHER IN THE BRITISH ISLES. By E. V. Newnham, B.Sc., Meteorological Office, London. Being a review of **British Floods and Droughts,** by C. E. P. Brooks, D.Sc., and J. Glasspoole, M.Sc., Ph.D. [Pp. 199, with 15 figures, 2 plates.] (London: Ernest Benn. Price 10s. 6d. net.)

The authors of this book have done a good piece of work in collecting information about the spells of extremely dry and wet weather that are known to have occurred in the British Isles. It was a comparatively simple task, although a laborious one, to deal with the period since Mr. G. J. Symons founded *Symons's Meteorological Magazine* in 1866, because the number of accurate published rainfall records is large from that year onwards, and has, apart from minor fluctuations, increased. *British Rainfall*, which is now published by the Meteorological Office, is the lineal descendant of *Symons's Meteorological Magazine*; it contains over 5,000 records, of which a large proportion are contributed by private observers. Instrumental records date back at least two hundred years earlier still, but the absence of standardised methods of measurement makes it necessary to exercise caution in accepting these. The authors admit the impossibility of obtaining a reliable notion of the general rainfall of the British Isles for any of these two hundred years, for they state (p. 75) that such estimates would be "about as near the mark as if we had rainfall measurements

from a single place and accepted that as representative of the whole country." For the years before about 1650, if estimates of the general annual rainfall are attempted these have to be based mainly on journals and chronicles, in which notable spells of wet or dry weather are recorded. Since such estimates must be even more unreliable, one wonders whether they are of any comparative value. In this connection we are informed that what is believed to be one of the most reliable records of weather in the Middle Ages, the Journal of the Rev. William Merle of Oxford, " presents a picture which is extraordinarily like that given by day-to-day records of recent years " ; on the other hand, the frequent mention of exceptionally wet or stormy seasons by other chroniclers at about the same period conveys quite a different impression. It is a temptation to suppose that if all our informants were as little given to exaggeration as Merle appears to have been, so many of the early years would not have been described as very abnormal.

The foregoing remarks show why it is questionable whether anything is gained by the construction of such a graph as that given in the work under review (Fig. 14), which purports to give the fluctuations in the " raininess " of the British climate since Roman times. This graph was based mainly on the frequency with which great rains and droughts were chronicled in each century, the method of calculation being correctly described as " purely arbitrary " (the words are taken from the book). It is true that enough is said by the authors to make it clear that the graph is very speculative, but such productions are apt to be reproduced in other works—more particularly in those of a popular pseudo-scientific kind—without any qualifying remarks, and may then be accepted as demonstrated truth by numerous readers. Fortunately, only a small part of the work is of such a highly speculative character ; the discussion of events during the past fifty years occupies over three-quarters of the book, of which three-quarters a large proportion is concerned with the period since the World War, a period for which the information not only about rainfall but about other meteorological conditions over Europe is particularly full, owing partly to the stimulus towards meteorological enterprise afforded by that war.

Had the authors confined themselves merely to a descriptive treatment of their subject, the result would still have been a work of real value. Fortunately, they have not restricted themselves in this way, and an attempt has been made to link up the observed anomalies of rainfall with various statistical relationships that have been established between the weather in the different " centres of action " throughout the world. Examples of such " centres of action " are afforded by the regions of

almost persistent high and low pressure around the Azores and Iceland respectively; and excellent examples of the kind of relationship alluded to above by the correlations that have been found between barometric pressure at the two centres—a negative correlation which shows that on the average, if the Azores pressure is above its normal value, the pressure at Iceland is below the normal for that region, and vice versa, and by further correlations relating the difference of pressure between the Azores and Iceland with the weather in various parts of Western Europe, including the British Isles. This linking of local phenomena with anomalies of the same or different weather elements in distant countries tends to open up useful lines of research, but cannot be expected to give by itself more than a very incomplete explanation of events. It seems to appeal to a certain type of mind as a possible means of obtaining mathematical formulæ that will enable the future weather of any place to be predicted from antecedent weather there and elsewhere without the necessity for an exhaustive analysis, on thermodynamical lines, of the process whereby part of the sun's heat is absorbed by the earth and its atmosphere, and is converted into kinetic energy through the agency of vertical currents and ordinary winds, from which all but a negligible fraction of the earth's rainfall is directly derived.

A brief summary may be added of the principal facts about spells of abnormal rainfall that are to be learned from this book. Before doing so attention may be drawn to the excellent introductory chapter entitled "The Rainfall of the British Isles," which states very clearly and concisely the principal ascertained facts about the average distribution of our rainfall, and about the different types of rain from which it is derived—the cyclonic, orographic, and local "instability" or thunderstorm types. Credit for this chapter is due not only to the authors but to those earlier workers who have been concerned with the analysis of the records obtained from private and official observers—Symons, Mill, and Salter—who have gradually built up a sound foundation for the subject. The causes of drought and flood are found to be numerous and complicated, and it is consequently impossible to lay down a single type of anomaly of the general circulation of the winds over the Atlantic and Europe as the typical type for either extreme. It is found, however, that the circumstances which reduce for long periods the strength of the moisture-bearing westerly and south-westerly winds, and so tend to cause drought, are often either a northward migration of the whole belt of westerlies, coincident as a rule with an increase in their strength, or a reduction of their strength which permits long periods of easterly or north-easterly winds. No clear explanation is given as to why such

changes in the general circulation of the wind take place. The conditions for excessive rain are shown to be scarcely definable, and still less explainable.

Periodicities in rainfall are shown to be of little use for forecasting seasonal rainfall, the proportion of the rainfall "accounted for" in this way being small, while a well-marked periodicity which may appear for a time is apt to vanish or change its phase.

It is doubtful whether more than is given in the above two paragraphs can be claimed as genuine knowledge in the main subject-matter of the book. To those who wish to learn about the more controversial relationships that have been suggested at one time or another between our weather and such factors as Gulf Stream Drift, Arctic Ice, etc., and to those who are interested in the details of recent and historic floods and droughts, the book can be recommended, for no similar body of information has been collected into a single volume in this country.

TEXTBOOKS ON NUTRITION. By Leslie J. Harris, D.Sc., Ph.D., Director of the Medical Research Council's Nutritional Laboratory, Cambridge. Being a review of: (1) **Chemistry of Food and Nutrition,** by Henry C. Sherman, Ph.D., Sc.D. [Pp. xii + 636.] (New York: The Macmillan Company, 1928. Price 12*s.* 6*d.* net.) (2) **The Story of Vitamins,** by H. Valentine Knaggs, L.R.C.P., etc. (London: The W. Daniel Co., 1929.)

Books on vitamins, nutrition, and dietetics increase rapidly in number. Their range covers—publications of vegetarian organisations; the propaganda of "New Health" Societies; official Government reports or treatises; exhaustive German encyclopædias on *die gesamte Ernährungskunde*; guides to housewives; "puffs" to advertise proprietary articles; pseudo-medical works for laymen, advocating the lemon or the soaked-currant cure; elementary, or advanced, or specialised manuals; and, even, *theosophy* applied to vitamins.

The last entry will surprise most readers. The work the reviewer has in mind here is called *The Story of the Vitamins* by H. Valentine Knaggs, L.R.C.P., etc., 1929. A few quotations from it will suffice to disclose its nature.

> "What an astounding thing it is, therefore, when we come to look at it, to think that all life in all its diverse and beautiful forms is in reality merely an endless number of vitaminic patterns. No wonder this vitamin friend of ours is of such vital importance!
>
> "There, after that second wonderful elaboration of the primary atom, the earth was poised, ready and eager, burning with radio-activity, in anticipation of still greater

happenings. Then, at that point in creation, the Creator was ready to consummate His plans to beautify every corner of His house so that the untold masses of crystalline and colloid vitaminic material might sweep forward with radio-active intensity into the lovely and entrancing patterns which were destined to evolve. . . .

" In fact, fish are radio-active by yielding up a cold blue-violet type of essence. . . .

" What if the Creator decided to reverse the polarity of the world ? What then ? Well then it would not be a case of ' Back to Methuselah ' but ' Back to Vitamins,' for all the life around us, its beauty and movement, would resolve itself, *en masse*, into its one primary form—the vitamin. And then ? Well then it would be possible, by another change of polarity, for creation to start all over again ! . . .

" *The author of this book spent much of his spare time during the years* 1910 *to* 1914 *investigating the subject of microphotography and vitamins.* . . .

" Disease only breeds unhappiness, and unhappiness in the world is a thing we want to do away with. So let us get to this research with a clear eye and a clear brain so that millions of people who suffer may know what it is to feel the unadulterated Life Force pouring through their veins, so that humanity may be brought nearer and nearer to the perfected state of its evolution. In the vitamin lie such power and energising material that in the near future, once we have learnt more about it, who knows but that disease will no longer exist in the world and humanity walk the earth with radiant happy faces and healthy bodies. . . .

" *And now having reached this point it is not improbable that some reader having noted the title of this book will say—' Yes, but what has all this to do with vitamins ? '* "

The scientific research worker will undoubtedly appreciate particularly the passages which are italicised above. He will be interested also to learn that Dr. H. Valentine Knaggs was investigating " vitamins " before the word had yet been invented by Funk, and before Sir Frederick Hopkins had published the pioneer paper on the question. This book is adorned with an interesting picture of its author as its frontispiece.

A considerable mental jolt is experienced on putting down *The Story of the Vitamins* and taking up Professor Sher-

man's excellent textbook. We have frankly nothing but praise for it. It deserves as wide a sale in England as we have no doubt it enjoys in its author's own country. It is a clearly written, a well-balanced, an accurately informed and a sufficiently documented manual of 600 pages odd, which should fulfil every need of the general student desiring a thorough grounding in the outlines of the subject. It may be compared in scope and style with such standard college class books as, say, Senter's *Physical Chemistry* or Holleman and Walker's *Organic Chemistry*. There is no other textbook on nutrition which covers quite the same field. Thus, McCollum and Simmond's *Newer Knowledge of Nutrition* is a more detailed survey likely to appeal rather to research workers than to the average class of student. Then, Funk's *Vitamins* presents the more personal viewpoint, and again with a wealth of detail very useful indeed to the expert. Ragnar Berg's *Vitamins* and Ellis and McLeod's *Vital Factors of Foods* are (like the last mentioned) not sufficiently recent to give the present position of the science. Lusk's standard work, *The Science of Nutrition*, is of course, concerned primarily with energetics. The only grumble we have with Sherman's book is that while it is dated 1928 (on the title page) a further examination shows it to have been set up in 1926 and since reprinted. It was certainly well up-to-date in 1926, but, needless to say, several important advances have been made in nutritional knowledge in the last four years. We therefore look forward with pleasure to seeing a fifth edition, and, as we hope, in the near future.

REVIEWS

MATHEMATICS

The Great Mathematicians. By H. W. TURNBULL, M.A. [Pp. viii + 128, with 19 diagrams.] (London: Methuen & Co., 1929. Price 3s. 6d. School Edition, 2s. 6d.)

PROFESSOR TURNBULL has given a brief account of the history of Mathematics by recounting something of the lives and work of its greatest exponents. In a subject which offers such a wealth of material the author must of necessity confine his attention to certain outstanding features. This he has contrived very successfully to do. Although the actual accounts of the contributions to knowledge made by individual mathematicians are inevitably curtailed, yet Prof. Turnbull has succeeded in linking them together to give a coherent and connected presentation of the main developments of the subject. It is fascinating to observe how almost invariably a step forward by one mathematician rendered possible a further step by a succeeding pioneer—how often some of the most valuable discoveries cast a shadow before them. So Hamilton worked out his great principle of the "Characteristic Function," and, as the author states, "the work of Gauss and Hamilton eventually merged in a broader mathematical harmony. It needed but one step more—to devise a means of applying these ideas to space of more than the three ordinary dimensions—for the theory of Relativity to come into being. This essential step was taken by Christoffel, who worked with Riemannian geometry, and to-day the grandly sounding World Function of Hilbert is none other than the characteristic function of the youthful Hamilton, rehabilitated in four dimensions. It was a stroke of genius when Einstein found in this exceedingly elaborate geometry the very medium needed to cope with actual physical phenomena."

As far as the arrangement of the subject-matter is concerned, it is, as would seem natural, dealt with chronologically. The author has endeavoured, wherever possible, to avoid technicalities, and has, as he states in the preface, "tried to show how a mathematician thinks, how his imagination, as well as his reason, leads him to new aspects of the truth."

The book appeals as an excellent little work in which the student who finds in mathematics an attractive study may make what is probably his first acquaintance with its nature and purpose as a deliberate human activity. Aware of the impossibility of giving an exhaustive account, the author has given some acceptable references for further reading. There is little doubt that the young student will be inspired by this little volume to avail himself of the references, and pursue the subject yet further.

SYBIL D. JERVIS.

A History of Mathematical Notations. By PROF. FLORIAN CAJORI. Vol. I: Notations in Elementary Mathematics. [Pp. xvi + 451, with 106 figures.] (London: The Open Court Company. Price 25s. net.)

So vast is the quantity of material on this interesting subject that Prof. Cajori very appropriately decided to present the *History of Mathematical Notations* in two volumes—of which the first is devoted to the history of symbols in

elementary mathematics. There is little doubt that this work will prove invaluable to all students of mathematical history.

After a preliminary introduction the book deals in turn with numeral symbols and their combinations, symbols in arithmetic and algebra, and geometric symbolism. Under the first of these headings the author has given some account of the symbols of the earlier civilisations, dealing first with the Babylonians, Phœnicians, Syrians, Hebrews, Greeks, Early Arabs, Romans, and following with short references to Peruvian and North American knot records, the Aztecs, Maya, Chinese, and Japanese, concluding with a detailed discussion of the Hindu-Arabic numerals. It is to the Indians that we owe the ingenious idea of expressing all numbers by means of ten digits to which are assigned simultaneously both an absolute and a place value. As Laplace observed, the very simplicity of the idea and the facility it imparts to all calculation are apt to blind us to its merits—while at the same time they undoubtedly place it in the front rank of useful inventions.

In the section concerned with arithmetical and algebraic symbolism Prof. Cajori gives first an account of groups of symbols used by individual mathematicians from Diophantus down to John Wallis. There follows a survey of the use of notations, of which an interesting example is the development of the symbols for addition and subtraction. The modern algebraic signs + and − appear for the first time in German and Latin manuscripts of the Dresden library. It is a matter of some interest that the two manuscripts had been annotated by J. Widman, who was the first mathematician to use the + and − in print—so that it was in Germany towards the latter part of the fifteenth century that these symbols came into use. In the early part of the seventeenth century they were adopted by writers in Italy, and at this time several forms of the plus sign existed, containing amongst them the Greek, the Latin, and the Maltese cross. The Greek cross, introduced by Widman, has, however, been the prevailing form of the plus sign.

Similarly, we have an account of the origin of the symbols for multiplication, division and ratio, proportion, equality, common factors, decimal fractions, powers, roots, unknown numbers, and aggregation. Here, as elsewhere, we observe how the achievements of mathematics have often been hindered by the persistence of older and inferior forms, so that the superior types which ultimately replace them win a belated recognition with surprising difficulty. To this cause is due the slowness of development at certain stages, for "nowhere more than in Mathematics is intellectual content so intimately associated with the form in which it is presented, so that an improvement in the latter may well result in an improvement in the former." It is striking also how intensely slow is the process of unification of symbolism, how great are the difficulties experienced in reaching a common world language. The author is of the opinion that the only hope for rapid approach of uniformity in mathematical symbolism lies in international co-operation through representative committees.

Finally, having treated somewhat briefly of the symbolism of ordinary elementary geometry, the author gives a short account of the struggles between the symbolists and rhetoricians. In the latter part of the nineteenth century and at the present time English geometrical textbooks contain a moderate amount of symbolism, thus avoiding the two extremes represented by Robert Simson on the one hand and Oughtred and Barrow on the other. So the "golden mean" has gained a victory, and a conflict in England lasting 250 years has ended in a draw—a valuable object-lesson to mathematicians on the importance of a suitable symbolism.

We are indebted to Prof. Cajori for his able exposition of this extraordinarily fascinating subject, and for his scholarly manipulation of material whose content might have seemed overwhelming. He has indeed achieved his purpose, of presenting in the history "a mirror of past and present

conditions in mathematics which can be made to bear on the notational problems now confronting mathematics." And doubtless, as he states, "the successes and failures of the past will contribute to a more speedy solution of the notational problems of the present time."

S. D. JERVIS.

PHYSICS

Wien-Harms Handbuch der Experimentalphysik. Band xxiv. 2. Teil—Röntgenspektroskopie. By AXEL E. LINDH. [Pp. vii + 436, with 197 illustrations.] (Leipzig: Akademische Verlagsgesellschaft m.b.H., 1930. Price M.40 geh., M.42 geb.)

WE have always had good cause to praise the several volumes of this important work as they have appeared, and again we have nothing but praise to add on the appearance of the new volume, on X-ray spectroscopy, by Dr. Lindh, of the University of Lund. The binding, illustrations, and printing are of the same outstanding qualities as those of its precursors. The book opens with an introductory outline of the earlier fundamental researches in the study of X-rays. It gives a comprehensive description of the many forms of X-ray tube used in up-to-date research laboratories. It treats the general principles of X-ray spectroscopy, paying particular attention to the description of ionisation chambers, but we should have liked to see the precautions advocated by Kulenkampf mentioned under his name. In his description of precision wave-length measurements, the author reproduces a complete set of data to illustrate his remarks on the necessary experimental adjustments, and in all sections of the book he gives detailed discussion of experimental errors. Special attention is devoted to the Siegbahn vacuum X-ray spectrographs and to the use of optical gratings in X-ray spectroscopy. The deviations from the Bragg relation and the dispersion of X-rays are fully discussed. In the treatment on emission spectra we find descriptions of the important experiments of Thoræus and Dauvillier on the K series of the elements from boron to fluorine, and a valuable set of tables of the emission spectra of the elements is provided. There is an equally excellent treatment of absorption spectra. In the description of the measurement of X-ray intensities, particular emphasis is laid on the counter-chamber method, perhaps rather too little attention being given to other methods. The effect of chemical combination on the absorption spectra of the elements is treated as fully as our present knowledge allows. The chapter on the continuous X-ray spectrum and the determination of *h* is also worthy of note. Robinson's work is well described in the description of the magnetic spectrum method for the determination of energy levels, and in the final chapter, on the determination of critical potentials of light elements by soft X-ray excitation, the work of Andrewes, Davies, and Horton finds an important place. In fact, the author has shown a particularly wide acquaintance with work done outside the Continent of Europe, and he is to be congratulated on the wonderful account which he has presented of a subject which is growing so rapidly, and which is being extended by so many able workers. He is, too, to be thanked for the complete lists of important pieces of original work, arranged, according to year of publication, to be found at the end of the book.

L. F. B.

Müller-Pouillets Lehrbuch der Physik. Elfte Auflage. Edited by A. EUCKEN, O. LUMMER and E. WAETZMANN. Band 2. Lehre von der strahlenden Energie (Optik). Edited by K. W. MEISSNER. [Part I—pp. xvi + 929–1708 + 19. Part 2—pp. xv + 1709–2392.] (Braunschweig: Vieweg und Sohn, 1929. Price R.M. 87.50.)

WE have much pleasure in recording the publication of the remaining portions of the Müller-Pouillets work on optics. The whole work was originally en-

trusted to Lummer, who unfortunately died in 1925. The first part of the work was published in 1926. It dealt mainly with geometrical optics and the theory of optical instruments, so that a very large portion of the newer work, which we might refer to as modern light, remained to be dealt with by Lummer's successor. The task was indeed not an easy one, for much of the work was already in the press, fresh contributors had to be found for some sections, and much had to be revised and rewritten owing to the rapid development of the subject. The result of K. W. Meissner's labours, however, is a very important contribution to the literature of the subject, and this complete work on optics is one which no teacher of advanced optics can afford to overlook. It is, of course, mainly designed for German students and teachers, and therefore treats rather more fully the German contributions to the subject. It is surprisingly up-to-date in many sections; for example, there is quite a good introduction to the new wave mechanics in the chapter on the fundamental ideas of the quantum theory applied to the theory of atomic structure. We heartily recommend this work. D. O. W.

Quantenchemie. By ARTHUR HAAS, D.Phil. [Pp. 76, with 7 illustrations.] (Leipzig: Akademische Verlagsgesellschaft m.b.H., 1929. Price R.M.3.80, kart.)

THIS little book contains four lectures on the fundamentals of the quantum theory applied to chemistry, delivered before the Physical Chemistry Society of Vienna in the spring of 1929. The lectures are, of course, somewhat expanded in the book, and a small appendix on ortho- and para-hydrogen has been added. It gives in a very convenient form the more important recent contributions of physics to the understanding of the periodic table, valency and chemical forces, the grouping of electrons within the atom, and the structure of molecules and nuclei. L. F. B.

Physics, Parts II, III, and IV. By W. J. R. CALVERT, M.A. [Pp. vii + 172, vii + 202, x + 333.] (London: John Murray. Price 3*s*., 3*s*., and 4*s*. respectively.)

THIS is a readable elementary treatise suitable for schools up to Matriculation standard. We are pleased to see such up-to-date subjects as acoustics of buildings, echo sounding, the flicker photometer, and the cinematograph given prominence, while such useless old stagers as Lissajous' figures are omitted. The elaborate description of the siren could also have been spared and its place taken by the stroboscope. "The siren is generally used as a means of determining the frequency of a given note," we read; but who uses it now? The mathematics, as it should be in this type of textbook, is cut down to a minimum; indeed, there is none worth speaking of in the Sound section, and in the Light section most of it is consigned to an appendix entitled "Lens and Mirror Formulæ." A second appendix to the latter describes a set of laboratory experiments for the student to perform, but there is no similar list appended to the Sound; indeed, when the student is advised to sing a scale to illustrate to himself the functions of the vocal cords, he is recommended not to do it in the laboratory! A useful set of questions (not numerical) is given for each chapter.

In the Electricity and Magnetism section we find the same principle of reserving the mathematics for appendices carried out, though we are rather surprised to find the Wheatstone bridge and the potentiometer relegated also to an appendix, since we thought that these by tradition formed the main portion of an elementary electricity course, to which all the rest lead up. The author follows a modern fashion when he makes electrostatics

follow after the electric current. "Starting switches," "overload releases," "polarised relays"; these topics will indicate the essentially practical and up-to-date trend of the section.

E. G. R.

CHEMISTRY

The Effects of Moisture on Chemical and Physical Changes. By J. W. SMITH, B.Sc., Ph.D. [Pp. xii + 235, 44 figures.] (London: Longmans, Green & Co., 1929. Price 15*s*. net.)

THE observations and theories of a number of investigators on the effects of drying on chemical systems and the remarkable observations of Baker and Smits on the effects produced by intensive drying on liquids have, in recent years, aroused great interest in this subject, and the literature has now grown to considerable dimensions. In this book Dr. Smith has made a very careful summary of the literature on this subject. The section on chemical changes closes with a short summary and very brief discussion of the more important theories which have been advanced. This is followed by several chapters devoted to the influence of intensive drying on physical properties, and chapters on the effects produced by other catalysts and the apparent influence of an electric field. The book will be of great value to the specialist on this subject as providing a very full summary of previous work. It can hardly possess that interest for the general reader which a more critical work would have had.

In the section on chemical changes, especially in the chapter on "Other Gaseous Reactions," a number of interesting and little-known experiments are brought to light, on reactions which have never been investigated in the light of modern knowledge. An example of this is the action of hydrogen sulphide on oxides of nitrogen. An interesting field for research is thus suggested to the chemists of to-day. The most serious criticism concerns the treatment of the question of the shift in equilibrium caused by intensive drying. It is clear that such a shift is only possible if the work done in removing a trace of water from the system is greater than the free energy change involved in the equilibrium shift. Thus we should not expect the shift caused by the removal of a trace of water to be measurable in chemical reactions involving a large energy change. This was pointed out by Bodenstein and Jost in connection with the hydrogen-iodine reaction, and their criticism is recorded on page 73. However, no attempt is made to explain the apparent shift observed by the author in the nitric oxide-oxygen reaction, to which the criticism equally well applies.

A valuable feature of the book is the space devoted to details of experimental technique, which will be of great value to other workers.

G. R. GEDYE.

Richter-Anschütz: Chemie der Kohlenstoffverbindungen oder Organische Chemie. Zwölfte Auflage. Erster Band, Aliphatische Verbindungen, bearbeitet von Fritz Reindel. [Pp. xvi + 882, with 18 figures.] (Leipzig: Akademische Verlagsgesellschaft m.b.H., 1928. Price 57 gold marks.)

RICHTER'S Organic Chemistry has been, for many years, the best of the smaller reference books; it is packed with information, a veritable Beilstein in miniature, and is invaluable as a handy key to the literature of organic chemistry. For years many practising organic chemists have kept it within easy reach, and have been grateful for the time it has saved them in consulting the larger reference books, too bulky to be kept on the working table. The appearance of a new edition is therefore somewhat of an event.

Twenty years have passed since the publication of the last edition, and

these years have seen a great change in outlook towards organic chemistry as well as the accumulation of many new facts. Although the latter is reflected in an increase of eighty-nine pages in the text, the point of view has not materially changed, and such theoretical considerations as there are in the book are rather those of a past generation than of to-day.

Frankly, the new edition is disappointing, not because it is not an improvement on the last, but that much is expected from a book with so great a reputation.

For detailed criticism one turns naturally to the subjects in which the most notable advances have been made in recent years.

The ultimate analysis of organic compounds has been revolutionised by Praegel; more reference to his methods could well have been made to the exclusion of those of Dennsted, which have generally been found disappointing. The method of Ter Meulen for the direct determination of oxygen is not mentioned.

Although our views of stereochemistry have advanced considerably the phenomena of optical activity is approached from the point of view of the asymmetric carbon atom rather than that of molecular dissymmetry.

In the sugar group much new material has been added, and the six-membered ring structure adopted. Recent work on the more complex carbohydrates is briefly summarised, and some mention is made of modern views on protein structure.

When describing the formation of cyclic ketones containing eight or more carbon atoms in the ring, no reference is made to the important work of Ruzicka, it being implied that the calcium salt of azelaic acid on distillation gives essentially cyclo-octanone.

Minor points are that no indication is given that the carbamide structure of urea is not entirely satisfactory and, more surprisingly, that no mention is made of the preparation of methylamine from formaldehyde, an operation now commonly part of an elementary practical course; the preparation of trimethylamine by this reaction is, however, noted.

In general, however, it is only fair to say that the work has been brought up-to-date, no light task, and is well printed and got up. To the best of the reviewer's recollection the price of this part of the work in 1909 was about twenty shillings, well within the reach of the advanced student; one cannot but regret that the cost has now become almost prohibitive.

O. L. B.

The Essentials of Chemical Physiology. By W. D. HALLIBURTON, J. A. HEWITT, and W. ROBSON. [Pp. xi + 383.] (London: Longmans, Green & Co., Ltd., 1929. Price 9*s*. net.)

FEW textbooks in the rapidly growing biological sciences live to attain their majority. Prof. Lusk's *Science of Nutrition* is now twenty-four years old and still flourishing; and here is a book written by Prof. Halliburton thirty-seven years ago now reaching its twelfth edition. Its value as a practical handbook to medical students needs no advertising: it is sufficient for the reviewer to note the more important changes introduced in this edition. In collaboration with two distinguished biochemists of a younger generation Prof. Halliburton has revised the book completely. The general arrangement—thirteen lessons, followed by an advanced section of seven or eight lessons—is unchanged, but considerable improvement results from the abolition of the rather bulky appendix, and the incorporation of its subject-matter in the appropriate parts of the text. Only thirty-five pages have been added, this being necessitated largely by the increased scope of the chapters on Blood and Respiration, and Urine. The paragraphs on the effects of extirpating the pancreas have been dropped from the lesson on

pancreatic enzymes, and a fuller treatment in the light of recent insulin studies is included in the lesson on tissue respiration. The sections on immunity and on the vitamins have been brought up-to-date, and the chapter on enzymes might with advantage have been similarly treated. And is it not time we stopped talking of the "tensions" of gases when we mean their pressures?

There is no doubt that this book has a long life yet before it, and there are few better ways in which a medical student can spend nine shillings.

P. EGGLETON.

The Mechanism of Enzyme Action. By F. F. NORD. [Pp. x + 78, with 7 figures.] (London: Baillière, Tindall & Cox, 1929. Price 9*s.* net.)

AT the request of numerous friends, Dr. Nord has attempted to supply the need for a "short, disinterested orientation on certain sections of enzyme chemistry." Three-quarters of the text is confined to the yeast fermentation, one-tenth is devoted to "transformations through other micro-organisms"—surely the citric acid fermentation alone deserves more than this?—and one chapter is left for other aspects of "sugar dissimilation." The subject-matter is thus rather restricted in scope, and hardly justifies the title. The text shows evidence of hasty production, and suffers from a number of Germanic constructions and inelegant Americanisms. The following is fairly typical:

"The significance of phosphate esters in the course of alcoholic fermentation, in contrast with the change of substances in muscles, *i.e.*, the formation of the myophosphates thus remains as yet obscure (unless we be permitted to predict a conciliation of the contradictions in a manner analogous, perchance, to the findings according to which the fact of fermentation of composite sugars by the effect to specific zymases without preliminary hydrolysis is due to a considerably greater number of zymases than as hitherto supposed)."

This example comes from page 10, but there are others on pages 11, 20, 24, 30, 35, 46, 51, 53, 58, and 66, which deserve honourable mention. Careless misstatements occur at intervals; on page 40, for example, we are told that "the transformation of pyruvic acid to lactic acid produces an increased hydrogen ion concentration (since the dissociation constant of pyruvic acid is $3{\cdot}6 \times 10^{-13}$, that of lactic acid being $1{\cdot}38 \times 10^{-4}$)," but on page 44, in another connection, "we know that pyruvic acid is a very strong acid ($K = 0{\cdot}56$)." A surprising number of errors is concentrated in these two statements. Another arithmetical mistake, coupled with some unique English, appears on page 50. Printer's and other small errors were noticed on pages 13, 18, 33, 34, 41, 42, 48, 61, and 69.

The book contains a good account of modern work on the yeast fermentation, particularly that of the Neuberg school, but very little information about the mechanism of enzyme action.

P. EGGLETON.

The Chemical Aspects of Immunity. By H. GIDEON WELLS. An American Chemical Society Monograph, Second edition. [Pp. 286.] (New York: Chemical Catalog Company, 1929. Price $6.00.)

THE chemical aspects of the study of immunity are in general so tenuous that, like the slender girl in the limerick, they are liable to disappear if looked at sideways. . . . But very soon, if the advances recorded in this edition fulfil their promise, this jibe will be quite unwarranted. I have got it out just in time.

It says much for the care and foresight which went to the first writing of this book in 1924, that in spite of the very thorough revision to which

it has now been subjected, nothing has had to be eliminated, and the general plan is unaltered. A great many additions have been woven into the text, which has been enlarged by thirty-two pages. Stress is laid on the definition of an antigen as that which *engenders* antibodies; recent work has brought to light a range of substances capable of giving precipitin reactions with specific antisera although incapable of stimulating their production in the body. Such substances are distinguished as "haptens," and are not necessarily proteins. Antigenic properties have so far only been observed in proteins. Toxicity and antigenic activity appear to be separable properties of a substance, and the evidence favours a non-protein nature for the purest toxins at present isolated; it seems probable that they may unite with the body proteins, so altering them that they become antigenic. The sensitivity of some individuals to iodine, formaldehyde, etc., is explained on such a basis. On the other hand, the latest evidence supports the view that antitoxins and antibodies in general are either proteins or very intimately associated with proteins. The view of agglutination now accepted is that the globulin of the immune serum is absorbed on the surface of the foreign bacteria, the action being, of course, specific. A similar view is held of the opsonisation of tubercle bacilli with immune serum. The possibility is revived that the Wassermann reaction may be a case of auto-immunisation, that is, of the formation of an antigen of protein-lipoid nature (derived either from the spirochæte or from damaged tissue), which engenders an antibody in the circulation.

As was the case of the first edition, the chapters are divided up by sub-headings to facilitate reference, and each chapter finishes with a summary. A definite improvement is the inclusion of all references as footnotes at the bottom of the first page on which they are cited. The book is therefore not only an excellent review of recent advances: it is a very handy book of reference as well.

P. Eggleton.

Outlines of Biochemistry. By Ross Aiken Gortner. [Pp. x + 793.] (New York: John Wylie and Sons; London: Chapman & Hall, Ltd., 1929. Price 30*s*. net.)

"Biochemistry" used to mean the chemical end (and a rather frayed end at that) of medicine and human physiology. Prof. Gortner has with considerable success attempted to write of it as a science in its own right; he writes as a chemist dealing with substances and phenomena met with in biology.

Out of a total of thirty-five chapters the first ten deal with the phenomena of the colloidal state—a subject which is probably at the heart of biochemistry. There follow ten chapters on the proteins and seven on the carbohydrates; treatment in proportion to their importance is given to the fats, lipoids, and essential oils, the tannins, chlorophyll, and related pigments, the flavone colours, and the vitamins. The treatment of proteins, as is to be expected from Gortner, is excellent, though the final chapter on miscellaneous nitrogenous bases (including the alkaloids) is altogether too brief and elementary. The chapter on immunological reactions included in the protein section makes particular reference not to medical applications, but to the value of immunological tests in tracing phylogenetic relationships among plants and animals. The work of Metz in the former connection deservedly receives prominent notice.

It is an unfortunate chance that the paragraph on glutathione in the section on nitrogenous bases was written just before the question of its constitution was reopened by Hunter and Eagles. The structural formula given by Gortner is the earlier dipeptide formula, now known to be incorrect. A second edition of this book would also be improved by the use

of " dynes per cm." instead of " dynes per sq. cm." as a measure of surface tension (see p. 40).

The vitamins are considered in some detail from the chemical standpoint—a feat which has only recently become possible—in a chapter towards the end of the book, and the last chapter is an excellent review of enzyme chemistry.

References to original papers are made at the foot of the appropriate page, but there is an additional list of detailed books and monographs classified in the order of the sections in the text, given at the end of the book. An author index of 14 pages, and a subject-index of 35 pages, greatly enhance the value of what would be in any case a first-rate treatise.

P. Eggleton.

Analytical Processes, a Physico-Chemical Interpretation. By T. B. Smith. [Pp. viii + 373, with 51 text-figures and 14 tables.] (London: Edward Arnold & Co., 1929. Price 12*s.* 6*d.* net.)

This book is written with a two-fold purpose. In the first place, a detailed discussion of some twenty typical analytical processes serves to elucidate some of the most important principles and theories of modern physical chemistry. On the other hand, the presentation is designed to impress on the student the value to the professional analyst of an insight into these theories. In particular such an equipment will enable him to tackle problems which demand a modification of routine methods with greater confidence, and will assist him in estimating the accuracy of such innovations.

These purposes boldly stated do not, however, give an altogether adequate impression. For, although the author does not parade it before us, there is surely a deeper purpose. Surely we see here a courageous attempt to assist in the development of scientists. We say assist, because a man becomes a scientist through his own efforts. The teacher who makes easy the path to answering examination questions does not assist, he more often hinders, more particularly when he is endeavouring to get his pupils " through " an examination the syllabus of which is too exacting. Our present educational system is responsible for the production of many university students who lose interest in anything they find not to be " needed " for the coming examination. The younger generation of university teachers is becoming thoroughly alive to this situation. This is a very hopeful sign, for although " spoon-feeding " is rampant in many of our schools, it is the universities who control the examinations. It is from the universities, therefore, that reform must come.

Those who wish to spoon-feed or to be spoon-fed will not appreciate this book. Analysis instead of being something to be *learned* becomes a subject of inquiry. Analytical methods are constantly being improved principally through advances in physical chemistry. Analysis has not reached finality, there are many questions to which no definite answer can be given. The important thing is to acquire a truly scientific outlook, to acquire a true perspective in appreciating the interplay of observation and theory, to be careful that theories make observation more searching and that observations are constantly directed towards assessing the scope of theories.

R. K. Schofield.

Hydrogen Ions, their Determination and Importance in Pure and Industrial Chemistry. By Hubert T. S. Britton, D.Sc. [Pp. xiv + 515, with 108 text-figures and 104 tables.] (London: Chapman & Hall, 1929. Price 25*s.* net.)

Those whose work has necessitated the determination of hydrogen ions are accustomed to using the works of Michaelis and Clark as their standard

books of reference, and will perhaps wonder whether there is room for another work on the same subject. Any doubt, however, is soon dispelled by a perusal of Dr. Britton's very readable book. This author does not attempt to vie with Michaelis in expounding the theory of the subject, nor with Clark in giving details of highly accurate technique. He takes up a more general viewpoint, and draws an impressive picture of the importance of hydrogen in general and industrial chemistry. A great deal of general chemistry may be learned from this book, and it is safe to predict that, in consequence of the simplification brought about by the use of quinhydrone, pH measurement will take an increasingly important place in a chemist's training. It provides a channel for bringing quantitative measurement into the heart of chemistry. For instance, it brings order into the perplexing subject of basic salt formation.

Roughly one-third of the book is devoted to the importance of hydrogen-ion concentrations in industrial chemistry. The industries considered include electroplating, tanning, sugar, paper, brewing, milk, baking, ceramics, dyeing textiles, and ore flotation. The author also discusses their bearing upon water purification, corrosion, sewage disposal, and soil fertility. It is evident that he has made a careful study of each of the industries dealt with, and his presentation of the subject is lucid though at times somewhat lengthy. It might be urged that an undue amount of space is devoted to the leather industry; but why should we complain when we are given full measure? A terse exposition is difficult if not impossible where a subject is in the process of rapid development, and it is possible that when the time comes for issuing a second edition, the theories of some of these industrial processes will have crystallised further, and lend themselves to a more compact presentation.

Though refraining from entering into the highest refinements, the author gives a thoroughly up-to-date account of technique, including the glass electrode, valve amplifying potentiometers, and automatic electrometric titrators. Standard data for preparing indicator and buffer solutions are given.

R. K. Schofield.

Simple Research Problems in Chemistry. For Junior Students. By F. Sherwood Taylor, M.A., B.Sc. [Pp. viii + 100.] (London: W. Heinemann, 1929. Price 4*s*. net.)

The spirit of this excellent little work is indicated in the first sentences of the Preface: "The chemist whom the world wants is not the man with a knowledge of a vast collection of facts, but the man who is able to solve problems," and with this ideal in view Mr. Taylor has collected together some 145 little problems in chemistry, varying from simple pyrogenetic experiments to those involving a clear understanding of the ionic theory and solubility products.

The book is divided into two sections, the one dealing with Problems in Inorganic Chemistry and detection of products of reaction, whilst the latter half of the book is concerned with Problems in Physical Chemistry, such as reversible reactions, catalysis, hydrates, and phenomena connected with ionic equilibria.

The intention of the author is apparently to place the book in the hands of senior boys at school, or first-year university students, and to let them tackle selected problems one by one, using the short theoretical discussions, aided by his own common sense, to help to find adequate solutions, referring also where necessary to textbooks and original papers.

Undoubtedly some of the problems given will be a joy to the keen student of scholarship standard and a pleasant substitute for the old-fashioned "Find-four-metals-and-two-acids-in-this" type of work.

The only fear is that the book as a whole may be a little too concentrated and indigestible for most junior students, and that ultimately it will be found concealed in a drawer in the demonstrator's desk, from whence it will emerge at examination times to provide ideas for practical examinations. Obviously the use that is made of the book will depend upon the views of the teacher; but in any case the keen student who is fortunate enough to have it on his bench will obtain great profit and intellectual pleasure from working through the problems in it.

F. A. Mason.

GEOLOGY

Spitsbergen Papers, Vol. II. Scientific Results of the Second and Third Oxford University Expeditions to Spitsbergen in 1923 and 1924. [Collection of 25 papers of separate paginations.] (Oxford University Press, London: Humphrey Milford, 1929. Price 30s. net.)

This handsome volume contains the scientific results of the second and third Oxford University Expeditions to Spitsbergen. The results of the first expedition in 1921 were published in Vol. I of the series. Both the later expeditions were led by Mr. George Binney, and accomplished very valuable exploratory, geological, and biological work in northern Spitsbergen, especially in Northeastland, usually a most inaccessible corner of the region. Twenty-five papers are included in the volume under review, of which one, however, is a remainder from the first volume.

The twenty-four papers resulting from the 1923 and 1924 expeditions are distributed as follows: exploration, geography, and general scientific results, five; meteorological, two; geological, three; biological and ecological, fourteen. A few more papers are to come, but these are to be incorporated in a third volume, together with the results of the Oxford University Greenland Expedition of 1928. It would be undesirable to single out any special contributions, but it may be said that the book records a splendid volume of excellent work in all branches. The main result has been the thorough survey and exploration of Northeastland with its remarkable ice sheet, which was accomplished by sledge and sea-plane journeys.

G. W. T.

Sydney University Reprints. Series VI (Geology and Geography). Vol. I, Nos. 1–20 (1924); Nos. 21–42 (1927). Vol. II, Nos. 1–11 (1929).

We have received three paper-bound volumes, as above detailed, of the geological and geographical papers published by members of the staff of Sydney University from about 1920 onwards. The University has published reprints since 1894 in bound volumes at intervals of every four years or so; but in 1922 a new scheme was adopted by which the scope of the reprints was extended to include papers written by workers in any Department, provided that they reached a certain standard. The title of the series was changed to *University Reprints*, and the papers were classified in thirteen series, of which geography and geology is the sixth.

The three volumes under review contain fifty-three papers by well-known Australasian research workers, including Prof. W. N. Benson, Prof. W. R. Browne, and Dr. G. D. Osborne, in petrology; Prof. Sir T. W. E. David, Prof. L. A. Cotton, and Prof. W. G. Woolnough, in dynamical and stratigraphical geology; and Dr. Taylor Griffith in geography. Naturally the greater number of the papers collected in these volumes are of local interest only, valuable as they undoubtedly are in assessing the advance of geological science in Australia. Prof. Cotton has, however, a paper of wider appeal in his Presidential Address to the Australasian Association for the Advancement of Science on "Causes of Diastrophism and their Status in Current

Geological Thought"; and Sir T. W. E. David also contributes a paper on his newly discovered fossils in the Adelaide Series, believed to be of Pre-Cambrian age, which have aroused world-wide interest.

G. W. T.

Structure and Surface. By C. BARRINGTON BROWN and F. DEBENHAM. [Pp. vii + 168, 104 figures, and Folding Map.] (London: E. Arnold & Co., 1929. Price 10*s.* 6*d.* net.)

THIS book deals with the relations between structural geology and geomorphology, the connection between geological structure and the surface of the ground, pictorially illustrated by means of block diagrams. This three-dimensional method of representation is the best yet devised, as it can show at one and the same time what happens at the surface and in depth, and the connection between the two. It may be said at once that the original illustrations which have been drawn from this point of view are very good and most instructive. The method utilised is to illustrate each of the simpler geological structures first by an idealised general block diagram, and then by an actual example taken from the one-inch geological maps of this country.

After an introductory chapter discussing stratigraphical and mapping points, and explaining the use of block diagrams, there are chapters on the general form of rock masses, measurements in the field (dip, strike, thickness), surface and outcrop, folds, faults, structures of igneous masses, surface relief, major land forms, minor land forms, the construction of block diagrams, and block diagrams of geological maps and structures. Three appendices provide notes on equipment and surveying instruments, the problem of apparent dip, and determination of differences in level. The treatment is concise, simple, and clear, and is made most lucid by a large number of excellent diagrams. A folding map at the end of the book gives a unique dissected block diagram of England and Wales showing the geological structures along four east and west lines. We are able to recommend this book strongly to students as the best on field geology and block diagrams that we know.

G. W. T.

Earth Flexures: their Geometry and their Representation and Analysis in Geological Section, with Special Reference to the Problem of Oil Finding. By H. G. BUSK, M.A., F.G.S. [Pp. vi + 106, 92 figures.] (Cambridge University Press, 1929. Price 12*s.* 6*d.* net.)

THIS book may be regarded as an attempt to systematise a part of the field of practical structural geology. As the author says in the Introduction, the subject is viewed from "the severely practical standpoint of a geologist in the field, whose object is to reproduce, as faithfully as possible from his mapped surface evidence, the form of an earth flexure along any given line of section." The accuracy possible is, of course, dependent on the degree of completeness of the field observations. The cost of testing and developing an oil field, for example, is so great that there is a strong demand for greater exactitude in geological methods of survey and section drawing, whereby the sites of wells and borings may be more accurately located, and time and money saved.

The book under review is intended to meet this demand; and while it assumes a greater knowledge of mathematics and geometry than the average geologist is likely to possess, no doubt this knowledge will in future form part of the equipment of oil geologists. The book sets out to show "how the information supplied from the geological map may be best applied, how geometrical methods for section drawing of folds may be used." The axial

surfaces of folds are graphically expressed by means of tangential circular arcs with the normals to the known dips as radii, and a considerable part of the book is devoted to the geometry of this procedure.

The book consists of six chapters of somewhat unequal lengths. After the introductory chapter comes one on geological process discussing sedimentation and folding. The third chapter deals with the geometrical construction of earth flexures in geological sections, the fourth with the axial plane, the fifth with geological mapping for accurate section drawing, and the sixth with flexures of Tertiary age in the petroliferous rocks of some extra-European countries. This final chapter contains some valuable discussions of oil-field structures in Burma and Persia, of rift-valley structure in Sinai, and of gypsum thrust-sheets in Persia showing their approximation to salt-dome structures. The book is illustrated by a number of excellent line drawings, and by stereographic structural projections.

G. W. T.

Sedimentary Petrography. With Special Reference to Petrographic Methods of Correlation of Strata and to Subsurface Oil Geology. By H. B. Milner, M.A., F.G.S. Second (Revised and Complete) Edition. [Pp. xxi + 514, 181 illustrations.] (London: T. Murby & Co., 1929. Price 21*s*. net.)

In this revised and complete edition the *Introduction to Sedimentary Petrography* published in 1922 and reprinted in 1927, and the *Supplement* published in 1926, are combined with a great deal of entirely new matter. The result is an imposing and valuable work which provides a comprehensive textbook of the petrography (*not* petrology as stated in the preface. *Vide* title) of sediments of both incoherent and consolidated character. The previous issues have dealt only with unconsolidated materials; hence the new chapter describing the consolidated sedimentary rocks is the chief departure from the original plan of the work. This chapter would be valuable, as also indeed the rest of the book, if only for the excellent microphotographs which illustrate and adorn it; but it is extremely well done in every way, although no attempt at interpretation is made. Indeed this would have been outside the scope of the work. Special value attaches to the descriptions of the less common sediments, such as nitrates, borates, sulphates, chlorides, phosphates, asphalts, and siliceous sinter, which are rarely dealt with, much less illustrated, in petrographical works.

Other new material is found in Chapters I–III, relating to samples, storage, and records. Chapters IV and V are new; the former being devoted to the petrographic microscope and its use in the examination of sediments. Chapter V deals with physical properties of minerals and rocks other than optical, and their description and measurement. Chapter VI, on the diagnostic properties of sedimentary rock minerals, is expanded and brought up to date. In these diagnoses the terms "high" and "low" for birefringence are applied rather indiscriminately. Thus dumortierite ($\gamma - \alpha = \cdot 011$) is recorded as of "high" birefringence, kyanite (·0119) as "low"; dolomite (·179) is stated to have "high" birefringence, but calcite (·172) "very high." The birefringence of tourmaline is misprinted 0·202.

The principles and practice of correlation and differentiation of sediments by petrographical methods are treated in Chapter VIII, and descriptive examples are given in Chapter IX. The bearing of sedimentary petrography on palæogeographical problems, and its application to the study of soils and other surface deposits, are dealt with in Chapters X and XI.

Numerous alterations and additions have been made to the illustrations, which have been doubled in number. A further word of praise must be given to these magnificent microphotographs and drawings, mostly the work of Mr. G. S. Sweeting, which must prove of the greatest value to students

and researchers. The ingenious detrital mineral determination tables given in Appendix I are one of the few examples of this kind of thing that really will help the student in his diagnoses.

We have to congratulate Mr. Milner on bringing to completion a remarkably valuable contribution to petrographical science.

G. W. T.

Deposition of the Sedimentary Rocks. By J. E. MARR, Sc.D., F.R.S. [Pp. vii + 245, 8 figures.] (Cambridge University Press, 1929. Price 7*s*. 6*d*. net.)

THIS book deals with sediments and sedimentation from a totally different angle from Mr. Milner's. It can be described as the study of sediments and sedimentation in relation to stratigraphy and palæogeography. Prof. Marr's mode of approach is purely geological, and he is thus much more concerned with the petrological than with the petrographical side of the subject. He has given a general account of the conditions which have controlled sedimentation in time and space, taking into account the stratigraphical and structural characters of the rocks, and the nature of their organic contents.

The first three chapters deal generally with physical conditions, chronology, correlation, and the uses of fossils. The general characters of deposits on land and sea are dealt with in Chapter IV, and land deposits are treated with more detail in Chapter V. The marine deposits, however, take up the remainder of the book. The various belts of sedimentation are delimited in Chapter VII, and the marine deposits of the "belt of variables" (Chapters VII–IX), the mud belt (Chapter X), and the organic belt (Chapter XI) are then successively described and interpreted, with examples drawn from Prof. Marr's wide field experience amongst the Palæozoic rocks of Britain. The book ends with chapters on climatic belts in the oceans, and on uniformity and evolution.

North-western Europe is exceptional in that epicontinental waters have repeatedly covered parts of its area, and that it has seldom, if ever, been the site of a real open ocean. Hence neither the Holocene period in time, nor north-western Europe in space, can be regarded as normal from the standpoint of conditions of sedimentation; and in dealing with sedimentation in the past, and in different parts of the world, Prof. Marr warns the British geologist against being too insular in his outlook.

This work deals with an aspect of the subject of sediments and sedimentation which is rarely treated in a comprehensive and consecutive manner in the textbooks. Hence Prof. Marr's work fills a gap in the literature. The book reads easily and pleasantly, and is extremely lucid notwithstanding a paucity of illustrations and a rather obscure statement regarding "zone fossils on slabs" (p. 37). "Orcadie" is misspelt on p. 167, but this is the only misprint detected by the reviewer.

G. W. T.

Elements of Mineralogy. By FRANK RUTLEY. Revised by H. H. READ, A.R.C.Sc., D.Sc., F.G.S. 22nd Edition. [Pp. xxii + 394, 84 figures.] (London: T. Murby & Co., 1929. Price 6*s*. net.)

THAT a scientific textbook should reach the extraordinary number of 22 editions is evidence, if evidence were needed, of its complete adaptation to the needs of students. Rutley's *Elements of Mineralogy* has reached this distinction; 18 editions were passed through before Rutley's death; and Dr. Read's very able revision of 1915 is therefore in its fourth edition, and has also experienced three reprintings. In the 1929 edition the book has been brought thoroughly up to date. Apart from minor corrections the

alterations made concern data and statistics relative to the minerals of economic importance. We admire Dr. Read's restraint in refusing to cumber this work with the useless determinative tables which figure in so many other textbooks on this subject.

G. W. T.

Dana's Manual of Mineralogy. By W. E. Ford. 14th Edition. [Pp. x + 476, 360 figures.] (New York: J. Wiley & Sons; London: Chapman & Hall, 1929. Price 20s. net.)

This is the almost exact American counterpart to Rutley's *Mineralogy*, and it has deservedly reached its 14th edition under the supervision of Prof. Ford. Sixteen years have elapsed since its last revision, but the changes that have been found necessary are inconsiderable. Short paragraphs have been added to bring the section on crystallography up to date, and in the descriptive section the paragraphs dealing with modes of origin and localities of occurrence have been thoroughly revised. This book is on a larger scale than Rutley's; it goes more into mineral occurrence and genesis, and devotes a long section to rocks and rock-forming minerals. It has not, however, rid itself of the superstition of determinative tables, which occupy 54 pages. The book is well printed and pleasingly got up in soft leather covers.

G. W. T.

Mineralogy: An Introduction to the Scientific Study of Minerals. By Sir Henry A. Miers, M.A., D.Sc., F.R.S. Second Edition revised by H. L. Bowman, M.A., D.Sc. [Pp. xx + 658, with 761 illustrations in the text.] (London: Macmillan, 1929. Price 30s. net.)

It is hardly possible to class the study of mineralogy among the more popular branches of scientific investigation, but there is little doubt that during the last twenty years or so the first edition of Miers' *Mineralogy* has achieved a considerable popularity among those whose interests were so inclined. It is quite unnecessary here to enlarge on it. It was certainly a stimulating textbook, and one in which the author had succeeded admirably in avoiding that stodginess which is apt to be associated with a subject so largely devoted to classification and description. In the second edition there has been a certain amount of enlargement—an increase from 584 to 658 pages, and from 716 to 761 illustrations—and a certain amount of revision, at the hands of Miers' successor at Oxford, Prof. H. L. Bowman, though as far as possible the original plan and words have been adhered to. This means, of course, that at the very least all the many good points of the first edition have been perpetuated. But actually it means more. Beginners in mineralogy will doubtless now value "Miers" more highly than ever.

Unfortunately, there is one source of regret; and that is that the second edition of so well-known a textbook should have little availed itself of the striking advances that the last decade has seen in the X-ray analysis of the *fine-structure* of minerals and crystals in general. It is true that a chapter on structure analysis has been included and the structures of a few minerals described, but one cannot help regretting that more emphasis was not laid on this aspect of the subject, that the structure of graphite which *is* given is out of date, and that all the recent work on the structures—truly beautiful structures—of such enormously important minerals as the silicates seems to have been sadly neglected. There was a time when crystallography was a mere hand-maiden of mineralogy, but now the servant has left the mistress far, far behind.

W. T. Astbury.

Tin: Its Mining, Production, Technology, and Applications. By C. L. Mantell, Ph.D. [Pp. 366, with 91 figures.] (New York: The Chemical Catalog Co., 1929. Price $7.)

A monograph on the subject of tin is a rare event even in these days of the making of many technical treatises. In the last quarter of a century only about three works exclusively devoted to tin have been written in English. In all the accounts of this metal the portion dealing with smelting is inadequate, or out of date, or both. Prof. Henry Louis, about twenty years ago, in his preface to *Metallurgy of Tin*, pointed out that this was due to two reasons: first, the unprogressive character of tin smelting; and second, the profound degree of secrecy observed by tin smelters.

The book under review is planned to cover the subject in a comprehensive manner, information from many sources having been gathered together. It opens with a brief chapter on the history of the metal which is followed by chapters on its various properties, on the ores and ore deposits, and on methods of mining and ore dressing. The various branches of metallurgy are dealt with in several chapters. Chapter VI, entitled "Smelting and Metallurgy," either implies a very restricted meaning to the term metallurgy, or suggests that metallurgy does not include smelting. This chapter, owing to the small amount of information available, lacks detail and in parts is somewhat out of date. The value of oil-firing and the use of regenerators are not clearly brought out. Furnaces with the old water vault are referred to, but there is no mention of the method adopted in some of the modern furnaces of collecting tin which leaks through the furnace hearth. The Pulo Brani flow-sheet in the matter of the weights of the charge constituents does not appear to be modern. The charges evidently refer to small furnaces. The influence of tungstic oxide on the smelting process, and also that its presence in the concentrates is penalised, seemed to have been overlooked. Apparently there is no mention of "grain" tin.

A chapter is devoted to recoverable or secondary tin, also the experimental work on gaseous reduction of stannic oxide and of tin ores, and the application of electrolytic methods to refining are discussed. There is, however, no reference to the Sulman Picard process of extraction by hydrochloric acid gas. It will prove of general interest to have an account of American practice, although the works were closed about seven years ago. The various applications of the metal, which include plating, tin-plate, foil, collapsible tubes, and the compounds of tin are all considered; while another section deals with corrosion, including the corrosion of tin-plate by food products. A long chapter of about seventy pages is given to alloys, but this, in spite of its length, is in some respects disappointing. The information is not well proportioned in accordance with the relative values of the different alloys discussed. There is a useful account of the properties of the chief industrial alloys which occupies about ten pages, but a fuller treatment of these alloys would have been an advantage, particularly in a book of this type. The next forty-five pages present the equilibrium diagrams of the binary systems from tin-aluminium to tin-zirconium, but no diagrams of the ternary alloys are given. The list of compositions of various tin alloys would have been of greater value if in certain cases the principal characteristic had also been briefly noted. The melting-points of all the fusible alloys would have been useful. Some photomicrographs could have been included in this chapter with advantage. The final chapter of the volume adequately describes the chemical reactions of tin and its salts, as well as various methods of quantitative analysis of tin ores. It is a pity that space could not have been found to make the chapter complete by an account of the methods of analysis of the market forms of the metal and the commoner alloys. The chief criticism against the book as a whole is that it tends to be unbalanced in its treatment of the subject. Particulars which many would consider to be of secondary

value are in some cases given more attention than those which are of chief importance industrially.

The work is one of the well-known series of monographs produced under the direction of the American Chemical Society and the American National Research Council. These monographs have a double purpose. The first is to make accessible the present knowledge of a subject in a readable form, and the second, to promote research by presenting a foundation which can form a basis for further investigations. The subject has been considered from many points of view, and a large amount of information has been collected and rendered available in a convenient form. The book is undoubtedly a useful addition to the literature on metallurgy.

E. COURTMAN.

BOTANY

The Structure and Life of Forest Trees. By Dr. M. BÜSGEN. Third Edition by Dr. E. MÜNCH. English Translation by T. THOMPSON. [Pp. xii + 436, with 173 figures.] (London: Chapman & Hall, 1929. Price 30*s*. net.)

THIS work is something between a textbook of botany and a handbook of forestry. For the student of the former subject it will be found a useful and interesting introduction to the extensive field of research in forestry with which the botanist is too often but inadequately acquainted. For the forester it provides an account of the more botanical aspects of his subject, and although it will hardly be found to dispense with the necessity of consulting more formal texts, the purely botanical facts are presented in a way which should arouse the interest of the technologist.

The translation has suffered in places from a too rigid adherence to the German authors' phraseology, but, in general, the translator's task has been well fulfilled and the misprints are comparatively few.

The frequent footnotes giving bibliographical references are a valuable feature and there is a separate index of authors' names. The subject index is decidedly inadequate and from it all plant names are omitted. In the general text plants are most frequently referred to by their popular names alone, a defect that is remedied in part by a glossary of popular names with their Latin binomial equivalents. Unfortunately this latter is incomplete.

By the general reader the phraseology will be found not too technical and the mode of presentation and the breadth of field covered should alike ensure the appeal of Prof. Büsgen's book to the many who are interested in the liaison between pure science and technology.

E. J. S.

Index Kewensis plantarum phanerogamarum. Supplementum Septimum. By A. W. HILL and the Staff of the Kew Herbarium. Roy. quarto. [Pp. iii + 260.] (Oxford Clarendon Press, 1929. 75*s*. net.)

THIS seventh supplement to this indispensable work of reference brings the index to the end of 1925 and such other names are included as were omitted from the supplements I–VI. When it is realised that the present work contains some thirty thousand names with their accompanying citations, the immense amount of labour involved in its compilation will be appreciated.

It is interesting to note that both in respect to the number of genera involved and the number of new species in a genus the Orchidaceæ figure most prominently, the number of additional names in such genera as *Bulbophyllum*, *Epidendrum*, and *Pleurothallis* being exceptionally large. In the Compositæ *Hieracium* is outstanding with more additional names than any other genus in the whole work, numbering approximately a thousand. Other conspicuous genera are *Quercus*, *Eugenia*, *Dioscorea*, *Psychotria*

(Rubiaceæ), *Mesembryanthemum*, *Piper* and *Peperomia*, *Drypetes*, *Neomamillaria*, *Rhododendron*, *Solanum*, and *Viola*, each with probably more than a hundred new binomials.

With such a constant and overwhelming increase in nomenclature the necessity for such an index becomes increasingly emphasised, and a tribute is due to those whose painstaking and laborious task does so much to lighten the burden of new names for the taxonomist.

E. J. S.

Recent Advances in Plant Physiology. By E. C. BARTON-WRIGHT, M.Sc. (Lond.), Lecturer in Botany in the University of London, King's College. With a foreword by Prof. R. RUGGLES GATES, M.A., Ph.D., Professor of Botany in the University of London, King's College. [Pp. xii + 352, with 51 illustrations.] (London: J. & A. Churchill, 1930. Price 12*s*. 6*d*. net.)

IT is probable that several teachers of plant physiology have considered, during recent years, the idea of supplementing the older standard works on this subject, but, on realising the size of the undertaking, have turned away to other things. Therefore a debt of gratitude will be felt to Mr. Barton-Wright by those who approve of his selection of topics and their treatment in this admirable series of *Recent Advances*, published by Messrs. Churchill.

The author has set out to write a book for Honours students, and so assumes a background given by the usual introductory courses to plant physiology—although at times this is forgotten, as, for example, in the final paragraph on page 115 and the opening to Chapter VI. He therefore deals chiefly with the advances which have been made between 1920–28. He has arbitrarily selected the following subject heads: soil science; the absorption by, movement in, and elimination from plants of water and substances in solution; carbon assimilation, nitrogen metabolism, and respiration as chapters in metabolism; the physiology of reproduction, germination, and growth. That even within these limits about 500 references are cited in the text shows the magnitude of the task attempted; and, moreover, the author states that he has discussed only the important fundamental papers, and those which have already, either directly or indirectly, had an economic bearing—for he considers that progress in plant physiology will depend upon its connection with agriculture and with industry.

In the reviewer's opinion Mr. Barton-Wright has succeeded in producing a very useful work. And although at times the sequence of treatment might possibly be improved—for example, the notice of the work of Luyten, Joustra, and Blaauw seems out of place in its present setting on page 285—it is surprising that continuity has been so well maintained where so much has been included. It is possible, however, that space might profitably have been gained for the fuller treatment of more significant matter, by considerably condensing the accounts given on pages 161–5 and 192–6, and elsewhere, of chemical speculations of which the author rightly and strongly expresses his disapproval—the section on the chemistry of humus is an exception, as this is spiced with the author's own work and is therefore acceptable.

If a second edition of the book is called for, it is suggested that the following points, which have emerged from a first reading of the book, might well be considered. Is "need" a physiological factor (p. 115, l. 11)? Is not formaldomedon the correct description of the compound between formaldehyde and dimedon, and is not the M.P. the critical physical constant used to distinguish it from other aldomedons (p. 166, l. 10)? Is not "settle" rather too strong (p. 167, l. 6)? In view of the fact that the author supports the formaldehyde hypothesis of photosynthesis, even although free formaldehyde is not found in green cells, is he justified in rejecting the hydrogen cyanide hypothesis of protein synthesis simply because hydrogen cyanide has not

been found in plants (p. 190, l. 17)? Should not "maxima" be substituted for "modes" on page 205, line 1? On page 222, line 14, would not "frequent" be more in accord with the facts than "main"? Should not the opening paragraph on page 230 be rewritten to distinguish more clearly between the dehydrase enzyme, which activates the hydrogen donator, and the hydrogen acceptor—which is certainly not an enzyme—and is not "enzyme" written on line 31 in mistake for "hydrogen"? Since the dehydrases are not inactivated by potassium cyanide, would not "many" be preferable to "all" on page 232, line 24? Finally, the following correct forms of words should be substituted for the misprints in the book: zymasis (p. 228, l. 19), guaiacum (p. 242), de-amination (p. 248, l. 21), endosperm (p. 250, l. 28); and the letter "c" is displaced in line 15 on page 316.

That there may be other minor examples of insufficient consideration in a book of such ambitious range need not deter teachers from recommending it to senior students as a suitable guide to the recent literature of the subject. Moreover, members of the teaching profession, whether biologists, physicists, or chemists, who wish to acquaint themselves with the chief modern trends in plant physiology, will be well advised to buy this book.

M. THOMAS.

The Application of Science to Crop Production. By A. HOWARD, C.I.E., M.A., and G. L. C. HOWARD, M.A. [Pp. iv + 81.] (Oxford: at the University Press, 1929. Price 9s. net.)

TWENTY years' exploration of the best ways in which botanical science could be most profitably applied to the growth of crops in India led both authors to the conclusion that the dissociation of practical agriculture from the separate sciences connected therewith was by no means ideal, even though both aspects were dealt with in a single research institute. As early as 1919 it was suggested that a separate institute should be founded for the study of crop-production along the broadest lines, the aim being to study the development of the plant as a biological whole. Five years later, in 1924, the project came to fruition, and a Research Institute for Crops was founded at Indore, under the direct control of the Agricultural Adviser, and managed by a board of Governors representing the Indian Central Cotton Committee, and the States contributing to the finances. Thus the Institute is run on Western lines, as it was erected and is financed entirely by subscriptions, and is controlled by the subscribers. The main objects are to carry on fundamental research on cotton, to train post-graduate students, and to provide an agricultural centre for the States of Central India and Rajputana at which cultivators and officers can be trained.

A suitable site for the Institute was found at Indore, in Central India, which fulfilled certain necessary conditions, as possessing varied types of soil with ample supplies of irrigation water, having a good supply of labour available, and being near a town to provide easy transport for crops and visitors, and also social amenities for the staff. Much care was devoted to the lay-out of the land, drainage and roads were provided for, and a systematic scheme adopted to bring the fields into order for experimental work. The laboratories and library were carefully planned, and the provision of a small lending library and arrangements for the sale of standard books have proved very popular. The buildings necessary for housing part of the staff have been put up as a model village, and the capital cost of the institute as a whole was £23,538.

Cotton is the chief crop under investigation, the programme including the investigation of fundamental questions applicable to the whole of India, the improvement of varieties grown under dry and irrigated conditions, and general improvements in methods of cultivation. In the latter connection it is emphasised that it is the weight of the cotton fibre produced on every acre

of land which is the matter of chief importance, and which is limited by the conditions under which the cotton plant grows. The eradication of perennial weed grasses is of vital importance, in that they always seriously reduce the cotton crop, and in unfavourable years they may spread so rapidly as to prevent the land being cultivated at all.

The control of the monsoon rainfall by an efficient system of surface drainage means a great increase in crop, as from 145 lb. to 510 lb. per acre of cotton and 370 lb. to 1,005 lb. per acre of wheat. Other experiments deal with various means of increasing the nitrogen supply available for crops, the methods used being as simple as possible and within the scope of the ordinary cultivator.

Much scope exists for improvement and extension of well irrigation, particularly in certain areas where the growth of the wild date (*Phœnix sylvestris roxb.*) indicates the presence of extensive subterranean water-bearing areas. The methods used at present are defective in many ways, and efforts are being made to persuade cultivators to rectify these errors. Other work of the Institute deals with the improvement of cattle and the sale of implements and machines, a special trading account having been opened for the latter purpose.

Care is taken to maintain a close liaison between the Institute and its supporters. Officers and students are accepted for training. Special meetings of ordinary cultivators are arranged, and general visitors are encouraged, with the result that already definite progress is being recorded from almost every Contributing State.

The detailed and lucid account of the growth and aims of the Institute concludes with a comprehensive list of papers illustrating the scope of crop production, and with a copy of the "Memorandum of Association and Rules of the Institute of Plant Industry, Indore."

W. E. Brenchley.

ZOOLOGY

The Sceptical Biologist. By Joseph Needham. [Pp. 288.] (London: Chatto & Windus, 1929. Price 7*s.* 6*d.* net.)

This is a collection of ten essays which have already been published in various journals. The themes chiefly dealt with are, first, the heuristic failure of vitalism and the necessity of a form of mechanism recommended by the author under the name of "Neo-mechanism." The discussion of these topics occupies some four or five of the essays. The second chief theme is the relation between natural science and religion, to which three essays are devoted. The author's method of dealing with this difficulty is by what he calls the "subjectivation" of mechanism. His "Neo-mechanism" is mechanism with the sting removed, since its assertions are not to be interpreted metaphysically. Mr. Needham believes that Scientific Naturalism has been finally laid to rest by James Ward, Mach, Alliota, Boutroux, Bergson, Whitehead and Eddington. But he also proposes to extend the same "subjectivation" to the other possible views, so that he is prepared to carry his scepticism beyond some of these authors. Mr. Needham seems to be chiefly interested in ensuring a fair hearing to all human activities as against setting one up in a position of superiority over the others. From this point of view the last essay—"Materialism and Religion"—is admirable. But it is not at all clear how far the author realises the consequences of his scepticism if it is to be taken seriously as an epistemological theory, and he does not discuss it from this point of view. He quotes Vaihinger with approval, but Vaihinger's book is already out of date in many ways, and Mr. Needham does not mention the modern realist movement. A thorough-going scepticism is easy enough to defend in the atmosphere of the study, but difficult to live up to in practice; because no one is able to withstand

the insistent demands of what Santayana calls "animal faith" with its perpetual challenge to the sceptic to re-examine his arguments and to cease taking himself seriously. Mr. Needham does not tackle the difficult problem of finding a mean between the simple-minded metaphysics and crude realism of the nineteenth century on the one hand, and an equally simple and too easy scepticism on the other. The remaining essays are historical, and deal with "S. T. Coleridge as a Philosophic Biologist," "Julien de la Mettrie," and "William Harvey and the Witches."

J. H. W.

Problems of Instinct and Intelligence. By MAJOR R. W. C. HINGSTON, M.C. [Pp. vii + 296, with 37 illustrations.] (London: Edward Arnold & Co., 1928. Price 10s. 6d. net.)

THIS book is devoted to a detailed study of animal behaviour—chiefly that of ants, wasps, and spiders. Eight chapters deal with the manifestations of "instinct," and six are devoted to "intelligence." There are also chapters on "Insect Memory," "The Unknown Sense," "Mental Evolution," and a final chapter by way of summary, discussing the rôles of instinct and intelligence in the above animals. The data described have for the most part been collected by the author himself during some seventeen years of observation in the East, but the observations of others are also discussed. Major Hingston does not share the view of the behaviourist school, that psychological concepts should be avoided in describing and interpreting animal behaviour. The book is good to read and well illustrated by line drawings. It may be warmly recommended to all interested in animal behaviour.

J. H. W.

Experiments in Bird Migration. I. Manipulation of the Reproductive Cycle: Seasonal Histological Changes in the Gonads. By WILLIAM ROWAN. *Proceedings of the Boston Society of Natural History*, Vol. 39. [Pp. 57, with 11 plates.] (Boston, 1929.)

THIS latest publication, by Prof. W. Rowan, of the University of Alberta, gives the first complete account of his remarkable experiments on bird migration from 1924 until 1928. A consideration of existing facts and opinions had led him to expect that some endocrine organ was involved in the onset of migration, and he then proceeded to investigate the possible external factors upon which changes in the endocrine organs might depend. In opposition to a common opinion, Prof. Rowan conceived the ingenious idea that the principal external factor was not temperature, but length of day. He accordingly devised experiments for testing this hypothesis by illuminating birds with powerful electric lights in open-air aviaries exposed to the full rigour of the Canadian winter, the bird chosen being chiefly the American sparrow—*Junco hyemalis*. The result has been definitely to disprove the view that the annual rhythm of the reproductive organs in birds (at least as far as this species is concerned) is dependent upon changing temperatures. When a lamp of sufficient intensity was employed and applied for a suitable period each night a definite enlargement of the testes and ovaries in the experimental birds, as compared with those of controls, was observed. The author himself says: "In the case of the junco, the 1927 Experiment 1 results leave no room for doubt that low temperatures have no direct effect, since a mean temperature of $-3°$ F., through the month of December, failed to inhibit the recrudescence induced by means of artificial illumination, while an entire week with an average temperature of about $-23°$ F. synchronised with the actual period of most rapid development."

Further experiments were undertaken to discover the precise way in which increased illumination was involved in this recrudescence of the gonads.

Different degrees of illumination showed that what was essential was that the birds should be prevented from going to sleep at night until later than the usual time. The possibility of a direct effect of ultra-violet light was excluded by the type of lamp used. Accordingly, Prof. Rowan devised a mechanical apparatus for keeping the birds moving in a dimly lighted room for a period after their usual roosting time. The result suggested that it is the increase in time of exercise that is involved, not the actual illumination, the latter only serving, if it is strong enough, to keep the birds active after dark. The experimental birds were found to be extremely restless, and sang incessantly, while the controls remained silent. In the words of the author: " By remote control from the experimental aviary the canaries [some of which were included in the experiments in addition to the juncos] were broadcast by the University Radio Station in its Christmas Programme at 9.30 at night with the thermometer exactly at zero. Via the loud-speakers of numerous homes throughout the Province, and beyond it, they started scores of other canaries singing in rooms sufficiently well lit to keep the birds entirely awake and lively, a reflection on the difference between adequate and inadequate lighting, even in spite of a temperature difference of some 70° in favour of those badly lit."

An account is also given of the result of releasing experimental and control birds, and also a preliminary account of histological examinations of the gonads. As might be expected, from the complexities of the situation in the first case and the difficulties of endocrinology (especially of making inferences from histological data) in the second case, these results are not yet so clear-cut as the former ones. But the testes of the experimental animals show a striking increase in size correlated with a complete recrudescence of spermatogenesis up to the formation of typical clusters of spermatozoa. A comparison of these with the testes of wild birds taken in the middle of May shows that the former " are normal in all respects except in the matter of date."

Whatever may be the outcome of his further experiments, Prof. Rowan has succeeded in these ingenious and laborious experiments in showing that it is possible to interrupt the rhythm of the reproductive organs in the junco by appropriate manipulation of the lighting conditions, and has thus illuminated a hitherto obscure corner of biology.

J. H. W.

The Internal Secretions of the Ovary. By A. S. Parkes, M.A., Ph.D., D.Sc. [Pp. xv + 242, with 11 tables and 69 figs.] (London: Longmans, Green & Co., 1929. Price 21*s*. net.)

This is an addition to the well-known Monographs on Physiology, edited by C. Lovatt Evans and A. V. Hill. The aim of the series is to present the progress in those lines of physiology which recent work has carried well beyond the summaries given in textbooks, and the volume here considered well deserves inclusion in the series. The questions related to the secretions of the ovary are in themselves important, and have been the subject of numerous recent attacks from all sorts of angles. So many investigators are engaged on the subject that it is extremely difficult to keep abreast of the rapidly growing literature unless actively working in the same field. The author has performed a most useful service in bringing the scattered information together within reasonable compass and putting it forward in a well-arranged, consecutive and clearly written form. The book is printed upon loaded paper, which enables the photomicrographs to be reproduced without loss.

The subject-matter is naturally treated from the physiological point of view, but it is perhaps a pity that the morphological aspect is not more in evidence. For example, on page 13 we read, "certain skin glands have undergone alteration to produce the mammary tissue," and two pages later,

"The exact nature of the secretion which takes place in the mammæ is still a subject of controversy." These statements hardly represent a summary of what is known on these two subjects, and, strictly speaking, is there any secretion in the mammæ? On page 27 the author states that species closely related zoologically often differ widely in their reproductive cycles, and quotes as examples the rat and the rabbit. These two animals, however, belong one to each of the two major divisions of the largest mammalian order, which are differentiated *inter alia* by striking differences in the male reproductive organs.

These are but minor flaws in an otherwise excellent work, and one which should be in the possession of all who are interested in this much-discussed field of study, whether they are scientists or medical men.

C. H. O'D.

Comparative Neurology. By JAMES W. PAPEZ. [Pp. xxv + 544, with 315 illustrations.] (New York: Thomas Y. Crowell Co., 1929. Price $6.)

THE aim of this book is indicated in its sub-title, which states that it is "An introductory laboratory course on the form, function, and structural interpretation of the nervous system," and it is intended for the use of students of biology, physiology, and psychology. From many points of view the functional morphology of the brain is becoming increasingly important, and this volume certainly provides a useful introduction to that complex subject. The many clear diagrams are of considerable assistance. It is divided into three portions. The first part deals with the gross structure of the brain in a series of mammalian types, including Ornithorhynchus, Echidna, Opossum, Rabbit, Cat, Dog, Lemur, Cebus, Baboon, Gorilla, Orang, Gibbon, and young human. The second part deals with the microscopic structure of the mammalian nervous system in a fair amount of detail. The third and shortest deals with the brain in the lower Chordata—Reptiles, Birds, Amphibia, Fish, Cyclostomes, and Amphroxus. While these are not treated so fully as the mammalian brain, they form a very satisfactory group through which the gradually increasing complexity of the Chordate brain can be traced, and the complex of primitive centres and subsequently acquired structures in the mammal can be understood. The third part also contains much useful information on the forms treated. The author is to be congratulated on producing a serviceable and informative work.

C. H. O'D.

Our Face from Fish to Man. A Portrait Gallery of our Ancient Ancestors and Kinsfolk, together with a Concise History of our best Features. By WILLIAM K. GREGORY. [Pp. l + 295, with a frontispiece and 118 text-figures.] (London: G. P. Putnam's Sons, 1929. Price 18*s.* net.)

IT is possible to present the findings of comparative anatomy, even complicated ones, in a popular form so that the ideas are conveyed accurately, and yet so free from technicalities that they may be followed readily by the interested lay reader. This may be done also, and the interest retained without sacrificing dignity to trivialities as is exemplified in the writings of Huxley and Lankester. At times we feel that the writer of the present volume has transgressed in this respect. In several places he speaks of "basic patents," relating to anatomical structures or relations, in a way that obscures rather than clarifies, and the following quotation furnishes another example of the same tendency. "How much arrogance, deceit, and wickedness would have been spared the world, if man had realised that even the most imposing human faces are but made over fish traps, concealed behind a smiling mask,

but still set with sharp teeth inherited from ferocious premammalian forbears."

The task before the author, that of giving a popular account of the complicated series of changes in the bony skeleton and in the soft parts of the head and of those in the sense organs throughout the vertebrata, until they reach their evolutionary culmination in man himself, is an intricate and difficult one. On the whole he has achieved a remarkable success. Moreover, by means of an ingenious device he has met the needs of two different classes of readers. Numerous very clear and instructive diagrams are introduced throughout the book and provided with simple explanations. While a certain number of technicalities cannot be avoided in the discussion of a subject of this nature they are reduced to a minimum, well chosen, and explained so that they should offer no difficulty to the serious reader. This then meets the needs of the layman. The more advanced reader will find, on pp. xiii–xl, more detailed and technical explanations of the text figures, and moreover at the end a bibliography that provides the justification for the facts upon which the author bases his arguments and also gives access to the extensive relevant literature which is widely scattered. There is also a full index. Altogether it is a book that serves a useful purpose, and one well worthy of a place on the bookshelf of anyone interested in comparative anatomy.

C. H. O'D.

Wanderings in Wild Australia. By Sir BALDWIN SPENCER, K.C.M.G., F.R.S., etc. [Two volumes. Pp. xxviii + 930, with 8 coloured plates, 2 folding maps, 281 illustrations on plates and numerous text-figures.] (London: Macmillan & Co., Ltd., 1928. Price 42*s*. net.)

AT various times throughout a quarter of a century the well-known author of these delightful volumes penetrated unfrequented parts in the interior of Australia. In the first trips he "humped his swag," later used camels, and finally made the first motor trip across the wild north lands. He started as a biologist, although always with an interest in ethnology, but as time went on he became more and more interested in the fast-disappearing natives. The preliminary chapters give an account of the interior of Australia from the geographical, geological, and biological points of view, and to anyone wishing to get an idea of this little-known and contradictorily reported region no more clear and straightforward account can be recommended. Throughout the description, the biologist keeps peeping out, and the adaptations of the plants and animals to their strenuous environmental conditions are interestingly treated. It has been stated that amphibia do not drink, yet here we find frogs, some of which are illustrated in coloured plates, who swallow water, store it in their bladders, and in this way tide over one or even two years of severe drought and heat.

The greater part of the two volumes is devoted to the natives, and this is in several respects unique. The author had the happy knack of getting on the best of terms with the natives and with those white men who understood and were trusted by them. He was also for some time "Chief Protector of Aborigines in the Northern Territory." He and his colleagues were the first and in a number of cases the only whites to see the renowned native ceremonies. The rapid change in methods of transport and other conditions of living have fundamentally altered the lives and customs of the aborigines, and many of the ceremonies are falling into the limbo of forgotten things. Indeed, the author in 1895 witnessed a great succession of ceremonies in the Arunta tribe, termed the Engwura, which lasted over four months and has never been repeated since. Not merely was Spencer especially privileged and observant, but few men would have seized the opportunities as he did, nor could they have approached and written on these topics in such a sympathetic manner.

As he points out, the Australian native possesses a peculiar interest, because he lived the life of a wandering nomad in much the same stage of culture as our own Stone Age forbears. In some respects they are remarkably deficient, as for example in counting where three is two and one, and five is two and two and one, and then they stop. In other matters, such as the memory of places, events, details of ceremonies, tracking, etc., they possess conspicuous ability.

The whole book furnishes most fascinating reading, as well as a vast amount of valuable information. The illustrating is lavish and very well executed. Sadly prophetic are the last words, " Two days later we were back in Darwin and my wanderings in Australia's Great Lone Land were over." Shortly after the book appeared, Spencer died on an island off Tierra del Fuego, whither he had gone to study another group of primitive peoples, and Australia lost one of her most brilliant scientists, her most outstanding recorder of the fast-vanishing native customs, and the world lost a great and wise man.

C. H. O.'D.

More Gleanings from Nature's Fields. By W. P. PYCRAFT. [Pp. xiv + 203 with 39 Plates.] (London: Methuen & Co., 1929. Price 7*s*. 6*d*. net.)

MR. PYCRAFT is known to a wide circle of readers through his weekly essays in the *Illustrated London News*. Forty of these, after a certain amount of revision and addition, have been gathered together in the present volume, and provided with an index. As the title suggests, it is the successor of a similar collection. The nature and limitations of the essays can be appreciated by the general reader, and the revision might well have deleted some of the frequent references to the obvious fact that a brief essay cannot deal fully with most of the subjects. Certain slips might also have been removed, *e.g.* on no human pelvic bone can a line 86 ins. long be drawn (p. 199); no group of beetles is named *Lamelliconia* (p. 153).

The essays themselves are written in a pleasing style, and the illustrations on the whole are excellent. The range of subjects is very wide, including animals and plants, sea anemones and prehistoric man, organisms of the sea, sea-shore, wood and garden, and touches many parts of the world. The variety is a result of the casual sources of inspiration; new specimens of quaint beings, seasonal phenomena, new publications, popular queries, and so on, and the very diversity adds charm to the whole. It can be dipped into here and there with the certainty of entertainment, and more continuous reading does not bore on account of the changes. The ingenuity of the author is nowhere better illustrated than in the chapter on spider crabs which had its origin in a learned and systematic treatise on that group. While the memoir is of considerable value to the specialist, this chapter shows that it contains much of interest to the general public.

All the essays are provocative—as they should be—for they are not simply descriptive nor written in such a manner as to suggest that now the oracle has spoken there is nothing more to be said. Rather does the author go out of his way to indicate the unsolved riddles the subjects raise and their bearing upon controversial questions. Mr. Pycraft has his own views on several of the latter, which are not shared by all his brother scientists, and this is a good thing, for it would be a dull world if we all agreed about everything, and it is folly to pretend to a unanimity of opinion in scientific matters where such does not exist. The book will be welcomed by Mr. Pycraft's regular readers, and should give pleasure to a wider circle who will find much interest and recreation in its pages.

C. H. O'D.

The Whip Snakes and Races. Genera Masticophis and Coluber. By ARTHUR IRVING ORTENBURGER. [Pp. xviii + 247, with 36 plates and 64 text-figures.] (Ann Arbour : University of Michigan, 1928. Price $6.)

IN the authoritative list of the Amphibia and Reptilia of North America, issued by Stejneger and Barbour in 1917, eleven species and varieties of snakes are included in the genus Coluber, some of which had previously been referred to other genera. It included *Coluber constrictor flaviventris*, the blue racer, and *C. flagellum flagellum*, the coachwhip snake, two obviously different forms. The present book is a detailed study of 1,718 specimens, including most of the types, and was undertaken with a view of trying to clarify our ideas of the genus. The result is that two different groups have been identified and are sufficiently distinct to merit placing in separate genera : Coluber, the racers, and Masticophis, the whipsnakes. The former contains two species of four varieties, and the latter twelve species, one of which, *M. ruthveni*, is new, of fifteen varieties.

The different varieties are all fully examined and described, and their inter-relationships discussed. In each genus also keys are given which enable the different forms to be identified. The book is fully illustrated with tables of scale numbers, maps of geographical distribution, and a wonderful collection of photographs of the whole animals, and of parts to show colour pattern or scale arrangement. All of the photographs are good, and some of them are the best photographs of snakes we have seen. The author is to be congratulated upon a useful, painstaking, and thoroughly well-executed piece of work.

C. H. O'D.

Genetics of Brassica. By EDMUND MALINOWSKI. [Pp. 1–26.] (Price F. 1·60.)

The Genetics of the Tettigidae (Grouse Locusts). By ROBERT K. NABOURS. [Pp. 27–104.] (Price F. 4·40.)

Self-Sterility. By E. M. EAST. [Pp. 331–70.] (Price F. 2.)

Vererbungserscheinungen bei Pilzen. By H. KNIEP. [Pp. 371–478.] (Price F. 5·60.)

Offprints from *Bibliographia Genetica*, Vol. V. The Hague: Martinus Nijhoff, 1929.

THE word "genetics" is not defined in the *New English Dictionary*. The study of heredity is not a new science, but it is only within recent years that it has become specialised enough to need a name of its own. In that time institutes of genetics have been founded in several countries and an elaborate descriptive and experimental technique has been evolved. A number of journals are now devoted solely to publishing the results of genetical researches.

The author of the first of these papers works in the Institute of Genetics at Skierniewice, Poland. He discusses investigations which have been carried out concerning the species *Brassica oleracea*, which includes the common culinary cabbages. The second is a detailed report dealing with breeding experiments with a species of locust carried out principally in the Kansas Agricultural Experiment Station, Manhattan. The third, emanating from Harvard, is an historical account of the phenomenon of "self-sterility," *i.e.* the existence of hermaphroditic animals and plants in which the union of the male and female gametes is difficult or even impossible. Darwin is credited with being the first to publish a systematic discussion of this subject. The fourth paper is a lengthy account of theories of inheritance

in their application to fungi. These studies are essentially technical ones and they can claim to be of both scientific and economic importance. Each has a comprehensive bibliography appended and three of them are also provided with a detailed index. The indispensable diagrams and plates are excellently reproduced. β_2.

Animal Psychology for Biologists. By J. A. Bierens de Haan. [Pp. 80.] (London: University of London Press. Price 4*s*. 6*d*. net.)

In May 1928, Dr. Bierens de Haan, Lecturer on Experimental Zoology in the University of Amsterdam, was invited to give three lectures at King's College, London. These lectures have been published under the above title.

The author does not belong to that school of scientists who regard the scientific method as a process of collecting and arranging facts, and looking for an hypothesis to fit them. Like Darwin, he knows the facts to look for, and his book shows an attitude of mind arrived at from an unconscious background of knowledge—intuitive belief almost, the truth of which he patiently investigates by controlled experiment.

His psychological views are on the whole very sound, although he does not refer to some of the more recent psychology which has established the views. Freud's work nor W. B. Cannon's work, for example, is not mentioned. And we are doubtful if biologists will gain much by worrying themselves about the three aspects of mental processes.

Nevertheless, the book is excellent. He has an eye for the essentials in the work of Lloyd Morgan, McDougall, and Thorndike. His own experiments form an important contribution, and leave fresh difficulties for the mechanists to face.

One conclusion which he draws we feel to be unjustified. He found that a hungry octopus would immediately seize a crab on the bottom of the tank, but not a crab suspended by a thread. When the crab was brought near to the octopus, it directed its siphon on the crab and tried to blow it away. "This observation proves," he says, "that a hungry octopus, ready to jump at any crab that appears before its eyes, does not recognise the crab in the unknown situation of hanging in the water, although the object itself is very well perceived" . . . "This not recognising the crab is just as curious as would be the case of a man who was in the habit of feeding on apples, but who should recognise this fruit only when lying before him on the table, not when hanging on the tree."

Possibly, but it might be analogous to a man looking with some concern at apples with no apparent support, floating between the table and the ceiling. The hungry dog which saw a bone moving mysteriously towards him ran for his life, thereby showing wisdom rather than faulty perception.

Dr. Bierens de Haan's work will help to place the study of Zoology on a wider basis. Cannon's work has established definitely that the mechanism of the animal body is altered by mental processes, and biologists cannot hope to avoid bias in some of their work unless they understand and allow for that factor.

J. C. Hill.

Heredity in Man. By R. Ruggles Gates, Ph.D., LL.D., F.L.S., F.R.A.I. [Pp. 385.] (London: Constable & Co., 1929. Price 24*s*. net.)

On setting out to review this volume we consider first for whom it is written. If it be intended to provide a warning to the lay reader of the possible results of unwise parentage, he will find many examples to alarm him and perhaps to induce caution; he will find moreover a number of interesting illustrations. If, however, the volume is intended as a guide to the scientist, we cannot recommend it, indeed we feel that we ought to speak much more strongly.

The book covers a vast field of inquiry and its author could not be expected to have first-hand knowledge covering its scope, but most assuredly he should be in a position to satisfy himself of the accuracy of statements before he makes them and to have some power of critical analysis which he can apply to material he makes use of. The following quotations will, we think, justify these observations:

Page 160. "The frequency of syndactyly is probably not more than 1 : 1,000, and of polydactyly slightly higher." On what are these estimates based? We are not aware of any determination of these frequencies from an adequate sample of the general population, including both sexes.

Page 258. "Pearl publishes a pedigree of human cancer in eight members of a family in three generations. This is 196 times the frequency in the population." What is the meaning of this statement?

Page 257. Speaking of neurofibromatosis the author writes: "This condition is rare, being found in only one in 2,000 cases." He does not state cases of what, but the section is concerned with the inheritance of cancer; it is not known to us that neurofibromatosis has any connection with cancer, though occasionally sarcomatous changes may take place in these as in any other fibrous tumours.

Page 258. "Strong (1926) cites a cancer family from Worthing, with a pedigree of cases in six generations. He concludes that the inheritance is recessive, but it could equally be explained as a dominant."

Page 258. "It was long ago pointed out that there is an antagonism between tuberculosis and cancer." This may or may not be a fact, but no evidence on the question is given; it requires to be proven and is no easy problem when the age incidence of the two diseases and the early tuberculosis death-rate are considered.

Page 35. Davenport is quoted as saying: "The progeny of two short parents are more variable than those of two talls" and that "regression of the filial stature towards mediocrity does not occur when the parents are of great stature, but is markedly present when the parents are short." Professor Gates accepts these to our belief entirely erroneous statements without apparently any critical examination as to the means by which such conclusions were reached.

Page 56. "There is greater variability between the offspring of fleshy parents than of slender parents." Surely a very doubtful statement requiring the detailed evidence on which it is based.

Page 84. A pedigree is here given of "*female* sex-linked colour-blindness" and the author has little difficulty in fitting it into his Mendelian preconceptions regarding the inheritance of colour-blindness. This pedigree, however, is believed by ophthalmologists not to refer to colour-blindness at all but to a primary defect of the lens which leads to a secondary anomaly in colour vision analogous with the vision obtained by looking through tinted glass—it is in no sense comparable with the male sex-limited congenital colour-blindness.

Page 79. With reference to hereditary optic atrophy and its mode of transmission the author writes: "Much unfruitful discussion has been based on this supposed inability of affected men to transmit their defect. Apart from its improbability on biological grounds, the idea appears to have grown up because hæmophilic sons usually die before they are old enough to reproduce." This is quite erroneous—the example of hæmophilia has always been recognised as a special case because of its heavy fatality; anyone who has examined and compared long series of pedigrees of colour-blindness and of hereditary optic atrophy will realise that the mechanism of transmission differs markedly in the two cases, and that though Lossen's law probably does not apply rigidly to any disease, yet an approach to it is exhibited by hereditary optic atrophy.

These quotations were noted in the process of looking through the volume in the search for a section which we could heartily commend; we can only conclude that they are characteristic of the volume as a whole.

In writing this review it is not the author's industry which is complained of, for he has accumulated an enormous mass of facts, but rather that his critical faculty has been in abeyance throughout, not alone with regard to his own statements, but also with reference to the work of others.

β_{19}.

Contributions to the Principles of Morphology. By W. B. Crow. [Pp. viii + 94.] (London: Kegan Paul, Trench, Trübner & Co., Ltd., 1929. Price 5s. net.)

Biological Principles. A Critical Study. By J. H. Woodger. [Pp. xii + 498.] (London: Kegan Paul, Trench, Trübner & Co., Ltd., 1929. Price 21s. net.)

Both these books deal with the theoretical aspects of biology, and in that sense both are philosophical. They are concerned entirely with the analysis and criticism of the theoretical principles and concepts of biology and, in the case of Mr. Woodger's book, with the more important theoretical conclusions, but not with the data or the technique of observation. Most biologists will admit the importance of thoughtful criticism of the foundations of their science directed to stimulating clear thinking and clear exposition. Moreover such criticism may remove many of those apparent incongruities in biological thought which have arisen chiefly because of the increasing specialisation in certain aspects of biology. The increasing isolation from each other of these different branches tends to loosen the whole foundation of the subject. Reconsolidation is urgently necessary and can be brought about only by widening the fundamental principles which underlie the various branches. The importance of the books under review lies in the fact that they are critiques of these theoretical foundations. Mr. Crow's book, as the title states, deals only with the principles of morphology. Its aim is to examine and to analyse these from a critical point of view and to provide a basis for theoretical discussion of the ordinary laws of structure and development. The subject is introduced by an account of the nature of morphological thought. The author is much influenced throughout by Goethe, to whose views a good deal of space is devoted. Subsequent chapters deal with the more important concepts of morphology, such as individuality, homology, symmetry and proportion, classification and type, and relationship by descent. The whole book is very clearly written and easy to read. It is as concise as is possible because reference to the various applications of the principles under discussion is avoided. The author is to be congratulated on writing so much that is important in so small a space and in being philosophical without being "long-winded."

The scope of Mr. Woodger's book is much greater, for it deals with the principles underlying the whole of biology. It is intended to perform the same function for biology that Boyle's *Sceptical Chemist* did for chemistry. Mr. Woodger's philosophy is much influenced by the Cambridge school of thought, and especially by the views of Professor Whitehead and Dr. Broad, but where his views are original he does not hesitate to express them forcibly. The scope of the book is outlined in the Introduction and the ground is prepared for the subsequent treatment of the problems involved. It is divided into two parts, of which the first is devoted to the theory of knowledge and to the problems involved in its application to the results of any branch of natural science. Part two deals with some of the more fundamental difficulties peculiar to biological knowledge. The antitheses between "Vitalism and Mechanism," "Structure and Function," "Organism and

Environment," "Preformation and Epigenesis," "Teleology and Causation," "Mind and Body" are discussed, among other problems, and the underlying principles are critically analysed. The final chapter is devoted to the future of biology. The chief defect of the book is that it is too long, because the various principles involved are so copiously illustrated with biological examples that the reader tends to be overwhelmed. It is, however, a well-written and profound work, and deserves a wide reading for the beneficial effect it should have in stimulating and clarifying modern biological thought.

F. W. R. B.

Outlines of Zoology. By J. ARTHUR THOMSON. 8th edition. (Pp. xxviii + 972, with 528 illustrations.] (Oxford University Press, 1929. Price 21*s*. net.)

THE first edition of this book was published in 1892. During the thirty-seven years that have passed since then seven new editions have appeared at approximately equal intervals. It is scarcely necessary to comment that these bare facts afford more evidence of the utility and general excellence of the book than can be derived from the views of any individual reviewer. The author of this review has had, in the past, a special regard for the *Outlines of Zoology*, perhaps because it was the medium through which he was introduced to that science. It is satisfactory that the latest edition does not differ in form or arrangement from its immediate predecessors, but many new figures and passages of text have been added with a view to bringing it into line with the more recent developments. The most noteworthy change is the additional space which has been devoted to the physiology of the different groups of animals. The morphology and embryology of certain important types, the Dipnoi among them, which did not receive sufficient attention in previous editions, have been dealt with more fully. The book provides the student with a compact, accurate and remarkably complete survey of the whole animal kingdom. It is characterised by the immense amount of information, covering systematically all the chief groups of living animals, which is contained in one small volume. No other textbook of zoology of the same scope and size can rival it in this respect.

F. W. R. B.

Locusts and Grasshoppers. A Handbook for their Study and Control. By B. P. UVAROV. [Pp. xii + 352, with 118 figures and 9 plates.] (London : The Imperial Bureau of Entomology, 1928. Price 21*s*. net.)

THE ruinous devastation of crops which follows in the wake of a horde of locusts or grasshoppers is a spectacle that is not readily forgotten by those who have witnessed it. Recurrent outbreaks in every continent of the world have time and again necessitated the expenditure of huge sums of money by various Governments to cope with the menace, but despite all the work that has been accomplished—as witnessed by the extensive literature on the subject—the problem of control still awaits solution. This the author attributes to the passivity of responsible authorities in the intervals between outbreaks. The whole question is then shelved at a time when active prosecution of research into the bionomics and behaviour of locusts might establish clues to more effective methods of combating them.

Mr. Uvarov's book is a perfect compendium of locust lore and should be indispensable to all entomologists and administrative officers engaged on the locust problem. Drawing upon his own wide experience, the author outlines the type of organisation that is necessary to carry out an effective campaign against the pest. A trained entomologist should be in charge, and the actual work of control in the field should be carried out by paid

labour. Nothing in the book impresses us more than the vast gaps in our knowledge of locusts. These can only be bridged by further continuous and intensive research of all aspects of the problem in specially equipped laboratories, where it can be tackled not only nationally but internationally. A plea is also entered for the enlistment of the services of the "pure" biologist in university laboratories, who would find in the systematics, anatomy, physiology, and genetics of the Acrididæ much interesting material for investigation, all of which might finally yield results of use in questions of control.

The author's outstanding contribution to the locust problem is his theory of phases, which helps us to understand the process of periodic fluctuations of locusts but does not explain its actual causes. These can only be revealed by an intensive study of the physical and biological factors of their environment. According to this theory each gregarious species of locust exists in two phases, one of which is a typical swarming locust (*gregaria*), the other a solitary grasshopper (*solitaria*). These extreme forms are morphologically and biologically distinct, but are connected by a continuous series of intermediate forms (*transiens*).

The book is divided into two parts, the first of which is general and deals with the anatomy and physiology of locusts and grasshoppers, their natural enemies and control. As regards the last it is important to note that poison-baits have practically superseded all other methods for destroying locusts, and this too although our knowledge of them is almost wholly empirical. The second part of the book is devoted to discussion, in individual chapters, of the known gregarious locusts, including descriptions of their different stages, distribution and ecology, bionomics and control. Solitary grasshoppers are treated separately in three chapters and there is a final chapter of general conclusions. A most valuable bibliography is added.

A. E. CAMERON.

Agricultural Entomology. By D. H. ROBINSON, B.Sc., and S. G. JARY, B.A. [Pp. xi + 314, with 149 illustrations.] (London: Duckworth, 1929. Price 15*s.* net.)

THERE is scarcely a phase of human activity that is not adversely affected by the depredations of insect pests. In agricultural practice alone, insects exact each year such a toll of every known cultivated crop as to seriously deplete the returns of the harassed farmer. Since the prevailing high costs of labour merely serve to aggravate the burden of insect damage, it is more than ever incumbent upon the agriculturist to employ every approved method of protecting both his crops and live-stock against potential losses. Whilst in the last fifty years our knowledge of artificial and biological methods of insect control has made very satisfactory progress, it is not improbable that in the future economic entomologists will achieve an even greater measure of success in controlling insects by concentrating upon the problems of insect ecology—the interrelations of the insect and its physical and biological environments—with a view to finding the causes of insect outbreaks and forestalling their occurrence. In other words, prevention will be stressed rather than cure.

The book before us is welcome for two reasons: first, because the British farmer will now have a thoroughly modern account of insects of economic importance in this country; and second, because the agricultural student of our universities and colleges will be furnished with an adequate introduction to a subject the study of which will demand more and more of his attention. In the first part of the book the authors have succeeded in presenting a clear and concise exposition of the essentials of anatomy, metamorphosis, and classification by way of making the second part on specific insect pests and

their control more readily comprehensible to the reader. The examples of pests have been well chosen and conveniently arranged under their various orders. Where in any particular instance insecticides cannot be profitably employed in combating a crop pest, good results are frequently obtained by a variation of the system of rotation in vogue, or by the adaptation of some existing cultural method. In the two final chapters the authors discuss the principles of insect control and the composition and application of insecticides. Arthropoda of economic importance other than insects, and the eelworms, which injure crops such as wheat, strawberries, potatoes, and sugar-beets, are dealt with in two appendices. The illustrations, which are mainly line drawings and photographs, are on the whole satisfactory.

The text is comparatively free of errors. On page 55 it is, however, incorrectly stated that human lice assist in the spread of plague. In the legend of figure 129 the larva of *Oestrus ovis* is erroneously said to occur in the brain, whereas it is depicted in the *frontal sinus* ! The larva of the corn sawfly, *Cephus pygmaeus* (p. 171), does not *completely* sever the straw preparatory to retreating into its hibernating cell in the subterranean part of the stem ; and the method of burning the stubble, which the authors advocate, to control this pest is practically worthless, as has been amply demonstrated in the case of the North American allied species, *C. occidentalis*, of similar habits. Comparisons are often odious, and certainly to suggest that *Gastrophilus equi* resembles *Oestrus ovis* (p. 239) is, to say the least, far-fetched. Anyone who has had experience of horse botflies will fail to agree with the opinion (p. 241) that "horses take little or no notice of them"; and very few will be found to subscribe to the statement (p. 210) that gadflies (*Tabanidae*), despite their popular name, cause cattle and horses to stampede. One can, however, perhaps discountenance these errors in a book, which will commend itself to a wide circle of practical agriculturists, who desire an acquaintance with the fundamentals of the subject without being immersed too deeply in technicalities.

A. E. Cameron.

The Mosquitoes of North America. By Prof. Robert Matheson. [Pp. xvii + 268, with 23 figures and 25 plates.] (London: Baillière, Tindall & Cox. Price 25*s*. net.)

The great importance of the Culicidæ in the transmission of malaria, yellow fever, filariasis, and dengue has in the last thirty years stimulated intensive investigation of the family by entomologists and public health authorities in every part of the habitable world. The result has been that around the Culicidæ there has accumulated a volume of literature with which the specialist at times has found it hard to cope; and as research is now being more actively pursued than ever before, there is no reason to suppose that there will be any immediate decrease in quantity. The great bulk of the work has for its aim the eradication or reduction of mosquitoes, but at the same time the systematic aspect is not being neglected. In Europe the studies of Edwards, Wesenberg-Lund, and Martini, in America those of Howard, Knab, and Dyar, have done much to clear up many doubtful questions concerning species and their distribution. Gradually the tangle of a cumbersome synonymy is being unravelled, and order is being introduced into a family which, until very recently, presented almost insurmountable difficulties to the average student of the Culicidæ. With the accumulation of more exact information concerning mosquito taxonomy and habits, the tendency to multiply species indiscriminately has been superseded by more accurate comparative studies, as a result of which, for instance, it has been shown that many of our palæarctic and nearctic forms are identical.

In North America the problems of mosquito control may be classified according to the chief breeding habitats. First there are the salt marshes

and permanent fresh-water swamps, the former of which are responsible for the swarms of migratory mosquitoes that infest the sea-boards of New Jersey and California. Here there flourish *Ædes sollicitans*, *A. cantator*, and *A. tæniorhynchus*. Secondly, in the surface accumulations of water remaining after the subsidence of floods in river valleys, *A. vexans* and *A. aldrichi* breed abundantly. Thirdly, in the temporary ponds of the prairie enormous broods of *A. lutescens*, *A. dorsalis*, and *A. spenceri* are reared annually subsequent to the melting of the snow in the spring. For the control of mosquitoes of salt marshes and swamps drainage schemes must be undertaken; for river valleys subject to inundation, dykes must be constructed, whilst for temporary ponds oiling and poisoning will ameliorate conditions in the vicinity of human habitations, in cases where filling might prove too expensive. The chief factors which determine the mode of control and its extent in any district are the density of the population, the degree of interference with human activities by mosquitoes, and the deterioration of value of otherwise productive lands. To-day the question of malaria scarcely touches upon mosquito control in North America. In the northern United States the disease has almost disappeared, and it is also less prevalent than heretofore in the southern States. This diminution is apparently correlated with the agricultural development of the country, which has probably led to the reduction of the chief vectors, *Anopheles quadrimaculatus*, *A. crucians*, and *A. maculipennis*.

The book before us will prove of the utmost utility to the student of North American Culicidæ. The facts of mosquito morphology and biology are clearly explained and well illustrated. The relation of mosquitoes to disease is discussed and the various methods of control are recounted. On the question of the efficacy of *Chara* and *Phyllotria* as control agents the author is non-committal. Very little information is available on the food of mosquito larvæ. Pending the results of more extensive investigation of the physical, chemical, and biological factors of breeding waters, no satisfactory explanation can be offered for the variation in the species of Culicidæ that may inhabit apparently identical ponds.

The systematic treatment of the Anophelini and Culicini occupies about two-thirds of the book, and the identification of the eighty-one species described is rendered comparatively simple by the provision of excellent tables. By no means the least valuable part of the book are the carefully executed text-figures and plate illustrations.

A. E. Cameron.

Textbook of Zoology. By H. G. Wells, B.Sc., and A. M. Davies, D.Sc. Revised by J. T. Cunningham, M.A., and W. A. Leigh Sharpe, M.Sc. Seventh edition, 1929. [Pp. viii + 559, with 227 figures.] (London: University Tutorial Press. Price 8*s*. 6*d*.)

Not many readers of H. G. Wells's more recent masterpieces probably know that many years ago he contributed to that much-used series of textbooks issued under the ægis of the University Correspondence College, which is primarily designed to assist students to pass their examinations. In the intervening years biology has undergone so complete a transformation, and the present edition retains so little of the original matter, that Mr. Wells can bear little if any responsibility for the book which still bears his name.

We have no doubt that for the purpose for which it is intended, viz. "for the Intermediate Examination in Science of the University of London," this volume will be found helpful. For the more general reader, its exclusive and frank devotion to the needs of the "cram book" renders it of little interest in comparison, shall we say, with so fascinating and entertaining a manual as Haldane and Huxley's *Animal Biology*, a work which covers a not altogether dissimilar ground. Wells, Davies, Cunningham, and Leigh Sharpe suffer particularly in the comparison when one searches—and in

vain—for allusions to some of the more fundamental discoveries of recent years. Perhaps it is the syllabuses of examinations which are to blame for the tendency to teach intermediate students what may be described as "advanced elementary biology," to the exclusion of "elements of advanced biology," or, in other words, detailed and descriptive treatment of the older knowledge instead of a simpler account of the more significant of the recent advances in the science.

On page 13 we learn that foodstuffs are made up of four classes of compounds, carbohydrates, fats, minerals, and proteins, and their properties are described. *Vitamins* are, however, nowhere so much as mentioned, although, as we know, the main constituents of foods are useless to maintain life in their absence. There is a discussion of the pancreas and its function, but insulin finds no place in it. Perhaps it is old-fashioned reticence which permits a fairly long discussion of reproduction (in the rabbit) without any allusion to coitus.

L. J. H.

MEDICINE

Textbook of Comparative Physiology. By Charles Gardner Rogers, Ph.D., Sc.D. [Pp. xvi + 635, with illustrations.] (London: McGraw-Hill Publishing Co., Ltd., 1927. Price 27*s*. 6*d*. net.)

This volume is an outcome of a course in comparative physiology which has been offered to the students in Oberlin College, Ohio. The course was designed to develop a broader view of physiology than can be learned from the study of mammalian physiology alone. Physiology, because it is most often worked at in departments connected with medical schools, has tended to be taught almost exclusively as the physiology of the highest organisms-mammals, including and specially directed towards the study of man. This has perhaps been the more understandable, since in the protozoa, for instance, the simplest animals so far as complexity of structure is concerned, all the vital processes are carried out by the same general cell substance. This makes the study of the functions (of which is the aim of physiology) exceedingly difficult. It is easier to study them in the metazoa and particularly so in those highly developed forms where specialisation of function has reached its highest level.

Nevertheless, in spite of this concern with human physiology, it is exceedingly important for those studying the subject to have definite ideas of the unity of life and of animal functions. This volume contains an extraordinary amount of information, helping towards such conceptions, arranged on the basis of the functions of the living body as manifested in all sorts of animal organisms from the simplest to the most highly organised, and is full of interesting details.

In the chapters devoted to the study of respiration there is a good discussion of the factors influencing the supply of oxygen to animals living in the water, and the economic consequences of these, in connection with fish life and the disposal of sewage refuse from a great city such as London. The influence of vegetable life and of light and air on the supplies of oxygen in water is clearly brought out, forming an excellent example of the dependence of different forms of life upon one another.

In a volume as packed with knowledge as this, it is quite impossible to deal with the matter in detail. It only remains to be said that professional students of physiology and of the morphological sciences will find it a mine of stimulating and useful information. Further, that the author has such a clear style and is so sparing of unexplained technical terms that it will also appeal to those who are interested in learning some of the many marvels manifested in the living body, and of following out their evolutionary development.

W. C. Cullis.

Fever, Heat Regulation, Climate, and the Thyroid-Adrenal Apparatus. By W. CRAMER, Ph.D., D.Sc., M.R.C.S. [Pp. ix + 153, with 40 plates and 11 figures.] (London: Longmans, Green & Co., Ltd., 1928. Price 15*s*. net.)

DR. CRAMER, in this extremely interesting book, gives a concise and summarised account of the conclusions he has reached as a result of his study of the secretion of adrenalin, and the conditions which affect it. For this he has made use of the histo-chemical method which he himself devised, and which renders visible the specific secretory granules of adrenalin in the cells of the adrenal medulla.

The thesis he maintains is that by the co-ordinated action of the thyroid and suprarenal organs the body is provided with a chemical means of heat regulation, and that stimulation of this apparatus by certain bacterial toxins, or by a chemically well-defined substance, produces fever.

His explanation, he claims, accounts very satisfactorily and on an objective basis for the physiological and psychological effects of climate. "A 'bracing' climate is one which stimulates the sympathetic, and the thyroid and adrenal glands, a 'relaxing' climate one which fails to stimulate them." In a final chapter on "Climate and the Human Apparatus," he gives explanations on this basis for many well-known reactions to different climates. Exposure to cold, accompanied by insufficient clothing and inadequate heating, causes an intense stimulation of the thyroid-adrenal apparatus, which exhausts it and leads to a consequential mental and physical deterioration. At the other extreme is the lack of stimulus which must be a feature of tropical climates. This will be followed by a reduced rate of metabolism, a weakened resistance to infection, and a great physical lassitude.

The ideal climate from the view of human efficiency, in terms of physical and mental activity, is one with a mean temperature lying between 50° and 60° F., and one too in which there are the considerable fluctuations which he has shown to have a stimulating effect on this apparatus. The ideal for human efficiency does not necessarily coincide with the ideal for human enjoyment.

All the same, it is good to realise that our own unjustly disparaged climate comes nearest to the ideal! Japan, New Zealand, and the south-east parts of Australia also approach the ideal, and parts of the American continent, fortunately from the British point of view, including considerable areas in Canada, provide such a climate.

Altogether, with its admirable plates, an exceedingly readable book, which makes a very good case for regarding the Thyroid-Adrenal Apparatus as playing an important part in the heat regulation of the body.

W. C. CULLIS.

Laboratory Outlines in Comparative Physiology. By C. G. ROGERS, Ph.D., Sc.D. [Pp. 130.] (London: McGraw-Hill Publishing Co., Ltd., 1929. Price 7*s*. 6*d*. net.)

THIS book gives an account of experiments designed to accompany the course of lectures whose matter was largely used in the volume *Comparative Physiology*, reviewed above. It contains, in addition to much that would be done in any ordinary physiological laboratory course, some experiments on the physical chemistry of cells and the general physiological properties of protoplasm, for which an ordinary course allows little time. Also the more usual experiments on function are sometimes extended to other organisms than those ordinarily in use.

The outlines suggest a very useful course for students of what might be called General Physiology, such as might well be carried out in schools and other institutions, where the subject of physiology is being taught more

from its general educational angle than from its specialist professional one. It is much to be desired that such teaching should enter more and more into the curriculum of the schools for the ordinary girl or boy, who is not going to the universities, even at the cost of its replacing some part of the work as at present arranged.

W. C. CULLIS.

The Extra Pharmacopœia of Martindale and Westcott. Revised by W. HARRISON MARTINDALE, Ph.D., Ph.Ch., F.C.S. Vol. II. Nineteenth Edition. [Pp. xxxviii + 759.] (London: H. K. Lewis & Co., Ltd., 1929. Price 22*s.* 6*d.* net.)

THE first volume of the corresponding edition of this work was reviewed in *Science Progress* (vol. xxiii. p. 550). The present volume forms an almost indispensable companion to the former, though it is quite capable of standing on its own merits. While the first volume is concerned mostly with treatment by drugs and chemicals, the second is concerned with matters of diagnosis, analysis, and assay of materia medica, and many other subjects too numerous to be included in the first. The great variety of subjects dealt with can best be appreciated by a glance at the table of contents, which includes articles on subjects so widely divergent as Isotopes, the Structure of the Atom, Bread and Flour Control, Radium Emanation, Transmutation of Hydrogen, Iontophoresis, Radiology, Gas Poisoning, Information regarding the Imperial Cancer Research Fund, and a Report on the International Conference on Cancer held in London in 1928. The information, needless to say, is up-to-date and reliable, and is supported as usual by a large number of references. A reader can reckon on gaining some new information each time he refers to the book.

P. H.

MISCELLANEOUS

Weather: Practical Dramatic and Spectacular Facts about a little-studied Subject. By E. E. FREE and TRAVIS HOKE. [Pp. xi + 337, with numerous illustrations.] (London: Constable & Co., 1929. Price 14*s.* net.)

AMERICAN weather is subject to frequent and disconcertingly large variations, and, in the tornado, to a type of storm that normally destroys everything that lies in its path. In New York warm winds from the Gulf of Mexico may take the temperature well above 60° in mid-winter, and a cold-wave from Canada causes it to fall nearly to zero (Fahrenheit) within two days. The general public there can hardly fail to take a passing interest in the causes of these violent changes, that sometimes bring financial losses amounting to hundreds of thousands of dollars. From this curiosity arises a demand for pseudo-scientific books of the type under review. The word "pseudo-scientific" is not used to imply that the explanations are necessarily inaccurate, but that owing to the necessity for not trying the patience of the unscientific reader, the arguments have had to be condensed and simplified with misleading results. The vanity of the reader is flattered, and he is apt to think that he understands a difficult subject when in fact he has not even got so far as realising that it is difficult. Meteorology is cheapened by the publication of such books, and the possibility that meteorological research may be adequately endowed in a democratic country like America must tend to be reduced when works of real value such as Humphreys' *Physics of the Atmosphere* are swamped by a flood of cheap productions of this kind, which give the impression that any schoolboy can master the subject in a few weeks.

E. V. NEWNHAM.

Subject-Index to Periodicals, 1928. [Pp. viii + 651.] (London: The Library Association, 1930. Price £3 10s.)

ANOTHER volume of the *Subject Index to Periodicals* has appeared in 1930, and covers matter which has been published in 1928. Our last review on page 726 of the April Number of SCIENCE PROGRESS this year was written by J. Wilks on the volume which covered matter for 1927.

The present volume possesses the same *format* as the previous ones, and will be of value to many scientists and, of course, to all librarians. The mass of printed matter has become so enormous that one is tempted to suggest that some means ought to be found for limiting the constantly increasing flood. At this rate the number of publications a hundred years hence will be immeasurable; and, as most of them do not contain much that is either valuable or novel, the world will probably not be a loser if the number is reduced. Of course, such remarks do not apply to the compilers of these volumes, who have evidently carried out a most laborious task, which we hope will be useful to all public libraries and the students there.

R. R.

From the Seen to the Unseen. By JOHN H. BEST, B.Sc. (Lond.), Vicar of Little Marlow, Bucks. [Pp. 548.] (London: Longmans, Green & Co., 1929.)

THE author of this volume writes: "If it be true . . . that the Universe proceeds from God, that it is indeed His handiwork, then it would seem almost inconceivable that it should not in numberless ways bear unmistakable and emphatic witness to Him." He then proceeds with great courage and industry to seek for this unmistakable and emphatic witness in the facts of nature.

The work is divided into seven main sections, the first of which is devoted to such physical considerations as the characteristics of Life and Energy; the second describes physiological systems; the third is concerned with embryology and the development of the individual; the fourth section discusses such psychological problems as the nature of memory and instinct; the fifth section is on evolution; there follows an astronomical section in which facts are given relating to the extent and structure of the universe and the possibilities of life on other spheres than our own; the seventh section is concerned with ethical and religious problems.

The volume is written primarily not for "the scientifically expert, but for those rather whose early education and subsequent reading have led them along very different lines," and as will be judged from the width of the survey the book cannot have much appeal to the scientific expert in any of the fields reviewed. The purpose of the author is kept in the forefront throughout; he is ever seeking to point out the mysteries of nature and the purposiveness of all developmental processes, human and otherwise; in this purposiveness he sees the evidence of supernatural direction. The reader must decide for himself whether or no he finds the author's arguments convincing.

No scientific expert would dare to attempt a description covering so wide a range of subjects as is included in this volume, but the book could be safely recommended to certain readers. The intelligent working-man who has not a ready access to the facts of life would find much to interest him, and the older schoolboy might learn much and be stimulated to wider reading. We are all aware that a little knowledge may be dangerous, but though the unwary may on some questions be misled by only being told a part of the truth, the cautious reader will readily realise the limitations which must accompany so wide a survey. We shall all agree that the popularisation of science should remain the work of the scientific expert,

but we must remember that the purpose of this volume is not the popularisation of science, and the clergyman who had the courage to set forth on so arduous a task as this author set himself is to be congratulated ; he at any rate read widely and spared himself no efforts to present facts with accuracy. The text is illustrated by many very good diagrams.

β_{10}.

Graphs and Statistics. A Suggestion for a Finishing Course in Mathematics. By JOHN MACLEAN, M.A., B.Sc. [Pp. xiii + 200.] (Bombay: The Tutorial Press, 1926. Price Rs. 4.)

Graphs and Statistics was written with the purpose of providing mathematical students with a wider survey than that provided by most academic degree courses. It claims to be suggestive only, and the colloquial style in which it is written is very different from that of the textbook. "In this book you see mathematics as a servant, a humble servant, of the other sciences. You get only glimpses of her queenly glory." The wide range of matters dealt with includes typical graphs such as parabolic, exponential, and periodic curves, the elements of the infinitesimal calculus, which seems rather out of place here, the use of the slide rule, nomograms, frequency distributions, and their constants and correlation. Examples are given of the application of such methods to meteorological, economic, physiological, and medical data, as well as to statics and dynamics.

β_2.

Elementary Applications of Statistical Method. By H. BANISTER, B.Sc., Ph.D. [Pp. 56.] (London and Glasgow: Blackie & Son, Ltd., 1929. Price 3*s*. 6*d*. net.)

THIS little book does not claim to be a work on statistics. "Its sole aim," we are told in the preface, "is to enable the many, who have perforce to deal with statistical data without having any aptitude for statistical analysis, to understand their results more fully." The topics dealt with in order are "goodness of fit," the mean and mode, measures of dispersion, the standard error of the mean and correlation. The use of modern statistical methods by students who have had no training in the subject is a fertile source of gross errors. It might be difficult to find a more flagrant one than the student might be led from this book to believe in, namely that a correlation co-efficient based on seven cases can be a useful result.

β_2.

Routledge Introductions to Modern Knowledge. (1929. Price 6*d*. net each.) No. 2. From Monkey to Man. By L. H. DUDLEY BUXTON. [Pp. 1–78.] No. 8. What Darwin really said. Connected Extracts from the "Origin of Species." With an introduction by JULIAN HUXLEY. [Pp. 1–80.] No. 11. The "Will to Work." By G. H. MILES. [Pp. 1–80.]

BOOKS of the size of these can hardly claim to be popular summaries of the important subjects with which they deal. At their best they can only be introductions which may arouse curiosity and stimulate more extended reading. In the short space allotted to him Mr. Dudley Buxton attempts to give an outline of the vast amount of evidence bearing on the relationships of man and other primates since Darwin affirmed that they had a common origin. Fossil human and anthropoid skeletons are principally dealt with.

In an introductory chapter, Prof. Julian Huxley explains that Darwin's main thesis is still accepted, and he indicates briefly, there and in footnotes, the ways in which some parts of it have had to be modified and extended by later research. Disconnected extracts follow, but, however well chosen,

it may be doubted whether they can give a fair impression to anyone entirely unacquainted with biology. They will serve a useful purpose, however, if they do no more than show how suitable for the general reader such books as "The Origin of Species" and "The Descent of Man" are.

As Assistant Director of the National Institute of Industrial Psychology, Dr. Miles is fully qualified to describe the most recent applications of experimental psychology. He stresses the need for vocational guidance and for the study of working conditions in office, factory, and workshop. Some idea is given of the methods of research employed and of some of the results already achieved. It is claimed that both the employer and the employee have been greatly benefited by these means.

By reading fifty books such as these one might rapidly survey a large part of the field of "modern knowledge." But it would be as superficially acquired as a person's knowledge of a country which he has only seen by flying over it. If the same time were expended in reading more advanced books on a single subject, the reader would gain both a clearer conception of scientific method and a more useful start in at least one branch of science.

β_1.

Colour and Colour Theories. By CHRISTINE LADD-FRANKLIN. [Pp. xv + 287.] (London: Kegan Paul, Trench, Trübner & Co., Ltd., 1929. Price 12*s*. 6*d*. net.)

THIS book is made up of a collection of papers, articles, reviews, etc., published at different times between the years 1892 and 1926. They are concerned chiefly with the criticism of colour theories in general, and the presentation of the author's own theory in particular. In addition, many of the facts of colour vision are described, and there is some discussion of what the author calls the "practical logic" of colour theories and of theories in general. There is an appendix containing five short articles on similar topics by other writers.

It is perhaps unfair to criticise such a collection as incoherent and containing excessive repetition, but it possesses both these qualities in a high degree. It is, in consequence, a wearisome book to read as a whole, although many of the articles, taken by themselves, are good. The tone of most of them is controversial, as one would expect in the presentation of a theory that has not yet received the attention it deserves, but too often the controversy tends only to obscure the issue for the general reader. For the specialist much of it would be unnecessary.

The theories themselves, in their essentials (and little more is given in most of these articles), are not difficult to grasp. Apart from Dr. Ladd-Franklin's theory, the Young-Helmholtz theory and the theory of Hering are the two of most importance. The Young-Helmholtz theory bases itself on the physical facts of colour mixture, and postulates three primary colour sensations, red, green, and blue, by the various combinations of which all the other colours or colour blends can be produced, and also the sensation white. It is essentially a physical theory, and neglects the fact that some sensations, *e.g.* that of white, are unitary, while others, *e.g.* purple, can easily be recognised as containing two components. Hering's theory, on the other hand, starts by recognising the fact of six unitary sensations, red, yellow, green, blue, black, and white, and postulates the presence of light-sensitive substances in the retina, which, according as they are broken down or built up by the action of the light, produce the sensations of green-red, blue-yellow, black-white. The two members of each pair are said to be complementary, but, in spite of modifications and additions, the theory offers no adequate explanation why the combinations green-red and blue-yellow are mutually destructive and produce the sensations

of yellow and grey respectively, yet black and white are capable of producing a true blend in which both components can be recognised. Dr. Ladd-Franklin's theory, like Hering's, recognises that there are four primary colour sensations, red, yellow, green, and blue, and the sensations black and white, and explains their production in the following way. Primitively there is a light-sensitive substance in the retina which dissociates under the influence of light, and produces the sensation of white or grey. This is the normal condition of the peripheral parts of the human retina, and of vision in a very dim light when the eye has become dark-adapted. It is the only kind of vision in some cases of complete colour blindness. A step in development occurs when this light-sensitive substance becomes complicated in such a way that it dissociates differently according to the wave-length of the light which strikes it. If the light is of long wave-length the particular dissociation produced causes the sensation yellow; if of short wave-length, blue. When both long and short wave-lengths arrive together the original dissociation results and the sensation white is produced. This again is the normal type of vision for the middle zone of the human retina, and in some kinds of partial colour blindness. A further step is taken when the dissociation produced by the light of long wave-length becomes differentiated into two kinds, that produced by light of the longest wave-length, which causes the sensation red, and that by a light of medium wave-length, which causes the sensation green. When these two kinds of light act together the two dissociation products combine and cause the sensation yellow, just as it was caused in the less differentiated stage. This is the normal vision of the central part of the retina in the human eye.

This is, of course, a very bare outline of the theory. It does appear to cover most of the facts and also to avoid most of the objections to the Young-Helmholtz and Hering theories. It has the further advantage that it is reasonable both from a psychological and from an evolutionary point of view. For further details and discussion Dr. Ladd-Franklin's collected papers must be referred to, and whatever criticisms may be made, there is no doubt that many matters of great interest will be found among them.

F. Goldby.

An Account of the Principles of Measurement and Calculation. By Norman Robert Campbell, Sc.D. [Pp. x + 293.] (London: Longmans, Green & Co., Ltd., 1928. Price 15s. net.)

The subject with which Dr. Campbell deals is one of great importance to all whose work involves metrical science. It is unfortunate therefore that it should be such a difficult book to read; for although it is addressed to experimental physicists and not to mathematicians, it is full of abstract generalisations which, in order to be at all clear, would require many more particular examples in illustration than are given. Another unsatisfactory thing about this book is the large number of statements which have to be accepted without proof. This reaches a climax in the chapter on approximate laws, where various rules are given for determining degrees of correlation: the author says that "no theoretical justification of them will be offered"—they are rules that he has somehow found to be useful in practice. An apology in the preface does not prevent this procedure from being very unconvincing. It is much to be hoped that when a second edition of this book is called for, Dr. Campbell will not hesitate to amplify it with additional examples and proofs.

The first four chapters deal with the laws governing fundamental measurement. Any system to be capable of measurement must have a definite "order" (First Law); must be capable of an operation termed "addition" (Second Law); and finally, subdivisions of the chosen standard must be

capable of being found such that they "equal" any prescribed members of the system (Third Law). Then follow chapters on numerical laws and derived measurement: any measurement of a property is termed "derived" if it depends on the formulation of numerical laws which state relations between magnitudes already measured, *e.g.* density. Chapters IX and X discuss the failure of the laws of measurement in the two cases when errors are regular or irregular, and the consequent important distinction between accuracy and sensitivity. The author rejects the ordinary Gaussian error function because of inconsistencies (*e.g.* no limit to the maximum error), but he admits that its use is justified by practice. He then puts forward formulæ of his own, which he maintains are equally justified in his experience, and have the advantage of greater arithmetical simplicity.

The book concludes with two interesting chapters on calculation and dimensional analysis, and finally an appendix on the actual measurement of certain magnitudes. Those who hope to find a short cut to the discovery of new physical laws by means of argument from dimensions will have their enthusiasm considerably damped by the last chapter. All the difficulties of the subject are due to "Rayleigh's tragic inability to give any clear account of a method which he himself used with such great success."

G. B. Brown.

Telegraphy and Telephony, including Wireless Communication. By E. Mallett, D.Sc. (Eng.), M.I.E.E. [Pp. ix + 413, with 287 figures.] (London: Chapman & Hall, 1929. Price 21*s.*)

This is the first attempt, so far as the writer is aware, to bring together within a single volume the most important parts of that mass of literature included under the title of Electro-Communications. The selection of the best material for such a book is necessarily a difficult task. With such a large field to cover in the very limited space of 413 pages one cannot help but admire the extraordinary detail that the author has achieved. Nevertheless it is manifest that the task was almost too much in some instances even for such an able exponent as he. The explanations of the various problems involved are undoubtedly beautifully concise, but one feels that in a few places they are hardly explicit enough for the layman. It is inevitable that considerable mathematical ability should be required properly to understand some sections of the book, and the writer doubts whether the average student of the subject would be fitted to use the volume as a first approach in the way claimed. The manner in which the subject is developed as one passes through the book from beginning to end is excellent, but in order to maintain the continuity of the argument it has been found necessary sometimes to separate from the main theme matters closely connected with it. Thus telegraph circuit arrangements are considered in several different parts of the book, and the writer feels that a more complete system of interconnecting references would be helpful.

The treatise is divided into three main parts as follows:

Part 1, Line Telegraphy; Part 2, Line Telephony; and Part 3, Wireless Telegraphy and Telephony. It opens with a general survey of simple telegraphic apparatus and elementary circuits. The theory of the leaky line is then dealt with, followed by a discussion on transients. Next telegraph repeaters and relays are considered. With a further chapter devoted to automatic and multiplex systems this fills the space allotted to line telegraphy. The introduction to telephony is made through a general discussion of alternating quantities and wave motion treated both mathematically and with the aid of mechanical models. This is followed by some very useful information about the characteristics of apparatus for the production and transmission of speech currents and their conversion back to sound. The general theory of the propagation of alternating currents along lines is then

developed, showing the effect of reflections, inductance loading, and the methods adopted to avoid interference between neighbouring conductors. Thermionic valves in their application to repeaters, echo suppressors, and carrier wave working are discussed, with a well-set-out description of the ordinary manual telephone system and the Strowger automatic equipment. Part 3 deals in the first place with the theory of the propagation of electromagnetic waves, special consideration being given to their application to wireless communications. The conditions prevailing in radio frequency oscillatory circuits are then analysed. The production of high-frequency oscillations by thermionic valves, methods of frequency control, and the modulation of the wave are explained. References to spark and arc systems of generating oscillations are also included. Lastly various receiving circuits, amplifying and rectifying arrangements are briefly considered. At intervals throughout the book numerical examples are given to illustrate points under discussion, and certain important mathematical investigations are included in an appendix.

Whatever criticism may be levelled against this book in regard to its scope and its pretensions, there can be no doubt that the author has provided a most useful dissertation dealing very effectively with a large proportion of the vast multiplicity of problems which the real student of this subject has to face. Prof. Mallett has achieved a remarkable success where many would have failed.

H. MONTEAGLE BARLOW.

High-voltage Cables. By L. EMANUELI, M.I.E.E., M.Amer.E.E. [Pp. vii + 107, with 81 figures.] (London: Chapman & Hall, 1929. Price 8*s*. 6*d*.)

THE author of this book is so closely identified with his subject that one expects from him only the very best. In this volume that reputation is amply maintained, his great ability in scientific analysis coupled with an extensive practical experience being always in evidence. As one who was privileged to listen to the original lectures on which the book is based, the writer feels that without the present publication much would have been lost, not only because the information is now available to a wider circle, but also because the subject is one that requires detailed study in a way which is only possible when set out in print. The book is divided into sections corresponding with the five lectures which the author gave. A summary of the principal processes in the manufacture of cables and the general characteristics of materials employed in the construction of various types forms the introduction. The second section is a critical investigation of the impregnating compound, many of the observations given being quite new. The electrostatic field is considered in the third section and various methods for determining its distribution in specific cases are detailed. The results of such measurements are examined with regard to their bearing upon cable construction. The fourth section deals with the injection of the impregnating compound and its effects upon the properties of the cable. The influence of ionisation of the residual gas in the dielectric when subjected to the intense electric fields met with in high-voltage cables is treated in a clear and concise manner. In the fifth section the mechanical properties of the finished cable with special reference to bending are discussed first. The interpretation of dielectric loss, power factor, and breakdown voltage measurements are then considered, and finally some theories are advanced to account for breakdown and deterioration in cables. Mention is also made of certain special devices designed to give additional protection.

The book is a workmanlike exposition of the latest developments in high-voltage cables and forms a most valuable addition to the literature of this subject.

H. MONTEAGLE BARLOW.

Blonde or Brunette: The Art of Hair-Dyeing. By H. STANLEY REDGROVE, B.Sc., and G. A. FOAN. [Pp. xii + 182, illustrated.] (London: W. Heinemann, 1929. Price 7s. 6d. net.)

HITHERTO we have always associated the practice of hair-dyeing with the possession of false-teeth and other such indelicate matters that are best referred to only in whispers amongst intimate friends; but the authors of this book would persuade us that the art of hair tinting has as much right to be considered as a branch of applied æsthetics as any other form of art, and, indeed, there are many who would probably prefer to meet Miss Anita Loos's blonde heroine to spending an evening in the company of Mr. Epstein's Rima.

At all events Mr. Redgrove, who is the author of books upon alchemy, and Mr. Foan, who is President of the Hairdressers' Educational Society, are obviously the right people to compile such a book as this, wherein may be found the details of those mystic arts whereby blondes may become brunettes, brunettes become blondes, and the flaming locks of Mr. Walpole's *Mirabelle Starr* be quenched in peroxide of hydrogen, that veritable Philosopher's Stone, to the innocent flaxen tresses of a Gretchen.

Possibly all will not agree with the authors' apologia for their art that, just as we have to take to glasses to improve our sight, so, too, there are cases where the personal appearance may be improved by the judicious use of hair dyes, for we are reminded of Mr. Gilbert Frankau's words in *One of Us*:

"I had a Mary too, and raven-hued
Was every tress of her. Alack! love died
The day she yielded them to peroxide."

It may be doubted also whether the inclusion of the formula for apigenin and α-hydrojuglone will help the budding trichologist much but still it is all to the good to have collected together all the information, theoretical and practical, that is contained within these two covers. The subject-matter is indicated by the titles of the sections: The structure and pigments of the hair, and colouring matter generally; Hair dyes, bleaches, and decolorants; The practical art of hair dyeing; Some causes of grey hair; and a bibliography.

The days of the barber-surgeon are past, but with the demands now made upon the barber it is becoming increasingly necessary for him to know something about the chemistry of the various dyes, washes, and lotions with which he has to deal, and his work is unlikely to decrease.

F. A. MASON.

Photo-Processes in Gaseous and Liquid Systems. By R. O. GRIFFITH, D.Sc., and A. MCKEOWN, D.Sc. (Pp. viii + 691, with 52 diagrams.] (London: Longmans, Green & Co., Ltd., 1929. Price 25s. net.)

THE remarkable development of atomic physics in recent years is responsible for the present widespread activity in the study of "Photo-Processes." Armed with the notions supplied by the physicist, the experimental chemist has made haste to reinvestigate the subject of photochemistry in all its aspects. Acceptance of the Quantum Theory impelled rigorous refinements of experimental technique, with the result that practically all the older work has been rendered obsolete. The important advances made since the War have created the need for a critical review of the subject in its entirety; not merely for the benefit of those engaged upon specific problems relating thereto, but for the enlightenment of the general scientific reader. It is obvious that a task of this character could only be undertaken successfully by highly competent and painstaking persons. The labour involved is colossal.

The authors, R. O. Griffith and A. McKeown, have done excellently well. Their book is comprehensive in scope, critical in treatment, and up-to-date in so far as it is possible for a book to be. References to the scientific literature are numerous for the year 1928.

In the opening chapters is presented an exceedingly lucid account of the interpretation of spectra, which is fundamental to the whole of the subject-matter. The theory of Atomic Spectra is exceptionally well developed, in logical rather than in historical sequence. The Russell and Saunders quantum number notation is adopted as being the one universally used nowadays. If the account of Molecular Spectra is less co-ordinated, the deficiency is certainly inherent in the subject itself at the present time. The standpoint of classical quantum theory is maintained throughout, though occasional reference is made to tho Wave Mechanics. The physical sections of the book deal with many topics of great interest to chemists. Energy-level diagrams are given. There is a discussion of the life periods of excited atoms. Heats of dissociation of diatomic molecules are deduced from band spectra. And much else. The long chapter on Fluorescence provides striking illustration of the way in which more or less empirical observations piece together when viewed against the background of modern physical theory. This is a very stimulating chapter. The transition from chemical physics to physical chemistry is accomplished rather neatly by a chapter on Chemiluminescence.

The second half of the book is devoted to a detailed study of the chief photochemical reactions, particular emphasis being laid upon experimental determination of quantum yields. The authors are at pains to point out where experimental data are lacking, and where further work is necessary. The treatment is admirable, though occasionally the force of argument is weakened by the results of recent experiments.

C. H. Johnson.

South Wales: a Physical and Economic Geography. By S. W. Rider, B.A., M.Sc., and A. E. Trueman, D.Sc., F.G.S. [Pp. viii + 190, with 34 maps and illustrations.] (London: Methuen, 1929. Price 4*s*. net.)

This work is primarily a physical and economic geography of the South Wales Coalfield and the industrial and rural areas immediately adjacent to it. The first four chapters deal with the physical basis of the region, *i.e.* its physical evolution, structure, relief, and climate. There follow chapters on the growth and distribution of settlements, the beginning of trade, and the development and present condition of agriculture. The next five chapters treat the industrial development of the area, chapters being devoted to the Copper Industry, the Iron and Steel Industries, the Tin Plate Industry, the Coal Trade, and Other Industries. Chapters XIII and XIV deal with the development of Inland Transport and Ports and Shipping. Finally there is a brief interesting treatment of the growth and distribution of population, which epitomises the phenomenal developments of the nineteenth century. The historical approach is adopted throughout. In fact, the writers have attempted to give a complete regional geography of their area. Thus, within such a small compass there is wide range of topics, but the same high standard is maintained throughout the book. It is well arranged, presented in a readable form, and amply illustrated. The County Boundaries and more place names used in the text might have been inserted in one of the general maps; there is not an adequate key to the Sunshine Map (p. 43), and the growth of typical towns could have been shown graphically on p. 160. There is a glossary of geological terms and a good bibliography.

R. E. Dickinson.

Manual of First-order Levelling. By HENRY G. AVERS. [Pp. iii + 93, with 18 illustrations.] (Washington: United States Government Printing Office, 1929. Price 30 cents.)

IN this little manual, Mr. Avers (Senior Mathematician in the U.S. Coast and Geodetic Survey) gives an account of the field work and methods of computation employed in the first-order levelling of the Department.

The term "first-order" levelling is used instead of the terms "precise" or "high-precision" levelling more commonly adopted in Europe. The enormous area to be covered by the work of the Department makes it imperative that *speed* should be kept in view, even at the cost of the highest limit of accuracy. The results exhibited are, however, of a very high order.

The report is full, clearly written, and well illustrated, and cannot fail to prove interesting to all who are concerned with such work. The instructions for crossing wide rivers, in first-order levelling, for instance, may be specially mentioned.

Even those whose requirements involve much greater speed and less precision than are aimed at here will do well to read the report, however, for it is full of useful hints on the care and adjustment of the instruments, and on other matters. Both the responsible department and the author are, in the opinion of the writer, to be congratulated upon it.

M. T. M. ORMSBY.

Seven-place Natural Trigonometrical Functions. By HOWARD CHAPIN IVES, C.E. [Pp. v + 222, with 20 illustrations.] (New York: John Wiley & Sons; London: Chapman & Hall, 1929. Price 12*s.* 6*d.* net.)

THE true nature of this little book would have been better expressed by such a title as "The Field Engineer's Handbook of Tables and Formulæ." Besides Trigonometrical Functions, it contains tables for stadia reductions, squares, cubes, and reciprocals, astronomical tables, collections of formulæ in plane trigonometry and volumes and areas, formulæ relating to curve ranging, and a comprehensive table of "units," connecting British, American, and Metric units, with tables for converting inches to decimals of a foot and minutes to decimals of a degree.

Besides all these, there are several appendices dealing with adjustment of instruments, area computations, curve ranging (vertical and horizontal), and field astronomy, including azimuth by the use of the solar attachment.

The author states that the natural values of the functions are tabulated because calculating machines have largely replaced logarithmic tables in computation. Besides the ordinary functions, these tables contain chords and co-chords, versed sines and external secants, and lengths of arcs. The values are given for each minute of angle.

It is remarkable that formulæ in spherical trigonometry do not appear to be included, though they are used in the appendices.

The portion (about 50 pages) devoted to the appendices suffers, naturally, from the limitations attendant on the effort to crowd so much into so small a space. But, regarded as a pocket-book of formulæ, the book contains a great deal of useful information, and it is clearly printed and of a convenient size for carrying in the pocket.

M. T. M. ORMSBY.

The Corridors of Time. The Way of the Sea. By HAROLD PEAKE and HERBERT JOHN FLEURE. [Pp. viii + 168, with 72 illustrations.] (Oxford: at the Clarendon Press. Price 5*s.* net.)

IN *The Way of the Sea* Peake and Fleure have published another of their small volumes on antiquity. The book deals, mainly, with the epochs when mankind, having emerged from the Stone Age, was founding centres

of trade, and what are termed "civilisations." Accompanying this process there was going forward a complex and intricate series of movements of traders, and others, in Western Europe, and different parts of the world, and these, together with the various cultural "influences" which such activities brought to bear upon the populations subjected to them, are described in considerable detail by the authors. Though their conclusions are, probably, in the main correct, yet the reader of *The Way of the Sea* is in danger of becoming confused by the number and complexity of these movements, and this is not advantageous to the student. It should, it seems, be possible to give an account of what was taking place in human affairs, during the momentous period with which this volume deals, in a manner more lucid, and less fatiguing. But, except for this lack, *The Way of the Sea* is a valuable production. Among others, there are chapters, all well and profusely illustrated, on Great Stone Monuments, Beakers and their Interpretation, the British Isles, the First Dynasty of Babylon, and the Middle Kingdom of Egypt, together with a Chronological Summary, Bibliography, and Index. *The Way of the Sea* should find many readers in archæological circles, and among those interested in the genesis of modern civilisations.

J. Reid Moir.

The Acoustics of Orchestral Instruments. By Dr. E. G. Richardson, D.Sc., Ph.D. [Pp. 158, with 31 figures.] (London: Edward Arnold & Co., 1929. Price 10*s*. 6*d*. net.)

In some branches of industry the development of the practice has preceded that of the science appertaining to it. In few cases is this more true than in the making of musical instruments of all kinds, for every musical instrument now in use has been developed almost entirely independently of any theory as to its mode of action, and in most cases the only theory there may be is only partially worked out and is still uncertain. Although an elementary explanation of the method by which the sound is produced often has been given in very early times, perhaps even by the Greeks, the actual mode of vibration is very complicated and the complete theory correspondingly difficult.

In this book Dr. Richardson gives an elementary explanation of the behaviour of the several instruments of the orchestra and of the organ, which should be both helpful and interesting to all those who have to do with these instruments, whether as makers or players.

In the first chapter he discusses the production of sound in the case of the simple organ pipe, and explains, with the help of spring analogies, the way in which air oscillates to and fro in the tube. The frequency of this oscillation depends, as Dr. Richardson shows, on the length of the pipe, for this determines the wave-length of the note.

The latter is not, however, an exact multiple or submultiple of the actual distance from the mouth to the open end, but in calculating the wave-length this distance has to be increased by the end corrections. One of these corrections, the one at the open end, is, as Dr. Richardson points out (p. 28), equal to 0·58 of the radius of the pipe. But there is a much greater correction to be added at the mouth end, as Bates has recently shown (*Phil. Mag.*, Jan. 1930, p. 23), varying from 2·4 to over 3·8 times the radius, which is some 4 to 7 times as great as the correction mentioned. This mouth correction becomes a little smaller as the frequency rises; it depends, however, much more on the height of the mouth, being greatest when this is least.

In Chapter II the production of "edge-tones" is described and applied to explain the production of sound in a flue pipe. The vibrating system is here a coupled one, consisting of (*a*) the column of air in the pipe, which tends to oscillate to and fro lengthwise in the pipe at one or more of certain definite

frequencies, and (*b*) a film of air issuing from the throat of the pipe and impinging on the opposite edge or lip of the mouth. Of these (*a*) has a much more definite frequency than (*b*). The energy of this vibrating system is being continuously dissipated as sound, chiefly from the mouth but also to a less extent from the opposite end when that is an open end; this loss of energy is made good from that of the film of air issuing from the mouth. This film has a natural period of its own produced by the edge-tone vortices, which tend to form at intervals determined by the width of mouth aperture and by the velocity of the air stream; but this frequency is rather an indefinite one, and it is easily "forced" by the motion of the air-column in the pipe. The way in which this reacts is not explained in this book, and while the theory of the edge-tones explains why the air-reed tends to vibrate, it is doubtful if the vortices themselves are formed in all cases, and whether, therefore, they can be looked upon as the actual sources of the energy communicated to the air-column. While the pipe is sounding there is an alternating flow of air into and out from the mouth of the pipe; these movements of the air are produced by the flow of the film of air ((*b*) above), but their frequency is controlled by the alternating arrival at the mouth of the compressions and rarefactions which are travelling up and down the pipe. When a compression travels down the pipe and arrives at the mouth, it causes the stream of air to be diverted and to pass to the outer side of the edge. In so doing the stream, by Bernouti's theorem, owing to its velocity, will produce a suction, and will tend to extract the air from the pipe until the compression has become changed into a rarefaction. Thus the compression which has travelled down is reflected back as a rarefaction which will travel up the pipe; it will be reflected back again from the upper end as a compression. At the same time the combined effect of this rarefaction and of the new vortex (which by this time tends to form on the inner side of the edge) causes the film of air to flow to the *inner* side of the edge, where it will build up an excess of pressure and thus gradually transform the rarefaction back into a compression. In this way the energy of the vibrating column of air is maintained.

The organ builders have found that cutting a number of nicks in the aperture at the mouth from which the air film issues, helps the pipe to speak. It is obvious that these nicks will produce an increased turbulence in the air stream, and will presumably tend to prevent the formation of the edge-tone vortices. This suggests that the rôle of these vortices is somewhat less important than is supposed by Dr. Richardson. Further experiments are required to settle this point, and for the present we prefer to keep an open mind on the question. All this illustrates the exceeding difficulty of the theory even of such a comparatively simple system as the flue organ pipe.

Dr. Richardson then proceeds to develop an interesting theory—worked out more fully in an appendix—to explain the effect of the sizes of the holes used to obtain the musical scale upon the frequency of the note given by a flute. On page 50 he mentions the interesting fact that the motion of the air through these holes is concentrated towards the outer margins of the hole. This distribution is analogous to the well-known concentration of a rapidly alternating electric current in the outer layer of a conductor. It has received a recent application in the piano player in the endeavour to obtain the maximum flow of air through a valve with a minimum of area of valve, for by altering the section of the aperture from a circular one to a slightly smaller one in the form of a cross, the force required to lift the valve is reduced while the quantity of air that the valve will pass remains the same.

In the next chapter the action of the brass wind instruments is dealt with. Here again we have coupled vibrating systems—probably four in all. Of these the chief are (*a*) the air-column in the long conical tube constituting the main portion of the instrument, and (*b*) the lips of the performer. As in the

case of the organ pipe, the length of the air-column controls the frequency of the note, of which the approximate pitch is determined by the tightness with which the performer's lips are stretched and the velocity of the air through them. There are two other vibrating systems according to Dr. Richardson, viz. (*c*) edge-tones produced beyond the lips in the cavity of the mouthpiece and (*d*) the air in the cavity of the mouth of the performer. There can be little doubt that to obtain the best results all these should be adjusted to assist each other. Some players have stated that they *sing* the note which they wish to produce. If so, there is still a fifth system—that of the vocal chords.

In the fourth chapter the percussion instruments, and in the fifth chapter the string instruments, are dealt with.

The relations of the frequencies of the notes of the musical scale were known to Pythagoras, and the modes of vibrations of strings and the effects of changes of length, tension, and mass have also been known for a very long time. But when any attempt is made to explain the mode of action of the complete violin, or to understand the effects on the tone quality of the size, the thickness, or the material of the belly, back, sound-post, and bridges, one finds insuperable difficulties in attempting to carry the theory beyond the most elementary principles. Nor has science been able to suggest any improvement in the violin since the time of the great makers, Stradivarius and Guarnerius.

Very similar statements are true of the piano. There, it is true, the instrument has been continuously improved, and is still in course of improvement at the present time. But the improvements have resulted rather from the accumulated experience of the makers, who have made slight variations from time to time in the methods of construction of the several components—the belly, the action, the hammers, the frame, the strings—and so have gradually evolved a more and more perfect instrument. Each of these makers has usually had some theory of his own as to the way the vibration is produced, but it is doubtful if these theories have been of any assistance. Almost every maker, for instance, has at some time tried a double sound-board on the analogy of the violin, only to find the experiment a failure.

The present position is that in all the ordinary musical instruments we have a construction which has been arrived at empirically, to which science has still to try and fit a theory. When she has succeeded in doing this, she may hope to begin to be of use to the manufacturers in suggesting new ways of improving the instruments. As illustrating how easily a scientific man may go astray in dealing with these complicated systems, we may refer to Helmholtz's statement that the hammer should strike the string of a piano at either one-seventh or one-ninth of the vibrating length of the string from one end, in order to cut out one or either of the two corresponding inharmonic upper partials which have frequencies seven and nine times that of the fundamental respectively. In practice this is found neither necessary nor desirable; and in fact the position of the striking point is varied, at the bass end it is at about one-eighth of the vibrating length from the end, and at about one-fourteenth at the treble. Nor does it appear to be true that the striking of the string at a particular point prevents the formation of the upper partials which would have a node at that point; for, as Dr. Richardson points out in the book under review (p. 114), "recent experiments have cast doubt on the validity of applying Young's law to the special case of a smartly struck string. It has been shown, for example, that though a string be hit at one-eighth of its length from one end, the eighth harmonic (four octaves above the fundamental) is still present in the resulting sound."

Dr. Richardson says that a harpist plucks the strings at one-seventh (p. 112). But in practice it is obviously impossible for a harpist to do

this, owing to the shape of the hand. Actually the string is plucked much more nearly in the middle.

Speaking of the violin, Dr. Richardson says on page 122, "the varnish does not appear to have any marked acoustic effect, although it slightly raises the tone of belly and back by giving them greater rigidity."

A new violin, before the body has been varnished, has an excellent tone, which the application of the varnish almost destroys, the violin becoming exceedingly dull and difficult to play on. It is not until the varnish has become thoroughly hard that the resonance is recovered, and as the varnish dries still more perfectly the quality continues to improve. There can be little doubt that the initial dulling effect of the varnish is due to its viscous nature, which will tend to destroy the energy of the vibrations. A compression or a stretching of the varnish will cause it to yield, so that there is a loss of restoring force, and the energy is transformed into heat. A great deal of nonsense has been written about the varnishes that the old makers used. Actual experiment has proved that the modern varnishes are as good as the old ones; and, as is well known, experiments have also shown that a modern violin can be made that is indistinguishable in its tone quality from the best of the old ones.

In the last chapter the acoustic properties of the hall in which the performance of the orchestra is being given are briefly dealt with.

The book is well got up and illustrated, and gives a very useful and interesting account of the theory underlying the instruments of the orchestra.

R. S. Clay.

BOOKS RECEIVED

(Publishers are requested to notify prices)

Einführung in die Nicht-Euklidische Geometrie. Von Hans Mohrmann, Prof. an der Techn. Hochschule, Darmstadt. Leipzig: Akademische Verlagsgesellschaft m.b.H., 1930. (Pp. xii + 126, with 65 figures.) Price M. 8.30, bound M. 9.30.

The Theory of Approximation. By Dunham Jackson, Professor of Mathematics in the University of Minnesota. New York: American Mathematical Society, 501 West 116th Street, 1930. (Pp. viii + 178.)

Science and the New Civilisation. By Robert A. Millikan, Norman Bridge Laboratory of Physics, California Institute, Pasadena. New York and London: Charles Scribner's Sons, 1930. (Pp. 194.) Price 7*s*. 6*d*. net.

The Physics and Chemistry of Surfaces. By Neil Kensington Adam, M.A., Sc.D. Oxford: at the Clarendon Press, 1930. (Pp. x + 332.) Price 17*s*. 6*d*. net.

Examples in Mechanics. By A. Robson, M.A., Senior Mathematical Master, Marlborough College, and C. J. A. Trimble, M.A., Senior Mathematical Master, Christ's Hospital. London: G. Bell & Sons, 1930. (Pp. vii + 155.) Price 4*s*. 6*d*. net.

The Size of the Universe. Attempts at a Determination of the Curvature Radius of Spacetime. By Ludwik Silberstein, Ph.D. London: Oxford University Press, 1930. (Pp. viii + 215.) Price 10*s*. net.

Physica. The Works of Aristotle translated into English. By R. P. Hardie, M.A., Reader in Ancient Philosophy in the University of Edinburgh, and R. K. Gaye, M.A., late Fellow of Trinity College, Cambridge. Oxford: at the Clarendon Press, 1930. (Pp. 184a + 267b.) Price 10*s*. 6*d*. net.

X-rays. By B. L. Worsnop, B.Sc., Ph.D., Lecturer in Physics, King's College, London. With a Preface by O. W. Richardson, F.R.S. London: Methuen & Co., 36 Essex Street, W.C. (Pp. ix + 101.) Price 2*s*. 6*d*. net.

Introduction to Physical Optics. By John Kellock Robertson, F.R.S.C., Professor of Physics, Queen's University, Kingston, Canada. London: Chapman & Hall, 1930. (Pp. vii + 422.) Price 20*s*. net.

Electron Physics. By J. Barton Hoag, Ph.D., Instructor in Physics, University of Chicago. With a Foreword by A. J. Dempster, Ph.D., Professor of Physics, University of Chicago. London: Chapman & Hall, 1930. (Pp. ix + 208.) Price 15*s*. net.

Un Coup d'Œil sur l'Histoire des Sciences et des Théories Physiques. Par M. Émile Picard, de l'Académie française, Secrétaire perpétuel de l'Académie des Sciences. Paris: Gauthier-Villars et Cie., 55 Quai des Grands-Augustins, 1930. (Pp. 100.)

Modern Physics. A General Survey of its Principles. By Theodor Wulf, S.J., Ignatius Kolleg, Valkenburg, Holland. Translated from the Second German Edition by C. J. Smith, Ph.D., M.Sc., A.R.C.S., Lecturer in Physics, Royal Holloway College, Englefield Green, Surrey. London: Methuen & Co., 36 Essex Street, W.C. (Pp. xi + 469.) Price 35s. net.

Elementary Chemistry for Students of Hygiene and Housecraft. By C. M. Taylor, M.A., Headmistress, Redland High School, Bristol, and P. K. Thomas, M.B., B.Sc. London: John Murray, Albemarle Street, W.1. (Pp. vii + 193.) Price 3s. 6d. net.

Practical Chemistry for Advanced Students. By Arthur Sutcliffe, M.A., B.Sc., A.I.C., Cambridge and Country High School. London: John Murray, Albemarle Street, W.1. (Pp. vii + 216.) Price 4s. 6d. net.

Some Applications of Organic Chemistry to Biology and Medicine. By George Barger, Edinburgh University. London: McGraw-Hill Publishing Co., 6 Bouverie Street, E.C.4, 1930. (Pp. 186). Price 12s. 6d. net.

Oxidation-Reduction Potentials. By L. Michaelis, M.D. Translated from the German Manuscript by Louis B. Flexner. London: J. B. Lippincott Company, 16 John Street, Adelphi. (Pp. xii + 197.) Price 12s. 6d. net.

The Beginnings of Chemistry. A Story Book of Science for Young People. By Harriett Blaine Beale. With illustrations by J. Edmund Woods. London: George Allen & Unwin, Museum Street. (Pp. ix + 243.) Price 4s. 6d. net.

Solvents. By Thos. H. Durrans, D.Sc., F.I.C. Being Vol. IV of a Series of Monographs on Applied Chemistry; under the Editorship of E. Howard Tripp, Ph.D. London: Chapman & Hall, 1930. (Pp. xv + 144.) Price 10s. 6d. net.

Colloid Symposium Annual (formerly called Colloid Symposium Monograph). Papers presented at the Seventh Symposium on Colloid Chemistry, John Hopkins University, June 1929. Edited by Harry Boyer Weiser, Professor of Chemistry, The Rice Institute. New York: John Wiley & Sons; London: Chapman & Hall, 1930. (Pp. viii + 300.) Price 22s. 6d. net.

Handbook of Chemical Microscopy. By Émile Monnin Chamot, B.Sc., Ph.D., and Clyde Walter Mason, A.B., Ph.D. Vol. I, Principles and Use of Microscope and Accessories, Physical Methods for the Study of Chemical Problems. New York: John Wiley & Sons; London: Chapman & Hall, 1930. (Pp. xiii + 474, with 149 figures.) Price 22s. 6d. net.

The Chemistry of the Colloidal State. A Textbook for an Introductory Course. By John C. Ware, Sc.M., Ph.D. New York: John Wiley & Sons; London: Chapman & Hall, 1930. (Pp. xiv + 313.) Price 18s. 6d. net.

Organic Syntheses. An Annual Publication of Satisfactory Methods for the Preparation of Organic Chemicals. Editorial Board, Hans T. Clarke and others. Vol. X. New York: John Wiley & Sons; London: Chapman & Hall, 1930. (Pp. vii + 119.) Price 8s. 6d. net.

The Penicillia. By Charles Thom, Principal Mycologist, Bureau of Chemistry and Soils, United States Department of Agriculture. With assistance of Dr. Margaret B. Church in handling the Cultures and in the Medical chapter, Dr. O. E. May upon the Biochemical chapter, and Dr. M. A. Raines in preparing the illustrations. London: Baillière, Tindall & Cox, 8 Henrietta Street, Covent Garden, W.C.2, 1930. (Pp. xiii + 644, with 99 figures.) Price 45s. net.

The Life-Force in the Inorganic World. By Eleanor Hughes-Gibb, F.L.S. London: George Routledge & Sons, 68 Carter Lane, E.C., 1930. (Pp. xix + 168.) Price 5*s*. net.

Achema Jahrbuch. Jahrgang 1928–30. Berichte über Stand und Entwicklung des chemischen Apparatewesens, herausgegeben unter Mitwirkung von Fachgenossen aus Wissenschaft und Technik von Dr. phil., Dr. Ing. Max Buchner. Hanover: Deutsche Gesellschaft für chemisches Apparatewesen. E.V. (Pp. 260.) Price 10 Rm.

A Textbook of Dairy Chemistry. Theoretical and Practical for Students of Agriculture and Dairying. By Edgar R. Ling, M.Sc., A.R.C.Sc., F.I.C., Lecturer in Agricultural Chemistry at the Midland Agricultural College, and Official Deputy Agricultural Analyst for the Lindsey Division of Lincolnshire. London: Chapman & Hall, 1930. (Pp. vii + 213.) Price 6*s*. net.

Chapters on the Geology of Scotland. By the late Benjamin Neeve Peach, LL.D., F.R.S., and the late John Horne, LL.D., F.R.S. London: Oxford University Press, 1930. (Pp. xvi + 232, with 27 figures and 18 plates.) Price 10*s*. 6*d*. net.

Economic Geology. By H. Ries, A.M., Ph.D., Professor of Geology at Cornell University. Sixth Edition. Revised. New York: John Wiley & Sons; London: Chapman & Hall, 1930. (Pp. v + 860, with 291 figures.) Price 30*s*. net.

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The Whip Snakes and Racers. Genera Masticophis and Coluber. By Arthur Irving Ortenburger. Ann Arbor, U.S.A.: University of Michigan, 1928. (Pp. xviii + 247, with 64 figures.) Memoirs of the University of Michigan Museums. Volume I.

A Handbook of the Mosquitoes of North America. Their Structure—How they Live—How they carry Disease—How they may be Studied—How they may be Controlled—How they may be Identified. By Robert Matheson, Professor of Entomology, New York State College of Agriculture, Cornell University. London: Baillière, Tindall & Cox, 7 Henrietta Street, W.C.2. (Pp. xvii + 268, with 23 figures and 25 plates.) Price 25*s*. net.

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Salmon and Sea Trout. With chapters on Hydro-Electric Schemes, Fish Passes, etc. By W. L. Calderwood, I.S.O., F.R.S.E. London: Edward Arnold & Co., 1930. (Pp. ix + 242, with 12 plates.) Price 12*s*. 6*d*. net.

Animal Life on the Seashore. By Louis P. W. Renouf, B.A., Dip. Agric., Professor of Zoology in University College, Cork, I.F.S. London: George Routledge & Sons, 68 Carter Lane, E.C., 1930. (Pp. 78, with 7 plates.) Price 6*d*. net.

A Report on the Fishing Survey of Lakes Albert and Kioga, March to July 1928, with Appendices I to V. By E. B. Worthington, B.A., F.L.S., Gonville and Caius College, Cambridge, Naturalist-in-Charge of the Survey. Published on behalf of the Government of the Uganda Protectorate by the Crown Agents for the Colonies, 4 Millbank, London, S.W.1. (Pp. 136, with 2 maps and 24 illustrations.) Price 10*s*. net.

The Genetical Theory of Natural Selection. By R. A. Fisher, Sc.D., F.R.S. Oxford: at the Clarendon Press, 1930. (Pp. xiv + 272.) Price 17*s*. 6*d*. net.

Aquatic Mammals. Their Adaptation to Life in the Water. By A. Brazier Howell, Lecturer in Comparative Anatomy, the John Hopkins University, Springfield, Illinois. U.S.A.: Charles C. Thomas; London: Baillière, Tindall & Cox, 8 Henrietta Street, W.C.2. (Pp. xii + 338, with 53 figures.) Price 22*s*. 6*d*. net.

The Story of Evolution. Facts and Theories on the Development of Life. By Benjamin C. Gruenberg. London: Chapman & Hall, 11 Henrietta Street, W.C.2. (Pp. xvi + 473, with 101 figures.) Price 21*s*. net.

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Human Speech. Some Observations, Experiments, and Conclusions as to the Nature, Origin, Purpose, and Possible Improvement of Human Speech. By Sir Richard Paget, Bart., Fellow of the Physical Society of London. London: Kegan Paul, Trench, Trübner & Co.; New York: Harcourt, Brace & Company, 1930. (Pp. xiv + 360.) Price 25*s*. net.

The Newer Knowledge of Nutrition. The Use of Foods for the Preservation of Vitality and Health. By E. V. McCollum, Ph.D., Sc.D., and Nina Simmonds, Sc.D. Third Edition. Entirely rewritten. New York: The Macmillan Company, 1927. (Pp. x + 675.) Price 18*s*. net.

Romance of the Machine. By Michael Pupin, of Columbia University. New York and London: Charles Scribner's Sons, 1930. (Pp. 111.) Price 4*s.* 6*d.* net.

The Flood. New Light on an Old Story. By Harold Peake, M.A., F.S.A. London: Kegan Paul, Trench, Trübner & Co., 68 Carter Lane, E.C., 1930. (Pp. x + 123.) Price 5*s.* net.

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Statistical Methods for Research Workers. (Biological Monographs and Manuals No. 5.) By R. A. Fisher, Sc.D., F.R.S. Third Edition, Revised and Enlarged. Edinburgh: Oliver & Boyd, Tweeddale Court, and London: 33 Paternoster Row, E.C., 1930. (Pp. xiii + 283.) Price 15*s.* net.

Modern Sunlight. By Leonard V. Dodds. London: John Murray, Albemarle Street, W. (Pp. x + 322.) Price 7*s.* 6*d.* net.

The Nature of Life. By Eugenio Rignano, Professor of Philosophy in the University of Milan. London: Kegan Paul, Trench, Trübner & Co.; New York: Harcourt, Brace & Company, 1930. (Pp. x + 168.) Price 7*s.* 6*d.* net.

The Living Past. By John C. Merriam, President of the Carnegie Institution of Washington. London: Charles Scribner's Sons, 7 Beak Street, W.1. (Pp. xi + 144.) Price 7*s.* 6*d.* net.

Latex. Its Occurrence, Collection, Properties, and Technical Applications. By Ernst A. Hauser, Ph.D. Frankfurt a/M. With Patent Review compiled by Carl Boehm von Boernegg, Ph.D., Frankfurt a/M. Translated by W. J. Kelley, Ph.D., Philadelphia, Pa. New York: The Chemical Catalog Company, 419 Fourth Avenue, at 29th Street, 1930. (Pp. 201.) Price $4.00.

Birth Control, Abortion and Sterilisation. By Dr. J. H. Leunbach, Copenhagen. General Secretary of the World League for Sexual Reform. With a Preface by Norman Haire, Ch.M., M.B. London: Kegan Paul, Trench, Trübner & Co., 68 Carter Lane, E.C., 1930. (Pp. vii + 78.) Price 2*s.* 6*d.* net.

Television To-day and To-morrow. By Sydney A. Moseley and J. Barton Chapple, A.M.I.E.E. With a Foreword by John L. Baird. London: Sir Isaac Pitman & Sons, Parker Street, Kingsway, W.C.2, 1930. (Pp. xxiii + 130, with 48 plates and 38 figures.) Price 7*s.* 6*d.* net.

The Theory of Electrical Artificial Lines and Filters. By A. C. Bartlett, B.A., Member of the Research Staff of the General Electric Co. London: Chapman & Hall, 1930. (Pp. ix + 155.) Price 13*s.* 6*d.* net.

The Bacteriophage and its Clinical Applications. By F. D. Herelle, Professor of Bacteriology, Yale University School of Medicine. Translated by George H. Smith, Professor of Immunology, Yale University School of Medicine. London: Baillière, Tindall & Cox, 7 Henrietta Street, W.C.2. (Pp. viii + 254.) Price 18*s.* net.

Bergey's Manual of Determinative Bacteriology. A Key for the Identification of Organisms of the Class Schizomycetes. By David H. Bergey, University of Pennsylvania, Philadelphia, Pa. Assisted by a Committee of the Society of American Bacteriologists, with an Index by Robert S. Breed, New York Agricultural Experiment Station. Third Edition. London: Baillière, Tindall & Cox, 7 Henrietta Street, W.C.2. (Pp. xvii + 589.) Price 27*s.* net.

Manual of Meteorology. Vol. III, The Physical Processes of Weather. By Sir Napier Shaw, LL.D., Sc.D., F.R.S. With the assistance of Elaine Austin, M.A. Cambridge: at the University Press, 1930. (Pp. xxviii + 445, with 149 figures.) Price 36*s.* net.

Cogitans Cogitata. By Herbert Wildon Carr, Fellow of the University of London, King's College. London: The Favil Press, Church Street, Kensington, 1930. (Pp. xii + 110.) Price 6*s.* net.

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Orokaiva Society. By F. E. Williams, M.A., Government Anthropologist, Territory of Papua. With an Introduction by Sir Hubert Murray, K.C.M.G., Lieut.-Governor of Papua. London: Oxford University Press, 1930. (Pp. xvii + 355, with 36 plates.) Price 25*s.* net.

Races of Africa. By C. G. Seligman, F.R.S., Professor of Ethnology in the University of London. London: Thornton Butterworth, Ltd., 15 Bedford Street, W.C.2. (Pp. 256.) Price 2*s.* 6*d.* net.

The Material Culture and Social Institutions of the Simpler Peoples. An Essay in Correlation. By L. T. Hobhouse, G. C. Wheeler, and M. Ginsberg. London: Chapman & Hall, 1930. (Pp. vi + 299.) Price 10*s.* 6*d.* net.

Science in Education. Its Aims and Methods. By Henry H. Cawthorne, B.Sc., A.K.C., Firth Park Secondary School, Sheffield. London: Oxford University Press, 1930. (Pp. vi + 110.) Price 5*s.* net.

The Terminology of Physical Science. By Duane Roller. Oklahama, U.S.A.: Norman University of Oklahama Press, 1929. (Pp. 115.) Price $1.

ERRATUM

On page 737 of the April Number, Vol. XXIV, No. 26, it was stated that "An Introductory Electricity and Magnetism" by Hansel and Woodland contained 43 figures. This should read "43 plates and 151 diagrams."

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SCIENCE PROGRESS

RECENT ADVANCES IN SCIENCE

PHYSICS. By L. F. Bates, B.Sc., Ph.D., F.Inst.P., University College, London.

The Gyromagnetic Effect.—Suppose we take a short piece of iron wire and suspend it vertically from the end of a delicate quartz fibre. If the iron is suddenly magnetised, we may imagine that the electron orbits responsible for ferromagnetism are arranged with their planes perpendicular to the axis of the wire. Accordingly, we produce a change in the angular momentum about the axis of the wire, to which a corresponding reaction must arise. This reaction is manifested by a rotation of the suspended wire as a whole. Now a long series of experiments at the University of Bristol have shown that the amount of angular momentum imparted to the wire is exactly one-half the amount predicted on the theory outlined above. This discrepancy has been termed the gyromagnetic anomaly. It disappears if we assume that the spinning electron is responsible for ferromagnetism. Now, there seems to be no doubt that the magnetic moment of paramagnetic bodies arises both from the spin and from the orbital motion of the electron, so that the ratio of the change of magnetic moment to the change of angular momentum which accompanies it, should give us important information concerning the relative amounts of the spin and the orbital contributions to the magnetic moment. The experimental value of the ratio in the case of ferromagnetic substances is e/m, where e and m are respectively the charge and mass of an electron. Clearly, then, the measurement of the ratio is a matter of some difficulty, even in the case of ferromagnetic substances, where we are assisted by a magnetic moment of reasonable magnitude. In the case of paramagnetic substances we should expect the ratio to be $\frac{1}{2}$ g. e/m, where g is the Landé splitting factor for the ion responsible for the magnetic moment ; g is equal to 2 for spin electrons and equal to 1 for electrons describing orbits in the manner usually pictured. The magnetic moment induced in a paramagnetic substance by a field of reasonable magnitude is very small, and therefore the measurement of the ratio is one of extreme difficulty. Consequently, the recent experimental determina-

tion of the ratio in the case of Dysprosium oxide by Sucksmith (*Proc. Roy. Soc.*, vol. 128, p. 276, 1930) is really an outstanding experimental achievement. A direct ballistic method of measurement was obviously impossible in this case, and Sucksmith employed a resonance method, in which the magnetising current was periodically reversed with a frequency equal to that of the natural period of vibration of the suspended specimen. Dysprosium oxide was obtained 99·9 per cent. pure. It is strongly paramagnetic and obeys the Curie law. The powdered substance was packed inside a very thin walled glass tube, since it was imperative to reduce the moment of inertia of the system to a minimum; in fact, the success of the experiment depended almost entirely upon the smallness of the moment of inertia, which was about 10^{-6} gm. cm. The powder was kept in position by a plug of shellac. Special small glass hooks were used to support the specimen thus prepared, which was mounted on a delicate quartz fibre inside a glass chamber, evacuated to a pressure of 10^{-4} mm. This chamber was placed inside a vertical solenoid in which a field of about 600 gauss could be maintained. An alternating magnetic field was produced by periodically reversing a direct current of some 15 amp. The period was kept constant to 1 in 10,000 by means of a specially designed switch gear. Disturbing effects due to the action of the electrostatic field of the solenoid upon the insulated suspended system, to mechanical vibrations, to the horizontal component of the magnetic field of the solenoid, and to the action of the earth's field on the specimen, all had to be overcome by special arrangements and technique. For the elimination of the horizontal component of the earth's field two large rectangular coils were used, and, as we shall see, they served another useful purpose. The effects of ferromagnetic impurity were enormous, for less than 10^{-6} gm. of iron might produce an effect 1,000 times as great as the normal gyromagnetic effect. For example, the oscillation of the specimen was made visible by means of a very small mirror mounted at its lower end. If this mirror was cut with a steel cutter it was useless. Non-ferromagnetic shellac had to be obtained, and no iron could be handled during the preparation of the specimen. Since the material was enclosed in a thin sealed tube, the latter could be washed in acetone after handling, to remove ferromagnetic impurities. In spite of this precaution the amount of handling, which a specimen could undergo without danger, was very limited. The amplitude produced at resonance by an alternating magnetic field is due to the gyromagnetic effect plus the disturbances mentioned above. If we consider only those disturbances which have the same frequency as the gyromagnetic effect, we find that they are

90° out of phase with it, and consequently the amplitude at resonance can never be less than that due to the gyromagnetic effect alone. The minimum amplitude at resonance might therefore be taken as the correct value. Further consideration, however, showed that the observed resonance amplitude when disturbances were present was identical with the amplitude at perfect resonance in the absence of disturbances, provided that the phase of the observed oscillation was identical with that of the fundamental frequency of the magnetising current. This was arranged by bringing the phase of the oscillation into agreement with the magnetising current by suitable adjustment of the current in the coil which neutralised the earth's horizontal field. The value of g, the splitting factor, is then given by the expression—

$$2\frac{m}{e}\cdot\frac{2\chi\cdot \mathrm{T}\cdot m_0 \mathrm{H}_0}{\pi\cdot\lambda\cdot \mathrm{I}\cdot\theta},$$

where χ is the susceptibility of the paramagnetic substance per unit mass, T the period of oscillation, m_0 the mass of the paramagnetic substance, H_0 the applied magnetic field, λ the damping factor, I the moment of inertia of the specimen, and θ the minimum amplitude at resonance. The results gave $g = 1{\cdot}28 \pm 0{\cdot}07$, thus showing that both orbital and spin contributions are responsible for the magnetic moment of paramagnetic substances. The value thus obtained is in excellent agreement with the value 4/3, corresponding to the $^6\mathrm{H}_{15/2}$ state, which Hund deduced as the most probable state of the Dy^{+++} ion, and the author is to be congratulated on this particularly fine piece of work.

Incidentally, Sucksmith contributed an interesting paper on the theoretical aspects of the gyromagnetic ratio at the discussion on magnetism arranged by the Physical Society of London on May 23. This discussion was unfortunately deprived of the presence of Dr. Stoner—whose excellent survey of the development of modern magnetism, which will appear in the report of the discussion, should be very useful to readers of this section—and of Profs. Gerlach and Weiss. Gerlach has recently discovered a new relation between electric and magnetic phenomena. When a fall of temperature exists in a ferromagnetic body which is set in a uniform magnetic field with the lines of force parallel to the temperature gradient Gerlach finds that an electromotive force is established. The latter appears to exhibit about the same degree of dependence on the magnetisation of the body as the resistance of the body. In Gerlach's experiments, one end of a nickel wire was kept at a temperature of about 20° C. whilst the tempera-

ture of the other end was raised, and the magnetic field was applied. The electromotive force thus established steadily increased and reached an approximately constant value as soon as the temperature of the heated end exceeded that of the Curie point at which spontaneous magnetisation disappears. On maintaining the hot end at a temperature above the Curie point and on gradually raising the temperature of the cooler end, the electromotive force gradually decreased and became zero when the latter end acquired the temperature of the Curie point. The magnitude of the electromotive force was independent of the nature of the temperature gradient and depended only on the temperature difference between the two ends. When the electromotive force was plotted against the temperature of the heated end, the curve obtained showed great similarity to the curves obtained by plotting the relative increase in resistance $\Delta R/R$ against the magnetic field for various temperatures. This similarity was emphasised when $\Delta R/R$ and the electromotive force were respectively plotted against the relative intensity of magnetisation. A small hysteresis change in the electromotive force corresponded with an observed hysteresis change in the resistance. It may be mentioned that resistance changes in ferromagnetic bodies are now coming into the limelight owing to the many suggestions that ferromagnetism may in part be attributed to the free or conduction electrons. In this connection reference should be made to the interesting experiments of Dorfmann and Jaanus (*Zeit. für Phys.*, vol. 54, p. 277, 1929), who investigated the behaviour of the thermoelectric power of nickel against platinum with temperature, particularly in the neighbourhood of the Curie point. Their results showed the existence of a fairly sudden change in the Thomson coefficient as the Curie point was traversed. The magnitude of the effect was such that it could account for the whole change in the specific heat of nickel at that temperature. They therefore suggested that the elementary magnetic carriers are precisely the same electrons which account for metallic conduction of electricity. These suggestions have further been discussed by Stoner, who concludes, however, that ferromagnetism is not due to free electrons.

The contribution which P. Weiss and R. Forrer made to the discussion was a valuable extension of our knowledge of the ferrocobalts and the nickelcobalts. It will be remembered that the Weiss magneton was originally put forward on the basis of measurements of the saturation intensities of these materials at very low temperatures. The new experiments were made at ordinary temperatures, and, it should be mentioned, involved a double extrapolation in obtaining the saturation intensities at low temperatures. Twenty-five ferro-

cobalts and nine nickelcobalts of different compositions were examined. The stable structure for alloys containing up to 78 per cent. of cobalt was the centered cube (α ferro-cobalt), from 78 to 95 per cent. the face-centered cube (γ ferro-cobalt), whilst from 95 to 100 per cent. it was a hexagonal lattice of maximum density (H ferrocobalt). With alloys with 0 to 13 per cent. and with 50 to 78 per cent. of cobalt the magnetic moment varied with the composition in a strictly linear manner, but with 13 to 50 per cent. the variation was non-linear. On producing the linear portions they intersected at a point corresponding to the alloy Fe^2Co and a magnetic moment of 13 Weiss magnetons, the slopes of the two lines being equal and opposite. The first line interested the axis at the known pure iron point with 11 magnetons, and if the other constituent was assumed to be pure cobalt, the line on being produced to the ordinate corresponding to 100 per cent. cobalt gave an intersection at 17 magnetons. On producing the second line, intersections corresponding to forms of 100 per cent. iron and 100 per cent. cobalt with 15 and 9 magnetons were obtained, respectively. For the alloys with 78 to 95 per cent. of cobalt a straight line was obtained, which, on being produced, gave 8·67 and 14 magnetons for pure cobalt and iron respectively. Hence the authors considered that the experiments indicated that atoms of iron and cobalt are capable of assuming several different atomic moments, which are not always associated with changes in the crystal lattice. Thus, in alloys of different composition, α iron was found with moments of 11 and 15 magnetons and γ iron with 14 magnetons. Similarly, cobalt in a centered cube ferro-cobalt—which must be regarded as α cobalt, a form which pure cobalt does not assume—possessed moments of 17 and 9 magnetons. Cobalt in a face-centered structure, *i.e.* γ cobalt, possessed a moment of 8·67 when alloyed with iron, and 9 magnetons when alloyed with nickel. For alloys with 0 to 68 per cent. cobalt, the nickel-cobalts crystallised in a face-centered cubic lattice (γ nickel-cobalts), whilst with 68 to 100 per cent. cobalt, the alloys, which were magnetically hard, crystallised in a hexagonal crystal lattice of maximum density (H nickel-cobalts). Two lines corresponding to these two sets of alloys were obtained; the first line gave an extrapolation value of 3 Weiss magnetons for pure nickel, and 9 magnetons for pure cobalt, whilst the second line gave 4 and 8·5 magnetons respectively, and although the value 4 was not regarded as highly accurate it was considered certain that nickel possessed two magnetic moments in the nickel-cobalts. Incidentally, Kaya (*Tok. Sci. Rep.*, vol. 17, p. 1158, 1928) found 8·5 magnetons for a pure crystal of cobalt.

The contributions from the Cavendish included a description of Kapitza's experiments with very strong magnetic fields, a mathematical investigation of the change of volume at the Curie point, by R. H. Fowler and C. F. Powell, and a paper on magnetostriction and the change of resistance of single crystals of iron and nickel, by W. L. Webster. The latter provided a qualitative explanation of magnetostriction phenomena in the unsaturated magnetic state. We may look upon an unmagnetised crystal of iron as a collection of a large number of small elements, each fully magnetised, but yet so orientated at random along the 100 directions that there is no resultant moment, and the magnetisation we produce by an applied field is really the result of a change of direction of some of these elements. Three distinct processes may thus be brought into play—firstly, a reversal of magnetism ; secondly, a change from one axis to another ; and, thirdly, a departure altogether from a cubic axis. The first two require only a small magnetic force, but the third process, bringing into play a strong molecular field, requires an applied field of considerable magnitude. The first two are responsible for the initial rapid rise of the familiar I, H curve, whilst the third is responsible for the further continuous change that is observed as the field is increased. With an applied field along a cubic axis only the first and third processes can be effective, but with applied fields along 110 and 111 directions, discontinuities in the I, H curve should occur when the forced deviation from the cubic axis towards the direction of the applied field begins to become at all pronounced. We should therefore expect discontinuities at magnetic intensities corresponding respectively to $1/\sqrt{2}$ and $1/\sqrt{3}$ of the saturation intensity in these two cases. This was borne out by Webster's experiments, and he used the above conceptions in a discussion of the magnetic phenomena observed with iron. For the three simple directions in an iron crystal the relative increases of length with intensity of magnetisation showed characteristic behaviours. Along the direction of easiest magnetisation, the 100 direction in iron, there was a progressive increase in length with increase in magnetisation, along the 111 direction no change was recorded until approximately half saturation was attained, when a rapid contraction set in and increased with the intensity of magnetisation, whilst along the 110 direction it appeared that some kind of compounding of the two effects took place. Akulov (*Zeit. für Phys.*, vol. 52, p. 389, 1928, and vol. 53, p. 582, 1929) has put forward the view that, when the direction of magnetisation does not coincide with the 100 direction, the crystal lattice is distorted. This would explain the difference in length of unit rods parallel to the several

crystal directions when completely magnetised. The increase of length which is observed on magnetisation parallel to the cubic axis is explained by assuming demagnetised iron to be made up of small asymmetric elements arranged randomly, so that a demagnetised cube, whilst symmetrically constructed, has not the same dimensions as a magnetised crystal, which must show an expansion parallel to the cubic axis of magnetisation and a lateral contraction when the small elements are aligned. This view permits a satisfactory explanation of the experimental results. It is interesting that along the 100 direction there is practically no change of resistance on magnetisation, in spite of the large change in length. A small change of resistance in a transverse direction is, however, found to occur.

The writer of this article contributed a review of the present state of our knowledge of the specific heats of ferromagnetic substances. According to the Weiss hypothesis of the internal molecular field inside a ferromagnetic substance, such a substance should possess an additional specific heat corresponding to the work necessary to demagnetise the substance, given by $-\frac{1}{2}\frac{N}{\rho J}\left(\frac{dI_0^2}{dT}\right)$, where N is the constant of the molecular field, I_0 the intensity of the intrinsic magnetisation, ρ the density of the substance, J the mechanical equivalent of heat, and T the absolute temperature This expression was not adequately tested until Sucksmith and Potter carried out their experiments on the specific heat of pure nickel. Later, although less exact, experiments on iron by Klinkhardt confirmed their findings, *viz.* that although there was an intimate connection between the specific heat of a ferromagnetic body and its intrinsic magnetisation it was not that predicted by the Weiss theory, and that the specific heat showed pronounced changes over a considerable range of temperature above the Curie point. Experiments made on the specific heat of manganese arsenide have shown that it possesses a very sharp maximum at 42·2° C., exactly at that temperature at which the value of dI_0^2/dT is a maximum. The numerical value of the maximum specific heat agreed very well with that required by the Weiss theory. There was no doubt, however, that the specific heat curve and the curve of dI_0^2/dT with temperature showed quite pronounced differences. The results were examined in the light of the Heisenberg theory of ferromagnetism as elaborated by Fowler and Kapitza (*Proc. Roy. Soc.*, vol. 129, p. 1, 1929). Assuming that a ferromagnetic atom possessed one magnetisable electron, Fowler and Kapitza showed that the additional specific heat in the neighbourhood of the magnetic critical point should be given by an expression

of the form $f(T) \cdot dI_0^2/dT$. At the critical point $f(T)$ apparently shows no pecularities, so that, as on the Weiss hypothesis, the maximum of the specific heat curve and the maximum of the dI_0^2/dT curve should coincide, which is satisfactorily supported by the experiments on manganese arsenide. The numerical relations on the Heisenberg theory, however, were not so satisfactory. Fowler and Kapitza predicted a "step-down" in the value of the specific heat at the magnetic critical point, of amount a little less than 3 cal. per gm. atom per deg. C. Actually, in the case of nickel a value of 1·7 is observed. In the case of iron, however, Klinkhardt's value is 8·0, so that the simple theory does not apply. In the case of manganese arsenide the value of the "step-down" is of the order of 100 cal. per gm. mol. per deg. C., *i.e.* at least thirty times that required by the theory, which was, of course, not designed for substances containing dissimilar atoms. Hence it was emphasised that an adequate theory of the specific heat of ferromagnetic substances must explain, firstly, the continuous change in specific heat as the substance passes through its magnetic critical point; secondly, the existence of pronounced specific heat changes at temperatures considerably beyond those at which the external magnetic properties disappear; thirdly, the magnitude of the change in the case of manganese arsenide; fourthly, the coincidence of the maximum value of the specific heat with the maximum value of dI_0^2/dT, and finally, and perhaps only as a point of lesser importance, the relatively high value of the minimum specific heat of a ferromagnetic substance above its magnetic critical point.

GEOLOGY. By G. W. Tyrrell, A.R.C.Sc., Ph.D., University, Glasgow.

Tectonics.—R. Staub's book, *Der Bewegungsmechanismus der Erde* (Berlin: Gebr. Borntraeger, 1928, 270 pp.), is a valuable contribution to world-tectonics in its careful assembling of facts and theories, notwithstanding that the general mechanism which is suggested for continental movement and mountain-making is not convincing. A review of the work will be found in *Geol. Mag.*, LXVI, 1929, pp. 43–5.

The second edition of L. Kober's *Der Bau der Erde* (Berlin: Gebr. Borntraeger, 1928, 499 pp.) is a much enlarged and almost entirely rewritten work (see Science Progress, January 1924, pp. 379–80). The main theme of the book is the contrast between the kratogens and the orogens; in other words, between the rigid blocks and the flexible strips of the earth's crust. A review will be found in *Geol. Mag.*, LXVI, 1929, pp. 187–8.

Prof. A. Holmes has laid all geologists under a debt of

gratitude for his remarkably thorough and useful review of the hypothesis of continental drift, based on the most recent literature (*Mining Mag.*, April–June 1929, Reprint, pp. 16). He again calls attention to the theory of convection currents in the sima beneath the continents (see SCIENCE PROGRESS, July 1929, p. 19) as a possible mechanism for lateral movements.

In his paper, "On Continental Drift and Cognate Subjects," Dr. R. H. Rastall (*Geol. Mag.*, LXVI, 1929, pp. 447–56) makes some pointed remarks on the necessity for mathematical investigation of the mechanics of nappe formation, and on the premature application of mathematical theory to geological "impossibilities," such as continental drift. He also points out that while the existence of land bridges between continents can be proved on biological and palæontological evidence, no one has yet squarely faced the problem of the mechanism by which such great land masses have disappeared. Dr. Rastall also again briefly sketches his theory of the origin of the continents, namely, that the highly heated earth was stripped of two-thirds of its outer skin by the formation of the moon, leaving the remaining one-third, as more or less solidified continental masses of sial, floating in a molten substratum in the region antipodal to the Pacific hollow left by the loss of the moon. This view obviously allows scope for the lateral movement of continents on the lines advocated by Taylor, Wegener, and Daly.

A Russian view of the Wegener hypothesis of continental drift and related topics is provided by M. Bogolepow's paper on "Die Dehnung der Lithosphäre" (*Zeitschr. d. Deutsch. Geol. Ges.*, **82**, 1930, pp. 206–28). Bogolepow is an enthusiastic advocate of Wegener's views, and of the periodicity of geological events. He holds that, "die Entstehung der jetzigen Kontinente und Ozeane ist allein durch Dehnungs- und Verschiebungsvorgänge zu erklaren."

In a provocative paper, entitled "Shall we return to Cataclysmal Geology?" Prof. E. W. Berry (*Amer. Journ. Sci.*, XVII, 1929, pp. 1–12) blows a wholesome breath of scepticism on some recent developments of tectonic theory. He is convinced that belief in the rhythm or periodicity of geological events is a return to the exploded catastrophic philosophy of Cuvier and d'Orbigny. He considers three questions, and comes to a negative answer in each case. Is orogenesis a single relatively-short event? Is it periodic and world-wide? Has it demonstrably affected the palæontological record? Prof. Berry advocates an extreme type of uniformitarianism, and believes that, "the slowly shifting geographic pattern, through its influence on climate, was a more general and more potent factor in evolution than orogenesis."

In a very important paper, Dr. J. S. Lee discusses some characteristic structural types in Eastern Asia, and their bearing upon the problem of continental movements (*Geol. Mag.*, LXVI, 1929, pp. 358–75, 413–31, 457–73, 501–22). As a whole Eastern Asia shows a southward movement against the Pacific on one side, and the Indo-Tibetan massif on the other. At the same time it tends to rotate clock-wise about Tibet. A tectonic system, founded on the structural types (indicated by Greek letters to which the trend-lines have some resemblance) of Eastern Asia, is applied briefly to the whole world. The writing is so compressed that it is difficult to summarise Dr. Lee's views at all adequately. He believes (with Staub) that the oceans, especially the Pacific, behave as resistant blocks, and that powerful orogenic movements are restricted to continental borders. He does not agree with the hypothesis of continental drift, but believes that the Atlantic has been somewhat widened. The energy for these rotational and other movements, and for orogeny in general, is derived from change of the earth's rotational speed, due probably to condensational changes in the barysphere. An increase in speed of the barysphere might result in differential movement between the barysphere and the crust, leading to diastrophism. Dr. Lee shows that the conception of alternate tectonic evolution and revolution fits in well with this theory. Dr. Lee seems to have first stated these views in a paper entitled, "The Fundamental Cause of Evolution of the Earth's Surface Features" (*Bull. Geol. Soc. China*, **5**, 1927, pp. 209–67).

Comparing North America and Asia in regard to Tertiary diastrophism, F. B. Taylor (*Bull. Geol. Soc. Amer.*, **39**, 1928, pp. 985–1000) comes to the conclusion that North America, since the late Cretaceous, has undergone much less lateral movement than Asia, and has also moved much less deeply in the earth's crust. Hence its mountain ranges are weaker than those of Asia, and have only a faint arcuate expression. There is an almost complete absence of fore-deeps and arc-seas. Taylor believes that Asia has moved south and south-east, in conformity with his theory of continental movement towards the equatorial regions. Though an independent author of the hypothesis of continental drift Taylor disagrees with Wegener on many points, especially in regard to the direction of movement of Asia, and the causes of the Pleistocene ice-sheet.

Notwithstanding that it will be reviewed later, it is convenient to mention the symposium on *The Structure of Asia* (London: Methuen, 1929, 227 pp.), edited by Prof. J. W. Gregory, in this place. The greater part of the book is occupied by a memoir on the stratigraphy and tectonics of the Iranian ranges by Dr. H. de Böckh, Dr. G. M. Lees, and Mr. F. D. S.

Richardson, of the A.P.O.C. geological staff. Prof. Gregory contributes a unifying introduction to the volume, in which he supports Suess's view that South-eastern Asia includes two great orogenic systems, the Altaid and the Alpine-Himalayan. Suess's plan, however, has to be extended farther east than he applied it. He also states, basing on the work of Mushketov and others in Turkestan, that the dominant Asiatic movement was from north to south, and thus agrees to some extent with Taylor.

In his discussion (in English) of volcanism in the geological history of the Tian-Shan mountains, V. A. Nikolaiev (*Mém. Soc. Russe Min.* (2), **68**, Livr. 1, 1929, pp. 117–37) tabulates the cycles of igneous activity as: I, Proterozoic cycle (or cycles); II, First or Lower Palæozoic cycle; III, Second Palæozoic cycle; and IV, Post-Palæozoic cycle—the most incomplete. Basic lavas, especially spilites, predominate in all cycles, except the last. In III keratophyric lavas are very abundant. Granito-dioritic plutonic rocks are prevalent in the first three cycles, but there are local appearances of alkali- and nepheline-syenites in III. The general "Pacific" character of the igneous rocks throughout is consonant with the tectonics of the Tian-Shan orogen. The magmatic history of the Tian-Shan appears to differ from that of Alpine orogens in the insignificance of volcanism in the Post-Palæozoic cycle.

Prof. G. B. Barbour has described the geology of the Kalgan region, on the borders of China and Mongolia, in an exhaustive and beautifully illustrated memoir (*Mem. Geol. Surv. China*, Ser. A, No. 6, 1929, 148 pp.). The stratigraphical succession is Archæan, Neo-Proterozoic (Sinian), Cretaceous (with acid igneous rocks), Oligocene (a widespread basalt formation), Pliocene, and Pleistocene. The diastrophic and physiographic histories of the region are treated very fully. A chapter on the problem of wider correlations is provided, wherein Prof. Barbour cites other circum-Pacific regions in which the diastrophic and igneous events exhibit parallelism with Kalgan, suggesting the action of a common controlling agency affecting the entire Pacific basin.

In their memoir on "The Structure and Correlation of the Simla Rocks," G. E. Pilgrim and W. D. West (*Mem. Geol. Surv. India*, LIII, 1928, 140 pp.) reject the older views of the structure of these rocks, in which a straightforward stratigraphical sequence was upheld. They find that the Simla metamorphic rocks are really much older than the Simla slates and Blaini beds, and have been thrust over the latter along a nearly horizontal thrust-plane. Furthermore, the sequence has been repeated several times by a series of recumbent folds. It is claimed that this view of the structure is more in accordance

with the Alpine type of folding that the Himalayas undoubtedly display than the older views.

A good review of the structure and tectonics of Australia, accompanied by an excellent coloured geological map of the continent, has been given by W. Geisler (*Geol. Rundschau*, XXI, 1930, pp. 109–23). Similarly Dr. E. Krenkel has contributed a useful discussion of the structural elements of South Africa (*Centralbl. f. Min.*, Abt. B., 1930, pp. 211–24), and Dr. A. Born a valuable analysis of the Cape fold-systems (*Zeitschr. d. Deutsch. Geol. Ges.*, 82, 1930, pp. 193–206). The following is Dr. Born's summary of tectonic and igneous events :

Post-orogenic volcanism . . .	Stormberg	Karroo
" Molasse " formation . . .		Karroo
Orogeny without volcanism . .	Beaufort	Karroo
Filling of geosynclinal without geosynclinal volcanism . . .	Ecca Dwyka	Karroo
	Witteberg Bokkeveld Table Mt.	Cape
Discordance		
Folded trough basement with ancient orogenic intrusions . . .	Transvaal or Nama System with old granites.	

Dr. A. L. du Toit describes the great 300-mile long, north to south, volcanic belt of the Lebombo Mountains in South Africa, where it forms the eastern border of the Union (*Trans. Roy. Soc. S. Afr.*, XVII, pt. 3, 1929, pp. 189–217). It consists of a basalt-rhyolite-basalt succession of Upper Stormberg age, which has the enormous maximum thickness of 9,000 metres. Intrusive into the sediments and lavas is a swarm of doleritic dikes trending north and south ; and in the lower basalts are felsite dikes, and large linear bodies of dolerite, gabbro, and granophyre. Dr. du Toit regards these intrusions as the feeders of the lava flows, the volcanism being of the fissure type. Crustal tension in an east to west direction is thus indicated during the eruptive period. The structure of the range is that of a monoclinal flexure, the growth of which took place within the middle (rhyolitic) period of volcanism. Both the extrusive and intrusive suites can be regarded as products of a single basic intracrustal magmatic reservoir within which differentiation had occurred.

Prof. P. G. H. Boswell has performed a notable service to geological science in translating Prof. F. Heritsch's book on *The Nappe Theory in the Alps* (London : Methuen, 1929, 228

pp.). The book is an effective counterblast and corrective to extreme forms of the Alpine nappe theory now prevalent. A review of the work appears in the *Geol. Mag.*, LXVII, 1930, p. 277.

In his paper entitled, "Grundzüge im Charakterbilde der epeirogenen Bewegungen Skandinaviens und des Baltikums im Kambro-Silur," Dr. H. Frebold (*Geol. Rundschau*, XIX, 1928, pp. 81–105) discusses the tectonic relations between the Oslo region and the Baltic shield region during the Cambro-Silurian. Cyclical sedimentation was dominant in the Oslo region during this era, but an abundance of limestone was characteristic of the Baltic shield. The undations of the latter were manifested also in the Oslo region where, however, the subsidence due to the development of the Caledonian geosynclinal was superposed upon them. Both regions reacted by uplift to the three orogenic phases, which occurred during the era, at the end of the Ordovician (Taconic), prior to the Downtonian (Ardennian), and at the end of the Downtonian (Erian).

In a paper dealing with the general geology of a pre-Cambrian area in Southern Norway, A. Bugge ("En Forkastning i det Syd-Norske Grunnfjell," *Norges Geol. Undersök.*, No. 130, 1928, p. 124, English summary, pp. 102–24) describes an enormous band of friction-breccia, 50–300 metres wide, which, with one interruption due to a fault, runs for 350 kms. from Gjovik to Kristiansand. This band of breccia cuts many different rocks, and throws off many short branches. It appears to mark a main geological boundary between the Archæan Kongsberg-Bamle formation to the south-east, and the younger pre-Cambrian Telemark formation to the north-west. New movements took place along the same line after the Caledonian orogeny, whereby part of the friction-breccia became the western boundary of the Oslo trough.

Dr. C. E. Wegmann (*C.R. Soc. géol. de Finlande*, 1929, pp. 101–29) illustrates methods of drawing profiles, and making block diagrams of tectonic structures in Archæan regions, by examples derived from his recent work in the Finnish pre-Cambrian (see SCIENCE PROGRESS, July 1929, p. 23).

In an important series of papers, Prof. E. B. Bailey, Prof. Th. Vogt, and Dr. T. L. Tanton give an account of the new methods of field study of folded rocks which have already led to some changes of views. As a consequence of a traverse of the Ballachulish district in 1924, Prof. Vogt has shown that the previously recognised stratigraphical order of the members of the Ballappel Foundation must be reversed ("On the Chronological Order of Deposition of the Highland Schists," *Geol. Mag.*, LXVII, 1930, pp. 68–73). Dr. Tanton then briefly describes various methods which have been put forward in recent years

for the "Determination of Age-Relations in Folded Rocks" (*ibid.*, pp. 73–6).

In his own contribution, entitled "New Light on Sedimentation and Tectonics" (*ibid.*, pp. 77–92), Prof. Bailey discusses *graded* and *current* bedding as the two main criteria of original order of superposition in folded rocks. The application of these criteria, or rather the criterion of current bedding, to the Ballachulish district has resulted, not only in the reversal of the order of succession formerly thought correct, but also in showing that the recumbent fold of Ballachulish is a syncline indicating movement towards the north-west, and that the Ballachulish slide is a lag.

"Graded bedding and current bedding are the distinguishing marks of two different sandstone facies." Greywackes, which often show graded bedding, are dealt with in some detail. They are regarded as due to accumulation in rather deep waters, the sandy material having been supplied as a result of sea-quakes.

In describing the Inverbeg lamprophyre dike on the western side of Loch Lomond, Sir J. S. Flett (*Summ. Prog. Geol. Surv. for* 1928, pt. II (1929), pp. 29–35) notes that its numerous inclusions which are, in general, strongly contact-metamorphosed, include garnetiferous and staurolite-bearing schists of a much higher grade of metamorphism than the adjacent Ben Ledi schistose grits. The garnetiferous schists closely resemble the Loch Tay schists, which outcrop farther to the north. The staurolite-schists, however, cannot be matched in the western Highlands, and Sir J. S. Flett regards them as pointing to the probable presence of a mass of "Older Granite" underlying part of the Loch Lomond region. If, as seems likely, a highly metamorphic series underlies the Loch Lomond area, the sequence of the schists may be an ascending one from north to south, as was maintained by J. Nicol; and latterly by Prof. J. W. Gregory. It may be mentioned that the writer of these notes has recently found similar inclusions in what appears to be the continuation of the Inverbeg dike on the eastern side of Loch Lomond.

BOTANY. By Prof. E. J. Salisbury, D.Sc., F.L.S., University College, London.

Morphology.—Skutch (*Amer. Jour. Bot.*, April 1930), in a study of the development of the Banana leaf, finds that the very young leaf is a rapidly tapering structure, in the lower part of which the two halves of the future "lamina" develop independently as structures intercalated between the midrib and the hyaline margin. The upper region of the young leaf rudiment forms the so-called precursory appendage, which the

author regards as a vestigial petiole. It should, however, be pointed out that the very early differentiation of this structure would appear to suggest that it may represent the morphological equivalent of the lamina of the Dicotyledon which on the phyllode theory of the Monocotyledonous leaf is absent from most members of the group.

Anatomy.—J. Leandri (*Ann. des Sci. Nat.*, T. xii, f.1, 1930) has examined the anatomy of representatives of most of the genera of Thymeliaceæ. The species from regions with rainfall during the cold season and drought during the vegetative period (Cape, Mediterranean, S.E. Australia) exhibit leaves with a thick cuticle which averages 6 μ (in *Passerina* spp. this attains 10 μ) and commonly with numerous double cells in the epidermis. The stomata are surmounted by a ring of thickening around the pore, and are often sunken. The axis shows numerous pericyclic fibres, a small pith, and numerous vessels (*ca.* 200 per sq. mm.). Similar features are met with in species from Tibet, the Kalahari Desert, and the Sahara. The species from oceanic islands, the Amazon region, and Central America, growing in a warm humid climate, differ in having a very lacunar mesophyll, a simple epidermis, and a thinner cuticle, but the salient anatomical features of the axis appear to be independent of climatic conditions. Four types of vascular organisation are distinguished. In *Drapetes* and *Octolepis* there are no medullary bundles. In *Pimelea* these only occur in the stem. In *Daphne* spp. such bundles are present in the stem and in the leaf trace also, whilst in *Aquilaria* such extend into the leaf itself.

Ecology and Distribution.—The mountain element in the flora of the Polish plain is the subject of an interesting monograph by Szafer (Krakow, 1930), who finds that this is chiefly comprised of species which have their main concentration at altitudes between 600 and 1,250 m. or between 1,600 and 1,900 m. One might have expected that the sucessive altitudinal floral zones would have contributed to an increasing degree as one descended, and that this is not so strengthens the conclusion, arrived at on other grounds, that the majority of these upland species of the plain are relics from a more extended distribution, mainly during the second glaciation. The list includes a number of British species, amongst which may be noted especially, *Allium ursinum*, *Carex pendula*, *Cirsium heterophyllum*, *Euphorbia amygdaloides*, *Gentiana verna*, *Geranium phæum*, *Leucojum vernum*, *Luzula sylvatica*, *Phyteuma orbiculare*, *Primula acaulis*, *Sorbus aria*, *Taxus baccata*, and *Veronica montana*. Whilst the majority are held to be relics of the later glaciation, a few are considered to be of pre-glacial origin (*e.g.* *Helianthemum canum*), and a larger number post-

glacial. The last comprise a number of forest plants, whereas the oldest relics are chiefly rock, meadow, and steppe species which invaded the Polish plain prior to the advent of conditions favourable to forest.

An extensive monograph has just appeared by H. del Villar on the soils of Spain which is of considerable interest to the ecologist. These soils show a considerable range as regards organic content, carbonate content, and reaction. The last varies from *p*H 6·1 to *p*H. 7·3. Some of these soils show a very high degree of leaching of calcium which, expressed as CaO, ranges in amount, in the surface layer, from 0·0 to 31 per cent. Six series are distinguished, *viz.* Tourbeuse, Sialitique, Alitique, Calcaire, Alcaline, and Alluviale. Detailed analyses are given of a number of each type, in which the various strata are distinguished (*Les Sols Mediterraneens etudies en Espagne*, p. 222, Madrid, 1930).

B. D. Wilson (*Soil Sci.*, May 1930), studying the translocation of calcium in acid soils that had been treated with lime, finds that exchangeable calcium was fixed in the second and third foot below the layer of soil that had been treated. Although the quantities involved were very small, they were nevertheless adequate to cause marked improvement in the growth of Sweet Clover. Such results indicate that the flush effects so often observed in nature may well be due to the water-borne salts, although these may not be present in detectable quantities.

Cryptogams.—The biology of various wood-rotting fungi has been studied on artificial media by S. R. Bose, who finds that the pores in *Trametes* are formed parallel to the direction of gravitational force, and that the stimulus of gravity determines the formation of the hymenium on the under surface. Reversal of the orientation of the fruit body results in the development of pores on the morphologically upper surface. Light tends to check growth and bring about sporophore formation; this latter being almost always associated with poor mycelial growth. The cultures further confirmed the observations of Lutz, that the restriction of wood-rotting fungi to particular hosts is probably due to the absence of inhibitory substances (*Jour. Linn. Soc. Bot.*, April 1930).

Ivimy Cook and Schwartz have studied infection by *Plasmodiophora brassicæ*, and find that this always takes place through the root-hairs. Swarm spores enter the root-hairs, where each forms a zoosporangium, the liberated zoospores from which migrate into the tissues of the host. Here the zoospores fuse in pairs, and give rise to plasmodia (*Phil. Trans. Roy. Soc.*, 1930).

In an account of the Californian species of *Vaucheria* by Hoppaugh (*Amer. Jour. Bot.*, May 1930), fourteen species are

included, of which one *V. longicaulis* is new. This species exhibits five to eight or even twelve lateral projections on the antheridia, which open by terminal round pores.

The fourth part of Vol. I of the *Handbuch der Seefischerei Nordeuropas* contains an account of the attached plants of the seas of Northern Europe by Nienburg. This includes a useful summary of the geographical distribution of the marine algæ, of which six types are distinguished, *viz.* North European (*e.g. Fucus vesiculosus*, *Ascophyllum nodosum*, *Chorda filum*, etc.). Arctic species (*e.g. Monstroma grœnlandicum*, *Lithothamnion fœcundum*), Arctic-French (*e.g. Elachista fucicola*), Southern (*e.g. Chætomorpha crassa, Scinaia furcellata*), Southern-Norwegian (*e.g. Bryopsis plumosa, Halidrys siliquosa*), and the West European-Baltic species (*e.g. Polysiphonia violacea, Pelvetia canaliculata*). A map is furnished of one example of each of these types of distribution.

Cytology and Genetics.—The chromosome of various diœcious species have been studied by Lindsay (*Amer. Jour. Bot.*, February 1930), who finds that in *Bryonia dioica* (n=10), in *Clematis virginiana* (n=8), *Smilax herbacea* (n=13), and *Carica papaya* (n=9), there is no evidence of an unequal pair of chromosomes in the pollen mother cells. This is probably true also of *Menispermum canadense* (n=26).

In the same journal for March Reeves reports a study of the ovule of *Medicago sativa*, from which he finds that the archesporium may be one or several cells. The embryo sac may occasionally be six-nucleate through failure of the second division at the chalazal end. Further, the antipodal cells disintegrate at an early stage before the female gametophyte attains maturity.

The chromosomes of *Rhododendron* are found by Sax (*Amer. Jour. Bot.*, April 1930) to be very uniform in character, with thirteen as the haploid number. Two cases of tetraploidy are described (*R. canadense* and *R. calendulaceum*).

A study of the chromosome complement of Pyrus by Darlington and Moffett shows that the somatic number in the majority of cultivated varieties of apple is 34, though a few, such as Bramley's Seedling and Blenheim Orange, have 51. The basic number would, therefore, appear to be 17, but this is regarded as secondary in origin, since of the thirty-four chromosomes present in most varieties there are seven types, of which four are represented four times and three are represented six times (*Jour. Genetics*, May 1930). In the same number of this journal, Crane and Lawrence report the results of experiments upon the fertility of apples which appear to show that the odd multiple polyploids are relatively infertile. The offspring of the triploids, whether derived from

selfing or crossing with diploids, are lacking in vigour, presumably owing to their aneuploid constitution. Also these same varieties exhibit a lower proportion of good pollen as compared with the apples with thirty-four chromosomes. The varieties with fifty-one chromosomes include, in addition to the two already mentioned: Baldwin, Bell de Boskop, Crimson Bramley, Genet Moyle, Gravenstein, Reinette du Canada, and Ribston Pippin. The evidence as to Lane's Prince Albert is conflicting, but the varieties with variously reported numbers between 34 and 51 are thought to belong here.

In the *Journal of Genetics* for April, Gregor describes five growth forms of *Plantago maritima*, occurring in the wild state, forming a series from completely prostrate types with peripheral inflorescences to wholly erect types with generally distributed inflorescences. All the specimens were obtained from an area comprising grass and exposed rock. The former was characterised by a prevailingly erect population with a range of vegetative height of from two to ten inches, whilst the exposed rock was characterised by a prevailingly prostrate type with a vegetative height of from one to eight inches.

Taxonomy.—The Australian species of the genus *Frankenia* have been the subject of a critical revision by V. S. Summerhayes, who recognises no less than forty-five species from this region, of which total twenty-four are new. He finds that the different species present a series of ovular arrangements, from the multiovulate condition with parietal placentation as found in *Frankenia pauciflora*, D.C., to the uniovulate condition with basal placentation as found in *Frankenia serpyllifolia*, Lindl. Both sepals and petals vary in number from four to six, the stamens from three to eight, and the carpels from two to four. The tendencies indicated in the genus are towards reduction in the number of ovules and aggregation of the flowers into heads (*Jour. Linn. Soc. Bot.*, April 1930).

PLANT PHYSIOLOGY. By Prof. Walter Stiles, Sc.D., F.R.S., The University, Birmingham.

Permeability.—For the determination of the laws governing the passage of substances into plant cells, investigators are turning more and more to the fresh-water algæ *Chara* and *Nitella*, as these plants contain cells of such size that a quantity of sap sufficient for analysis can be obtained from a single cell, while the fact that these plants normally live in an aqueous medium has also been advanced as a reason for their suitability for work of this kind. The marine alga *Valonia* has formed a subject of much investigation for similar reasons. D. R. Hoagland and A. R. Davis had previously reported that the sap of *Nitella* cells consists chiefly of a solution of salts with a

conductivity approximately equivalent to that of a decinormal solution of potassium chloride, and that, moreover, the principal ions are present in the sap in a much higher concentration than in the water in which the plants grow. These earlier results they now confirm ("The Intake and Accumulation of Electrolytes by Plant Cells," *Protoplasma*, **6**, 610–26, 1929). Thus the sap was found to contain about 104 milli-equivalents of chloride per litre, while the pond-water in which the plants were growing contained only about 1 milli-equivalent. Potassium was present in the sap to the extent of from 43·5 to 59·1 milli-equivalents per litre, but the pond-water contained only 0·051 milli-equivalents of this ion. The hydrogen ion concentration of the sap was approximately constant at *p*H 5·2, and so long as the cells remain alive variations in the external medium between *p*H 5 and 10 have no influence on the hydrogen ion concentration of the sap.

Increasing the intensity of illumination was found to have a considerable influence on the accumulation of bromide in the cells. Thus increasing the intensity of illumination five times doubled the amount of bromide absorbed from a solution of potassium bromide. There appears also to be a residual effect of illumination, for an accumulation of bromide was observed to take place in the dark if the cells had been exposed to sufficiently intense illumination previously. This effect of light does not appear to be due to a change in protoplasmic permeability, since an increase in this latter might explain a more rapid rate of absorption, but not the mechanism by which the ion is accumulated. Whether the effect is related in any way to photosynthesis is in doubt.

The effect of the presence of one ion on the accumulation of another was also studied. Chloride and iodide ions retard the accumulation of bromide, but sulphate, nitrate, and phosphate have no appreciable effect in this direction. It would appear that only anions, which themselves will accumulate rapidly in the vacuole, can retard the accumulation of other anions. The kation associated with the anion is also important in regard to the accumulation of the latter. For example, bromide accumulates more rapidly from a solution of potassium bromide than from one of sodium bromide, and more rapidly from sodium bromide than from calcium bromide.

Owing to the high value of the electrical conductivity of the cell sap, to the approximate equivalence of simple kations and simple anions, and to the osmotic relationships of the cell, Hoagland and Davis do not think that adsorption is responsible for the accumulation of ions in the cell.

This accumulation of ions in the cell sap of *Chara ceratophylla* has been observed by Runar Collander ("Permeabili-

tätsstudien an Chara ceratophylla. I. Die Normale Zusammensetzung des Zellsaftes," *Acta Bot. Fennica*, **6**, 20 pp., 1930). Thus potassium is present in a concentration sixty-three times that of potassium in the sea-water in which the plant grows, the concentration of nitrate in the sap is about eighty times that of this anion in the external medium, while phosphate is possibly more than four hundred times as concentrated in the sap as in the external medium. The accumulation of the other common anions is not so striking, although definite. Collander thinks it possible that the accumulation of kations may be brought about in some way by an interchange of these ions with hydrogen ions produced in cell metabolism, and that anions may accumulate in the same way through an exchange of these ions with anions present in the cell. No doubt an interchange of ions is possible, but it is not made clear how a diffusion of an ion against its concentration gradient is possible.

The problem of the accumulation of ions has also been attacked by S. C. Brooks (" The Accumulation of Ions in Living Cells—a Non-equilibrium Condition," *Protoplasma*, **8**, 389–412, 1929) by means of experimental work with *Valonia macrophysa*. Cells of this plant were analysed after exposure for various lengths of time to sea-water in which the concentration of potassium had been altered. It was found that the proportion of potassium in the sap tends to increase both when the concentration of potassium in the surrounding sea-water has been increased and when it has been diminished. At the same time the concentration of sodium in the sap increases with a slight change in the concentration of the sea-water or during a short period of exposure, but with longer exposure or in more greatly modified sea-water the sodium concentration diminishes. The concentration of chloride appears to run parallel with that of potassium, but the changes are smaller.

The theory put forward to explain these results supposes the outer surface of the protoplasm is a mosaic of (1) areas permeable to kations and practically impermeable to anions, and (2) areas permeable to anions and practically impermeable to kations. Inside this membrane carbon dioxide is being constantly produced, so that carbonic acid is formed which dissociates into hydrogen and bicarbonate ions; these ions therefore come to be in considerably greater concentration in the cell than in the exterior sea-water. The hydrogen ions will tend to diffuse out into the sea-water through the kation-permeable parts of the membrane, but since they are unaccompanied by an equivalent quantity of anions, other kations must enter the cell in equivalent amount. In this way the accumulation of any ion in the cell sap can be explained, equilibrium

being obtained when the ratio of its activity in the sap to its activity in the sea-water is the same as the ratio for the hydrogen ion.

In the same way the bicarbonate ion is being exchanged for the chlorion of the sea-water.

It would appear that if the permeability of the membrane to kations increases, this effect is greater for potassium than for sodium. This is explained by assuming a porous structure for the protoplasmic surface. The permeability of the surface for either kations or anions is then determined by the diameter of the pores and the sign and magnitude of the electrical charges at the surface of the pores. The general result of this condition will be to exaggerate the difference between the mobilities of the penetrating ions.

Further experiments on the penetration of methylene blue into living cells are recorded by Matilda M. Brooks (" Studies on the Permeability of Living Cells. X. The Influence of Experimental Conditions upon the Penetration of Methylene Blue and Trimethyl Thionine," *Protoplasma*, **7**, 1–16, 1929). Here again the material used consisted of cells of *Nitella* and *Valonia macrophysa*. Spectrophotometric analyses show that the dye penetrating into the cells is methylene blue itself, and not an oxidation product such as trimethyl thionine as had been suggested by M. Irwin, although trimethyl thionine enters *Nitella* much more rapidly than does methylene blue. The rate of entry of methylene blue varies with the hydrogen ion concentration of the solution, but the same equilibrium value is reached in all cases with equality of concentration within and without the cells.

The absorption of lead by the roots of bean plants (*Vicia faba*) has been investigated by S. Prát (" The Absorption of Lead by Plants," *Preslia*, **6**, 72–8, 1928). He found that normal healthy plants absorbed very little lead from nutrient solutions containing lead chloride, but that plants damaged by removal of the cotyledons absorbed all the lead from the solutions in a few days and grew very little, although when transferred to a normal nutrient solution they continued to grow. Prát therefore agrees with Osterhout that vitality means ability to resist unfavourable influences, and that the permeability of protoplasm can be taken as an indicator of its vitality.

The absorption of lead from a 10^{-4} M solution of lead chloride was retarded by calcium chloride in a concentration of 10^{-3} M. This conclusion is in opposition to previously expressed opinions.

K. Höfler and A. Stiegler have examined the permeability of different cells of the same plant (" Permeabilitätsverteilung

in verschiedenen Geweben der Pflanze," *Protoplasma*, 9, 469–512, 1930). The species employed was *Gentiana Sturmiana*, and the substance employed for the tests was urea.

It was found that the permeability varied considerably from tissue to tissue. The most permeable cells are those of the epidermis of the red flower stalk. These cells increase in the urea content by 0·02 to 0·10 M when placed in a molecular solution of this substance. The permeability of the cortical parenchyma and pith cells of the stalk is somewhat smaller. The permeability of the cell of the petals is smaller still, being only from $\frac{1}{20}$ to $\frac{1}{40}$ that of the stalk epidermal cells. In the root also the cells of the piliferous layer are more permeable than those of the cortex.

AGRICULTURAL PHYSIOLOGY. By JOHN HAMMOND, M.A., School of Agriculture, Cambridge.

Growth and Under-nutrition.—Within the last few years a number of books have been published which deal with various phases of the problem of growth and which bring together the at present very scattered literature on this subject.

In *The Chemical Basis of Growth and Senescence* (Philadelphia, 1923), Robertson has expounded his autocatalytic theory of growth, in which he has explained many of the apparently unrelated phenomena of growth : the autocatalysis which occurs during nuclear synthesis forms the basis of the theory. The effect of differentiation in promoting senescence is due to the fact that it inhibits nuclear synthesis : as one tissue after another becomes stabilised during the process of autocatalyst accumulation (which limits the dimensions of every cellular community), it bears the date of its stabilisation upon the dynamic equilibra within the nuclei, these being set at a high level in the older cells and at a lower level in the younger cells in proportion to their ontogenetic youthfulness. Starvation tends to rejuvenate the organism in two ways, in the first place by reducing the ratio of cytoplasm to nucleus, and in the second place by preferentially diminishing the tissues of low metabolic rate and thus leaving a greater proportion of tissues of high metabolic rate, which are usually tissues of an ontogenetically early type and which form a large proportion of the most physiologically essential organs.

In *Fasting and Under-nutrition* (New York, 1923) Morgulis has surveyed the chemical, physiological, and morphological results of inanition. While long-continued under-nourishment may not only retard bodily growth and cause mental inferiority, it is the growth impulse and not the quantity of consumed food which plays the leading rôle. Growth which ensues after a preliminary inanition is not unlike embryonic growth in its

intensity: the weight gained by previously starved animals on refeeding is greater in proportion to the food eaten than that gained by normally fed animals of the same age.

In *The Effects of Inanition and Malnutrition upon Growth and Structure* (London, 1925) Jackson has brought together the literature dealing with this subject in relation to plants and invertebrates, as well as dealing in detail with the effects on the different organs and tissues of the vertebrates. The thymus responds so promptly and extensively to conditions of malnutrition as to justify fully Simon's designation of it as a "barometer of nutrition." While some parts such as the reproductive organs are particularly susceptible to malnutrition, others, such as the skeleton, heart, kidney, lungs, and stomach, are much more resistant, and these in the young growing animal may even increase in weight at the expense of other tissues during starvation. The brain in young animals grows persistently in weight during severe submaintenance, but is injured in some way so that it may not be able to recuperate fully when subsequently placed under good conditions of nutrition. The curve of loss in body weight as a whole is logarithmic in type, resembling an inverted growth curve. Total inanition is usually fatal in a much shorter time than partial inanition with water; in birds, however, there is usually but little difference. Resistance to inanition increases progressively from birth to maturity. Resistance in the young is lessened by the smaller storage of reserve food materials, and the food requirements are also relatively greater in the young, for the needs for growth as well as for maintenance must be supplied: it is quite possible for a young growing organism to starve to death on a diet sufficient to maintain the body without loss of weight.

In *Growth* (Yale Univ. Press, 1928), a series of lectures delivered at the University of Missouri, Robbins stresses the importance of water and temperature on the growth and the effect of light in influencing the proportions of the plant. Brody, who deals with growth and senescence, suggests that three mechanisms constitute the internal factors for growth as distinct from the external factors regulating it: these are—the inherent force residing in the cells to grow indefinitely (as has been shown by tissue culture work), the growth-retarding influences resulting from the finite universe in which the cells or organisms find themselves, and the hormonic mechanisms. Hogan discusses the relation between growth and nutrition; in describing the deficiency diseases of growth such as rickets he remarks that as there is a geographical distribution so there is also a seasonal influence. He points out that the weight of the young can be influenced only by very severe underfeeding of the mother: with regard to postnatal under-feeding experi-

ments he concludes that if the rate of growth is retarded the growing period will be lengthened. Jackson, who deals with growth and form, points out that in fish (Hecht, *Anat. Rec.*, 7, 1913) the permanent adult form and proportions are reached relatively early, and that this occurs only in vertebrates with indeterminate growth, *i.e.* with no fixed limit of adult size, whereas in animals having determinate growth the external form changes continually during the period of growth, and as soon as the form becomes constant growth ceases. He accepts the classification of body cells into permanent, in which cell division ceases early in prenatal life (nerve cells); stable, in which cell division continues for a longer period sometimes even up to the adult stage (most organs); and labile, in which cell division continues throughout life to balance loss (epithelial and blood cells). He also distinguishes three periods of growth for the body in general—the embryonic period by numerical increase of cells; a stage during which some cells divide and cease, and then undergo differentiation and enlargement; and a final period, when growth is almost entirely by hypertrophy alone. Greene summarizes the physiological factors regulating normal and pathological growth, in particular the relation of the internal secretions to growth, such as the excessive skeletal growth associated with increase of anterior pituitary substance.

In a series of bulletins (Univ. Missouri, *Agr. Exp. Sta., Res. Bulls.* 96 to 105, 1926–7) Brody *et al* have given a summary of investigations on growth and development with special reference to domestic animals. Growth curves and formulæ are presented for body weights and measurements of cattle, fowls, and other farm animals, grouped for the most part in accordance with Robertson's theory into the self-accelerating and the self-inhibiting phases of growth. A comparison is also made between the senesence of the gland and body as a whole, and the senescence of the mammary gland during the course of lactation.

Meat Production.—In *Farm Meats* (New York, 1923) Helser has summarised the literature dealing with factors which determine quality in meat and the relative values and composition of the different joints. In *Meat and Meat Products* (Philadelphia, 1925) Tomhave, among other things relating to the practical handling of meat, gives the carcass yields from different types of animals; the proportion of this to live weight increases with the age and the weight of the animal. In *By-Products in the Packing Industry* (Chicago, 1927) Clemen has described how the scientific utilisation of the by-products of meat has developed and formed a source of profit to the industry, which would otherwise have had to raise the price of meat to

the consumer. In *Beef Production in Great Britain* (Liverpool, 1928) Wood and Newman have given an account of an investigation into the chemical composition of the different joints in steers varying in age from fifteen to thirty-six months old, and in different stages of fatness. They find, among other things, that a bullock fattened and killed at three years old has consumed 22½ lb. of food dry matter (of which only 1¼ lb. is concentrated food) per lb. of saleable meat produced, whereas a baby beef animal killed at eighteen months old has consumed 11¾ lb. of food dry matter (of which 2½ lb. is concentrated food) per lb. of saleable meat produced. Although the young animal produces meat from less food than an older one, it requires about twice as much concentrated food: the supply of cheap concentrated foods is therefore the important problem in beef production. In *Meat through the Microscope* (Chicago, 1929) Moulton has described the way in which science is assisting the meat industry, the Institute of American Meat-packers and the University of Chicago co-operating in the project. Valuable sections summarising the work done in the chilling and freezing of meat are included, together with illustrations of the muscle fibres under different conditions of freezing. In chilling (1–3° C. in twenty-four hours) 2–2½ per cent. of moisture is lost, for the most part from the surface, which appears to become sealed over with a dry tissue, and helps to protect the carcass from the action of bacteria; fat surfaces also offer poor growing conditions for bacteria. In freezing slowly the greater part of the water goes through the muscle sheath and freezes between the muscle fibres; when rapidly thawed this water is not well reabsorbed by the fibre, and as a result a red serum oozes out of the meat as it is cut up for use. When muscle is frozen quickly enough, however, no water will separate outside the individual muscle fibres; such muscle will not drip on being thawed and cut up. The drip is proportional to the cooling rate, and is nil only when freezing is practically instantaneous. Under the same conditions the drip is less for veal than aged beef, and is less with mutton than with beef. There are many practical difficulties in obtaining quick freezing, and recent methods for overcoming these are described. Experiments are reported on the use of sodium nitrite, which promises to supersede sodium nitrate in the curing of meats; higher temperatures speed up the rate of curing, but increase the incidence of spoilage. Treating of the by-products which are prepared for use as food for animals, he states that the demand already exceeds the supply during many months of the year, and is rapidly increasing. He suggests a general law governing the deposition of fat in the body—that the temperature at which fat deposition normally occurs affects the composition of the

fat deposited; thus the kidney fat, being subject to higher temperatures than the subcutaneous fat, has a higher melting-point, this applying in general also to the fat and body temperatures of different species.

METEOROLOGY. By E. V. NEWNHAM, B.Sc., Meteorological Office, London.

Buchan's Cold and Warm Periods.—The large fluctuations of temperature that often occur in spring and early summer, and that not infrequently give variation over a range equal to that represented by the difference between the normal temperatures of January and July respectively, are apt to attract the attention of people who normally take very little interest in meteorology as such. The articles and correspondence that appear in the newspapers show a widespread belief that these periods of accelerated, or of retarded or even reversed, seasonal changes of temperature tend to begin and end on certain fixed dates, which were stated by Alexander Buchan many years ago. Buchan does not appear to have taken up the question as to whether the dates that he found to correspond to such cold and warm periods over a limited part of Scotland for a certain number of years were applicable to other parts of the British Isles, but he clearly regarded this tendency as a more or less permanent feature of the climate of the region studied by him. The paper summarised here[1] was written in consequence of a remark by Sir Richard Gregory during his presidency of the Royal Meteorological Society in 1929, that it was a pity that nobody had tested the long records of temperature available for London to see whether there is any ground for believing that Buchan's dates have an application to the climate of London.

It should first be noted, however, that, as anyone can verify for themselves, conditions that give rise to a cold or warm spell in one place often fail to do so at another place at no great distance. For instance, in early summer, northerly winds and clear skies will sometimes be accompanied by a pronounced cold spell on the north coast of Cornwall when the sheltered southern coasts, escaping the chilling effect of the sea owing to the north wind being a land wind, may get a mean temperature well up to the normal and day temperatures often much above the normal. This fact suggests that if a set of dates specially favoured by cold or warm weather were to be found for one place they could not be expected to apply equally well to another place differently situated. Further, the absence

[1] "Irregularities in the Annual Variation of the Temperature of London," by C. E. P. Brooks and S. T. A. Mirrlees, *Q.J. Met. Soc.*, October 1930.

of any evidence for the existence of such dates at one place could not be taken to prove that such are not to be found elsewhere. The question of the existence of a general climatic tendency of Buchan's kind can therefore be settled only by considering one place at a time, or at least one region having extremely closely correlated temperature throughout.

Before considering Brooks's and Mirrlees' results, it will be as well to note the dates given by Buchan. They are as follows:

Cold Periods.	*Warm Periods.*
February 7–14.	July 12–15.
April 11–14.	August 12–15.
May 9–14.	December 3–14.
June 29–July 4.	
August 6–11.	
November 6–13.	

Buchan described his cold spell as a period when temperature, instead of rising, remains stationary or retrogrades or falls for a few days at a more accelerated speed than usual; a warm spell as one when temperature instead of falling stops in its downward course or even rises, or rises for a few days at a more accelerated speed than usual, the standard of reference being presumably a curve of daily mean temperature without irregularities fitting the mean curve everywhere except during the cold and warm spells. It is of obvious practical importance in many ways to know whether there is observational support for this theory. The sudden set-backs in the normal seasonal advance of temperature that usually occur in the spring and early summer are liable, especially in May, to cause serious losses to fruit growers, and the recognition in advance of the most probable time of their occurrence would allow of precautionary measures being taken against frost. Moreover, the transplanting or "bedding-out" of plants that cannot withstand cold weather until there has been time for their roots to settle into the new ground could be timed in a suitable way.

Brooks and Mirrlees used the temperature recods for Kew Observatory for the fifty-nine years 1871 to 1929, and divided this material into two periods of thirty and twenty-nine years. These were studied separately. It was found that the recognition of cold and warm spells of the lengths suggested by Buchan would be difficult on curves obtained simply by plotting the mean temperature for each day of the year, and it was considered, and with apparent reason, that suitable smoothing would remove irregularities of shorter length without obscuring anomalies of the Buchan type. The method adopted

was to form overlapping means of five successive days, *e.g.* January 1–5, 2–6, 3–7, etc. To obtain a standard curve of reference resembling that used by Buchan, the monthly means for the whole fifty-nine years were analysed harmonically, a correction being applied for the varying length of the month. The first two terms, that is to say, the half-yearly and yearly harmonic components, yielded a curve that seemed suitable. This is shown in the accompanying figure, which contains also the graphs of the overlapping five-day means for the two periods. A careful inspection of these will show the following features, which may be described in the author's own words:

"The irregular five-day curves in general keep rather close to the smooth curve with three exceptions.

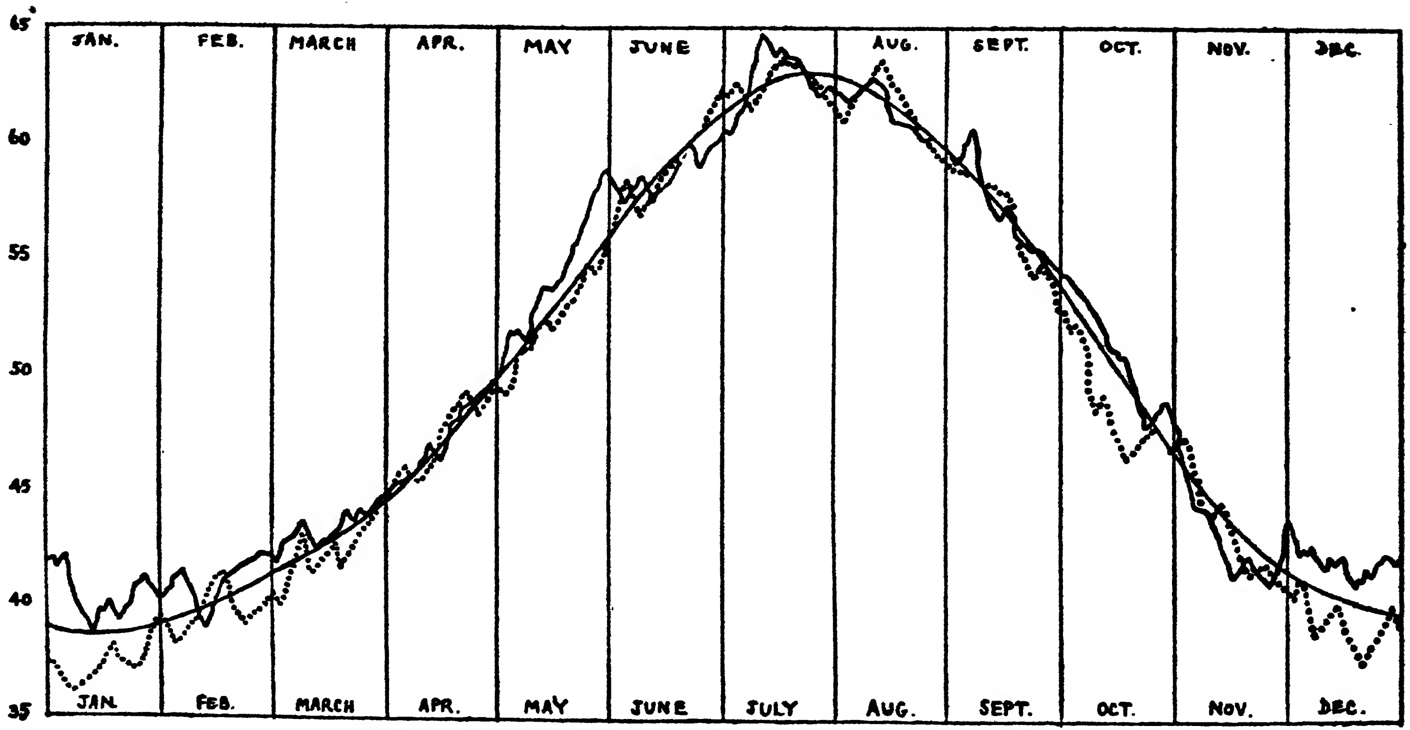
"The first exception occurs in December and January, when the two five-day curves become widely separated and do not cross in spite of quite considerable oscillations, which appear to be entirely independent in the two periods. This feature of the curves is associated with the prevalence of abnormally warm winters during the present century.

"The second exception occurs in the latter half of May. The curve for 1871 to 1900, though on the whole below the smooth curve, shows no striking anomaly, but the curve for 1901 to 1929 shows a period of abnormal warmth for the season. The curve from May 29–31 reaches a level which is not found again until June 17, . . . its complete absence from the curve for 1871 to 1900 shows, however, that it is not a permanent characteristic of our climate.

"The third exception occurs in most of July and the first half of August, and differs from the other two in being shared by both curves almost equally. In mid-July temperature rises to its maximum for the year, then falls to a secondary minimum at the beginning of August—better defined in 1871 to 1900, but still clear in 1901 to 1929—and rises again to a secondary maximum in mid-August. This feature is probably the most important on the diagram for the purposes of this study. . . .

"Of the six cold periods enumerated by Buchan, not a single one is in any way supported by the figure, but of his three warm periods, the first two, July 12–15 and August 12–15, both fall in maxima of both curves on the figure. There is, however, no support for a warm period on December 3–14."

The authors then go on to inquire more closely into the question as to whether some or any of the Buchan periods, or any other periods, can be identified in the London observations using suitable statistical formulæ for testing the reality of any warm or cold periods noted, and consider not only the Kew figures but those for Greenwich. They eventually come to the following conclusions:



The smooth, continuous line is the harmonically smoothed temperature curve for Kew, 1871–1929.
The dotted line shows the overlapping 5-day means for Kew, 1871–1900.
The irregular continuous line shows the overlapping 5-day means for Kew, 1901–1929.

(1) That there is no apparent tendency for any of the six periods designated by Buchan as cold periods, or for any of his three periods designated as warm, to appear regularly so in London.

(2) There appears, however, to be a definite tendency for the summer rise of temperature to cease towards the end of July, and to give place to variable temperatures without any upward or downward tendency until the autumn fall can be detected, about the middle of August.

(3) That there is probably no abiding tendency for any part of the year to be either abnormally warm or abnormally cold for the season, but that such tendencies appear to spring up and persist for, say, ten, twenty, or thirty years, and then to vanish.

(4) That while Buchan's cold and warm periods were proably true for Scotland in the 1860's they are certainly not true for London now.

It will be seen that, in the absence of any way of detecting when the lifetime of any given "tendency" is to be regarded as ended, no practical use can be made of anything of this kind that may be revealed by the analysis of temperature records for past years.

ARTICLES

FACTORS AFFECTING ADHESION PHENOMENA IN GLUED METAL OR GLASS JOINTS

By W. B. LEE, M.A., B.Sc., Ph.D., A.I.C.

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THE nature, range, and mode of action of the forces between atoms, ions, and molecules together constitute fundamental attributes of matter. A better understanding of the nature of adhesion and molecular forces should be of great interest in biological problems and in all branches of study which have to deal with actions at interfaces. A knowledge of the exact nature of the " force-fields " surrounding atoms and molecules should enable us to calculate the properties of matter that depend on these forces. A satisfactory theory of adhesion should, however, be built upon an experimental foundation.

The aim of this paper is to present a connected critical discussion of direct adhesion problems, and to weld into a homogeneous whole some of the isolated records appearing in the literature. The adhesion phenomena discussed below are mainly those which seem to be especially relevant in considering the conditions which must be satisfied by any acceptable theory of adhesion.

Adhesion is closely related to quite a number of subjects frequently treated as separate branches. Such are, for example, cohesion, surface tension, adsorption, static friction and lubrication, wetting, spreading coefficients, etc. In this review discussion will be devoted especially to *adhesion problems relating to glued joints between polished metal, glass, and quartz surfaces.*

The best source of information on the subject of " *cohesion* " is to be found in the collected papers of a cohesion symposium (*Trans. Farad. Soc.*, vol. 24, p. 53 *et seq.* (1928)). " *Adhesion* " phenomena receive a little attention in that connection, but no satisfactory review has yet appeared.

In 1875, Tyndall " *wrung* " together two Whitworth surface plates, and by suspending them in vacuo proved that the adherence between them was not entirely due to atmospheric pressure as had been previously supposed. Tyndall considered that the plates adhered owing to partial molecular contact,

since the surfaces of such plates were very true. At about the same time Stefan (*Chem. Centr.*, 1874, p. 369) carried out some experiments of a somewhat different nature which he described as experiments in "*apparent adhesion.*" He found that the force of adhesion between two polished plates depended upon their distance apart, their size, and on the nature of the fluid *in which they were immersed*. The force of adhesion was much greater in liquids than in air. The time required to pull the plates apart was inversely proportional to the force applied. Stefan concluded that when the separating force begins to act the distance between the plates is slightly increased and the liquid in the intervening space becomes dilated so that its hydrostatic pressure is decreased and liquid is "sucked in." He concluded that no equilibrium is established because the decrease of the hydrostatic pressure between the plates permits the external liquid to flow between them, and the difference of pressure is therefore diminished. The same actions are continually reproduced. Stefan's experiments are of great interest in view of some recent investigations by Hardy and Nottage (see below) which must also be described as experiments on "apparent adhesion."

In 1912, Budgett (*Proc. Roy. Soc. A.*, **86**, 25, 1912) repeated Tyndall's experiments on the "*wringing*" phenomenon, which represents *true* adhesion. He used hardened steel end-gauges with faces true to within one-millionth of an inch. He concluded that Tyndall's results must be attributed to traces of grease or moisture at the surfaces of the plates rather than to molecular attraction of the molecules of steel on one face by the molecules of steel in the opposite face. His results may be summarised as follows:

1. Breakage (*i.e.* under tensile stress) occurs in the liquid itself; not between liquid and steel.
2. Only 4 per cent. of the force required to rupture the film is due to surface tension.
3. The remaining 96 per cent. must be necessary to overcome the molecular cohesion of the liquid.
4. The tensile strength of water may, under special conditions, amount to sixty atmospheres.
5. The thinner the film, the stronger the joint.

It may be mentioned here that Budgett's values for certain liquid films (where comparison is possible) run parallel with Hardy's well-known static friction measurements. It appears that there is a limit to which the thickness of a film can be reduced by "wringing," and when this is reached the liquid will *seize* to the steel. The sliding and twisting of the blocks during "wringing" tend to further reduce the thickness by

spreading the liquid and this action is strongly resisted by the film, considerable force being necessary to move the blocks over one another. Corrections for atmospheric pressure were made, and it was proven experimentally that in no case did this exceed 7 lb./in. (The thin liquid film formed a neck just before breaking.)

Peters and Boyd (*Scientific Papers—Bureau of Standards*, No. 436, May 2, 1922) have shown that when two very plane surfaces are brought together in the way they describe, the separating film of alcohol is not more than one-millionth of an inch thick. Tests showed that two gauges with the ordinary lapped finish when brought into contact in the described way required to separate them a pull in the direction perpendicular to the surfaces of from 35 to 40 lb./in.^{2} With gauges possessing a high optical polish more intimate contact is possible, *i.e.* the film is much thinner—and the required separating force ranged between 95 and 100 lb./in.^{2} They found that a nick or burr on the edge or a small surface elevation which holds the two surfaces one hundred-thousandth of an inch apart made adherence almost impossible Two surfaces adhered when covered with a film of grease or with moisture from the hand. The thickness of these films was, however, a rather indefinite quantity, in most cases about three-millionths of an inch, and while considerable force was required to slide the gauges on each other they could be pulled apart by a force of 5 to 10 lb./in.^{2}

It was found in the National Physical Laboratory that if a gauge is made of a hollow cylinder and a rod ground most carefully to fit inside, and if the rod be smeared slightly with paraffin, there is no difficulty in inserting the rod into the cylinder. But it is essential that the motion shall be continuous. If the rod which is being pushed in or rotated quite smoothly is allowed to stand for a few moments, the whole goes solid and the rod can only be moved by a sledge-hammer combined with a twist, whereupon it suddenly goes free again.

Terzaghi, *Phys. Rev.*, **16**, 54 (1920), concluded that thin films of water between plane solid surfaces have a *semi-solid* character. A drop of water was placed between two glass plates and was allowed to evaporate. When the thickness of the layer of water was reduced to about 100 $\mu\mu$ the evaporation stopped completely. Terzaghi therefore suggested that the molecules of a solid are able to exert forces over distances of the order 50 $\mu\mu$, since such thin films of water have shear and tensile strength as well as enhanced viscosity. The writer has also found that it is difficult to evaporate pure liquids when in the form of very *thin* films of variable thickness between a lens of *very large* radius of curvature and an

optically polished plane surface. Directing a jet of the laboratory compressed air into the "joint" did not cause any appreciable evaporation even with a volatile liquid such as ether.

It has been found in the National Physical Laboratory that the force necessary to separate a pair of gauges when wrung together with paraffin oil is only about 30 lb./in.^{2} immediately after wringing, but it increases to about 80 lb./in.^{2} or higher when an interval of about one hour has elapsed after wringing. *N. P. L. Report*, p. 153 (1927).

Shereshefsky, *Journ. Amer. Chem. Soc.*, **50**, 2966–2980, 2980–2985 (1928), finds that the lowering of the vapour pressure of water is much larger than that calculated by Kelvin's equation. The large value is attributed to an increase in the surface tension of the liquid in the capillary. Shereshefsky believes the classical theory to be incorrect.

Sir William Hardy has shown that a thin film of lubricant is profoundly changed by the attractions of the bounding solid surfaces. If it was originally fluid it loses fluidity, and the strain set up by the traction in static friction experiments takes place in a medium which is peculiar in that it varies in state rapidly along the normal while it is homogeneous in a tangential direction. The view is taken that "boundary" friction is due to *cohesive forces* acting across interfaces. Langmuir's original view was that the molecular forces do not directly extend beyond 3 or 4 Å. This does not agree with recent adhesion experiments of Hardy and Nottage, McBain and Lee, Rolt and Barrell, Watson and Menon. (In the case of *liquid* adhesives between smooth solid surfaces, the observed effect may be due to a "chain effect." See below.)

According to Hardy, friction is due to the sum of the tangential components of the resistance to displacement and rotation of atoms and molecules. On this view it is found that the fields of the solids are not limited practically to a particular part of the molecule (*e.g.* the COOH group of a fatty acid), because the static friction is not independent of the nature of the solid. Hardy [1] was the first to suggest that the field of the solid surface is transmitted in gradually decreasing amount by the atoms of the chain becoming polarised so that the nature of the solid affects the friction. Similarly McBain and Lee found it affected the adhesion. Evidence for the orientation of molecules at interfaces or on a composite surface which Hardy considers as a factor determining static friction is found in chemical substances which are readily polarisable, *e.g.* acids provide the changes in surface

[1] Cf. also Hardy (*Proc. Roy. Soc. A.*, **86**, 631, 1912) for the idea of "a strain transmitted from molecule to molecule."

energy and there is contact P.D. between the film of a composite surface and the matter on which it lies.

It is exceedingly difficult to prepare metal surfaces which are so smooth and plane that they give contact over an appreciable area. Using modern surface-gauges of great smoothness, Rolt and Barrell (*Proc. Roy. Soc. A.*, **116**, 401, 1927) have shown that contact occurs only over a portion of the area and that after several " wringings " there is still a removal of metal from the spots in highest relief, this wear disappearing only gradually. " Seizing " of metallic surfaces is probably confined to a few spots only on each surface.

Adsorbed films of air, even of dry air, are partially responsible for optically polished metal surfaces not cohering properly when pressed together. The writer has found that heating the dry steel pieces to a temperature of 100° to 200° C., which gets rid of some of this adsorption, improves the " contact " between the pieces when bringing them together. On the other hand, it is well known that a minute film of condensed water vapour aids the " contact " at ordinary temperatures. This is in accord with certain conclusions of Hardy and Nottage (see later). However, Hardy has shown that glass may be prepared in such a state of cleanliness that a spherical surface cannot be drawn across a plane surface of the same material without tearing taking place.

The strength of the joints made with pure chemical substances as adhesives between optically plane surfaces of steel, glass, or quartz is surprisingly high when the joints are prepared by " wringing." McBain and Lee (*Proc. Roy. Soc. A.*, **113**, 620, 1927) found that pure crystalline substances, such as highly purified " T.N.T.", fully rival many well-known adhesives in the strength of joint obtainable by them, provided the films are *very thin*.

They may yield joints between optically polished metal surfaces whose breaking strength may approach *one ton per square inch*. Pure liquids give results of a much lower order of magnitude even when they are only a few millionths of an inch thick. In all cases " the thinner the film, the stronger the joint." This rule holds for ordinary adhesives as well, provided that the film completely fills the space between the surfaces joined. There is direct parallelism between joint-strength and the mechanical properties of the materials joined. Joint-strengths rise with tensile strength and elasticity and fall with atomic volume and compressibility. Joint-strength is often much the same whether tested in tension or in shear.

Rolt and Barrell (*loc. cit.*) have found that the thickness of wringing films between very highly polished steel surfaces of high accuracy was 0·005 μ for paraffin oil, 0·007 μ for vaseline

oil, and 0·008 μ for ordinary lubricating oil. (It may also be recalled that Johonnot and Chamberlain found the limiting thickness of free films of a solution of sodium oleate in water to be 0·006 μ.)

When joint-strength tests are made in tension employing optically plane glass, quartz or metal surfaces " wrung " together with a liquid (which may or may not be subsequently solidified) rupture of the joint seems to occur in the liquid or solid adhesive itself, rather than at the metal-adhesive interface so that we obtain a " measure " of the internal pressure or cohesion of the thin film itself under the influence of the molecular fields of the opposing metal surfaces. Consider the metal-adhesive-metal joints MA_1M and MA_2M of the same dimensions and test them in tension. (M = metal, A_1 = first adhesive, A_2 = second adhesive.) Assuming rupture of the joints occurs in the *adhesives* : $\text{Adhesion}_1 >$ cohesion of $A_1 >$ measured tensile strength_1, say T/S_1. $\text{Adhesion}_2 >$ cohesion of $A_2 >$ measured tensile strength_2, say T/S_2. If $T/S_1 > T/S_2$ it does NOT follow directly that *adhesive* $\text{attraction}_1 >$ *adhesive* attraction_2. We are now faced with the problem as to whether we are dealing with a good lubricant or a good adhesive. A liquid with a high spreading coefficient, irrespective of the actual mechanism of spreading, may adhere to water or metal even though it has relatively low cohesion. This is the case with many lubricants. On the other hand, a liquid may have high cohesion and only a moderate spreading coefficient, but strong adhesion to metal. This is a suitable adhesive for joining two surfaces.

McBain and Lee have demonstrated that two optically plane surfaces of steel or quartz may be " wrung " together when they are *completely immersed in water*. A considerable amount of work has to be done in the " wringing " process because the adsorbed film of gas, or trace of foreign matter, has first to be abraded, or replaced by a liquid before adhesion is secured. This is of interest in connection with some recent experiments of Hardy and Nottage.

Hardy and Nottage (*Proc. Roy. Soc. A.*, **112**, 62, 1926) extending Stefan's experiments on " apparent adhesion " find that for a liquid joint between glass or steel surfaces (made, however, without " wringing ") *any* normal force, however small, provided only it is sufficient to overcome the relatively slight resistance offered by the surface of the liquid, will break the " joint."

Most of their experiments were made with a steel cylinder " floating " in a pool of liquid on a steel plate, the liquid having been allowed to replace air, which was first present. Identifiable reproducible values for the tensile strength of

the "joint" could, however, be obtained by breaking the joint "instantaneously." In the experiments of McBain and Lee, both published and unpublished, a time-factor of this nature was absent, and the joints which were made by "wringing" pure liquids between optically polished steel or quartz surfaces were quite rigid, and in some cases the tensile strength was three or four atmospheres, instead of an insignificant fraction of an atmosphere as in the "apparent adhesion" experiments of Hardy and Nottage. The contrast in these results is due to the fact that the surfaces were "wrung" together with a combined sliding and twisting motion in the experiments of McBain and Lee, whereas this was not the method used by Hardy and Nottage (cf. above).

A very striking result was obtained by Hardy and Nottage on "apparent adhesion." They found that a steel cylinder could be "float" (with its axis vertical) in clean dry air at a distance of 4 μ from an optically plane steel plate. In their experiments, the plane ends of the steel cylinder were also "optical" and carefully cleaned according to a routine method. If the surfaces were contaminated with "adsorbed" grease or smeared with the merest trace of palmitic acid this air-film between the surfaces was reduced to 1·7 μ. The work of Watson and Menon on surfaces contaminated with minute oil globules (*Proc. Roy. Soc. A.*, 123, **123**, 1929) supports that of Hardy and Nottage.

Watson and Menon found that the distance between the cylinder and plate does not vary greatly even if the air pressure in the container be reduced to 2 mm. of mercury. The results of Hardy and Nottage, and Watson and Menon are difficult to explain unless one assumes that the range of molecular attraction of the steel surfaces extends over thousands of molecular diameters, a difficult conception in this case, with air between the surfaces, where a chain effect in molecular orientation is improbable. When a *liquid* is drawn in between the cylinder and plate and sufficient added to form a large pool the level of the cylinder does not change, so that here again the liquid film may have a thickness of the order 4 μ. Here the *effective* large range of molecular attraction may be ascribed to a "chain effect." Watson and Menon suggest that possibly this film consisted "of a number of small oil globules between two adsorbed films of air or water vapour on the metallic surface" (*loc. cit.*, p. 192).

If pure air or a pure liquid be placed between the two plane surfaces of the cylinder and plate (if liquid, sufficient being added to form a large pool round the "joint"), and a weight be added to the cylinder it "descends" (according to Hardy) in the pool of air or liquid, but on releasing the

applied load it rises to its original position. This latter result is strongly supported by Hardy's classical investigations in boundary lubrication. These results are not to be expected and in fact do not occur (at least to any appreciable extent) if the steel cylinder and plate are "wrung" together so as to form a rigid body. A difficult question to which at present there is no very satisfactory answer is "why does the metal cylinder (weighing 5·6 grams) return to a distance of the order of several μ, and remain in this position upon releasing the external pressure after having pressed the pieces into 'metallic contact'?" A chain-effect in orientation appears improbable, when pure air is the "film" of "adhesive."

The writer has made some preliminary experiments on some of these phenomena and has found nothing which stands in contradiction to the results of Hardy and Nottage and of Watson and Menon.

Physicists frequently assume that the reason why two lenses or prisms are not in actual contact when one piece is placed upon another is that dust particles prevent contact. Hardy and Nottage claimed to have disposed of this objection by making their measurements in a clean chamber through which dust-free air was streaming, and used specially cleaned optically plane surfaces. Moreover, by carefully cleaning "optical" surfaces and bringing them together quickly even in "ordinary" air, it would appear improbable that sufficient "dust" accumulates to hold the pieces apart a distance of 4 μ. Hardy and Nottage have shown that the cylinder does not remain suspended in air owing to ordinary electric repulsion between surface charges.

The foregoing investigations on true adhesion and so-called "apparent adhesion" illustrate the rule that the method of using an adhesive is as important as the properties of the adhesive itself. A clear distinction should be made between two methods of preparing adhesive joints.

(1) "Wringing" the surfaces together with a combined sliding and twisting motion. (This is different from simply *placing* the two surfaces together with the liquid in between them.)

(2) Placing the two surfaces together and then allowing the liquid or molten adhesive to be "sucked in."

Method (1) is used in metrology measurements in testing end-gauges and leads to films of the order of one-millionth of an inch thick, when the surfaces used are optically polished. This is the method used in the extensive investigations of McBain, Hopkins, and Lee in forming joints between smooth polished surfaces.

Method (2) leads to much thicker films (unless the joint

is already under very high pressure at the time the liquid or molten adhesive is allowed to be "sucked in"). The films being, in general, thicker by this method, the resulting joints are much weaker.

Thin films or fibres of matter may possibly have higher true tensile strength than that of the same matter in bulk, whether adhering to surfaces or not. The classical experiments of A. A. Griffith on thin silica fibres (*Phil. Trans. Roy. Soc. A.*, **221**, 180, 1920) form a good illustration of this point. An alternative explanation more in favour is that these freshly formed surfaces may be freer from cracks.

The strength of joint obtainable with a pure crystalline chemical substance as the adhesive between optically plane metal surfaces depends upon its melting-point and its chemical constitution. The so-called long-chain compounds which have been studied are relatively poor adhesives compared with substances such as glucose, triphenyl carbinol, coumarin, and Rochelle salt. The *n*-fatty acids and their related alcohols are, however, better adhesives than the *n*-paraffins. Summarising we may say, that pure chemical compounds of different types show very different adhesive and lubricating power when used for joining polished metal surfaces, the strength varying in definite relation to chemical constitution. (Cf. McBain and Lee, Hardy and Nottage.)

It is interesting to note that monomolecular films of the higher fatty acids on water will resist compression of 10 dynes per cm. or more with very little collapse. This is equivalent to a compression of forty atmospheres on these extremely thin films. (The experiments of N. K. Adam in this connection are well known.) The adhesion between the ends of the molecules and the water must be very great to prevent buckling under this or larger compression. McBain and Lee, as also Hardy and Nottage, find the adhesion of the higher fatty acids to give results of from 100 to several hundred lb./in.^{2} between optically polished metal surfaces according to the film thickness. It thus seems that results of a similar *order of magnitude* are obtainable from Adam's experiments and those in adhesion tests between highly polished or metal surfaces. Adam's experiments, however, give the higher values.

McBain and Hopkins (*J. Phys. Chem.*, **29**, 197, 1925; **30**, 114, 1926; "Second Report of Adhesives Research Committee," London, 1926) showed that strong joints could be made between metal surfaces with many of the common adhesives. Shellac and its derivatives formed an outstanding group in which the joint-strength exceeded 2 tons to the square inch. (McBain and Lee find that the results depend appreciably

upon the rate of loading in the tests, being in general lower the lower the rate of loading.) They pointed out that when strong joints are obtained between polished metal surfaces or surfaces of optically polished glass, fused silica, or true crystalline surfaces such as quartz or calcite, true adhesion must be involved. They distinguished this as specific adhesion to contrast it with the other type which they showed frequently occurred with porous bodies—namely, mechanical adhesion, where the adhesive obtains a foothold through mechanical embedding in the irregularities and pores of the surface, even where in the extreme case the materials seem otherwise indifferent to each other. They suggested that any liquid material which wetted both surfaces and could be solidified *in situ* should constitute an adhesive. A scientific study of glued wood joints has been made by Browne and his collaborators at Wisconsin, by the Royal Aircraft Establishment of Great Britain, and by McBain, Hopkins, and Lee. The results obtained, though interesting and important, do not lend themselves so readily to the investigation of the really fundamental facts relating to adhesion as do studies involving the use of optically polished plane metal or glass surfaces.

Ollard (*Trans. Farad. Soc.*, XXI, Part 1, August 1928) deposited nickel electrolytically on the end of a steel rod in such a way that a tensile test could be made. In the testing process part of the steel surface was torn away, the adhesion between nickel and steel being 30 kg./mm.^{2} The electrolytic nickel itself had a tensile strength of 69 kg./mm.^{2} When the surface of the basis metal has been well cleaned, union is so perfect that the crystals of the electrolytic metal are continuous with those of the basis when the two metals are the same, *e.g.* copper on copper.

Crowe (*J. Soc. Chem. Ind.*, **43**, 65–70, 1924) measured the joint strength of various thicknesses of lead-tin solder between copper surfaces. The joint strength diminished rapidly with increasing thickness over the range of thickness studied, which did not, however, include extremely thin films such as those studied by McBain and Lee (*loc. cit.*).

The writer holds the following views of the nature of adhesion. These views are based upon a survey of existing knowledge of adhesion as well as upon the adhesion phenomena in *glued joints* reviewed above. They appear to be a necessary preliminary to the formulation of any satisfactory mathematical theory of adhesion.

It appears that in all these joint-strength tests the molecular fields of the solid metal, glass, or quartz surfaces are superimposed upon the ordinary cohesive forces of the thin liquid, or subsequently solidified film. This may possibly be true

independently of molecular orientation, molecular aggregation, or micellar formation, although these phenomena are almost certainly connected with the mechanism of adhesion. The position of fracture for a *liquid* in joint-strength tests would appear to be at the weakest place, viz. the middle of the film. The influence of the attraction fields of the solids extends right through ordinary adhesive films made by the process of wringing the surfaces together, although with the most accurate gauges the surfaces may be brought into *partial* contact. It seems that the *effective* range of molecular attraction in these liquid joints may be hundreds or thousands of molecules thick. This effect naturally diminishes rapidly with the distance, and accords with the experimental rule "the thinner the film the stronger the joint."[1] Such wrung films have high shear and tensile strength. The liquid film loses its mobility, has very low vapour pressure, high viscosity, and surface tension. It is in fact a quasi-solid. One explanation of the preceding facts is that the range of molecular attraction is small, but that the force-fields of the solid surfaces are transmitted right through the liquid film by a chain effect in molecular orientation. The second alternative is that there is, in these cases at least, a large direct range of molecular action, as visualised by some of the older physicists.

When the liquid film is solidified the position of fracture also seems to be in the adhesive film itself. For amorphous solids it is probably in the middle of the film. Hardy and Nottage, however, contend that in the case of *highly purified* (crystalline) substances allowed to *solidify in situ* fracture occurs close to one or both of the opposing solid metal surfaces which are united. They postulate that in such cases there is an adsorbed layer which may be only a few molecules thick, and is in an "undercooled" state. The middle portion of the film is crystalline and fracture occurs in the region separating the adsorbed layer and crystalline material. This does not mean that the range of molecular attraction of the metal or quartz surfaces is limited to the adsorbed layer. On the contrary it extends at least *in effect* right through the joint, as for a liquid. This is proven in Hardy and Nottage's experiments in which one piece was of steel and the other of copper. They contend that the break occurred only close to the copper surface, but the "adhesion" (or better, "cohesion") value was the exact mean of the values for the same pure chemical substances between steel-steel and copper-

[1] This rule has been tested experimentally for films of liquid down to only a few hundred molecules thick. The rule also holds for pure chemical substances, *e.g.* a *n*-fatty acid solidified between optically polished metal surfaces.

copper surfaces. A similar rule holds for Hardy's investigations on boundary lubrication.

The range of the *effective* sphere of action of the molecular fields of the metals, glass, and quartz seems to be different for different pure substances, *i.e.* the effective range of molecular attraction is a function of the nature of the adhesive, and of the surfaces united. This is the explanation of the relation between the strength of joint and strength of materials joined discovered by McBain, Hopkins, and Lee. The surface energy of the adhesive film at the position of fracture probably runs parallel with the surface energy of the clean solid surfaces themselves. The necessary data for the solid metals are not available, but the fact that the surface tension of the *molten* metals follows the order of the joint strengths with a given adhesive appears significant.

The writer considers that the theory of Langmuir in its original form is inadequate to explain adhesion phenomena in general, although the extension of it by Hardy, McBain, and Lee (chain effect in molecular orientation) is in complete accord with extensive investigations on liquid "joints" between polished metal, glass, or quartz surfaces. The conditions become more complicated when crystallisation of the adhesive occurs *in situ*. Here the directed forces of crystallisation overcome the force-fields of the solid surfaces at a definite distance away from the surfaces.

On the other hand, adhesion phenomena in "joints" could be explained by assuming a large direct range of molecular attraction such as is typified for example in the so-called "physical" theory of Edser (4th *Report on Colloid Chemistry*, London, 1922). There appears to be little doubt that at least with liquid adhesives the effect of the molecular fields of the opposing solid surfaces is transmitted right through the liquid film which in the *extreme* case (cf. Hardy and Nottage) may possibly be several μ thick. When we deal with liquids as in Quincke's well-known experiments we arrive at a large effective range of molecular attraction of the solid surfaces. The theory of monomolecular films on water is misleading when applied to the conditions obtaining in glued joints, and constitutes only a particular case.

When clean dry *air* is the "adhesive," Hardy considers that the force-fields of the steel surfaces extend over a large distance, and in general far beyond the adsorption layer. This is difficult to account for, except by assuming a relatively great range of direct attraction, *i.e.* without molecular orientation.

I am indebted to Prof. J. W. McBain, F.R.S., for his interest in this work, which is closely related to our joint publications on *Adhesives and Adhesion.*

Summary

1. A critical survey of existing knowledge of adhesion phenomena in *glued joints between plane polished surfaces* is presented for the first time.

2. The paper records the experimental basis which any acceptable theory of adhesion should explain.

3. In general, true adhesion phenomena in glued joints, both for liquid and solid adhesives, indicate that the *effective* range of molecular attraction may be hundreds of molecular diameters or alternatively that a chain effect in molecular orientation results which *simulates* a large direct range of attraction. The earlier, narrower form of Langmuir's theory, and likewise that of monomolecular films on water, tend to be misleading when applied to adhesion phenomena in *joints glued with pure chemical substances*, and constitute only one particular case of adhesion.

4. A few isolated experimental observations made by the writer, hitherto unpublished, are welded into the general scheme of adhesion phenomena.

Stanford University, California.

June 1st, 1930.

THE DISCOVERY OF THE GAS LAWS

By W. S. JAMES, M Sc., A.I.C.

III. THE THEORY OF GASES AND VAN DER WAALS' EQUATION

THE problem of the structure of matter has always been most persistent in arousing the curiosity and speculation of man, and especially has the riddle of the nature of air perplexed the philosophers and early scientists. Because air expands or contracts with change of pressure, any theory of the structure of solids cannot be directly applied to gases, but has to be modified and enlarged so as to account for this variable volume. Thus the seventeenth-century atomic theory, which explained the structure of solids and even liquids with highly satisfactory success, was only partially able to explain the behaviour of air. It must be remembered that the existence of gases other than air had not yet been established, and so air was regarded as quite an exceptional substance. This additional property of a variable volume called for some special explanation, and various hypotheses were constructed, such as rotating vortices, whirling springs, a funiculus, molecular repulsion, caloric, and the kinetic theory. But throughout this general flux of opinions there was one persistent idea which remained merely an idle speculation until 1846, when it was first definitely supported by experimental results, and later received clear expression in Van der Waals' equation. This central idea was that under a sufficiently high pressure the particles of a gas would come together, forming a solid mass whose volume would not decrease further with an increase of pressure—in other words, Boyle's Law would fail at high pressures. This piece of deductive reasoning was not supported by experimental work until Regnault in 1846 showed that Boyle's Law, hitherto regarded as being rigorously exact, was merely an approximation. The development of this idea up to 1873, when it was incorporated in Van der Waals' equation, will now be given in greater detail.

Descartes, always more of a philosopher than a scientist, had in 1637 explained the motion of the planets by his theory of vortices, and he applied his universal principle to explain

the behaviour of air. He visualised air as consisting of long slender particles, whirled round by matter of the first kind, each vortex trying, owing to its whirling motion, to force away the surrounding vortices and to expand, thereby maintaining a bigger vortex. The larger the vortex, the more slowly does the whirling motion become and the less does it resist the ingress of other vortices.

This theory must have influenced Boyle, for at the same time as he published his law, 1662, he gave the following explanation of the mechanism of rarefaction, together with two diagrams showing the air particle like a watch spring:

"Let us suppose then, the particles of bodies, at least those of air, to be of the form of a piece of ribbon, that is, to be very long, slender, thin, and flexible laminæ, coiled or wound up together as a cable, piece of ribbon, spring of a watch, hoop or the like, are; we will suppose these to have, all of them, the same length, but some to have a stronger, others a weaker, spring. We will further suppose each of these so coiled up to have such an innate circular motion, as that thereby they may describe a sphere, equal in diameter to their own. . . . By this circular motion the parts of the laminæ, endeavouring to recede from the centre or axis of their motion, acquire a springiness outward like that of a watch-spring, and would naturally fly abroad until they were stretched out at length; but that, being incompressed with the like on every side, they cannot do it without the removal of them, as not having room sufficient for such a motion."

If a gas expanded to occupy double its initial volume, Boyle imagined the individual coils to expand accordingly and then possess half their initial force of spring.

In one instance he supposed the diameter of the coils to be $\frac{1}{10^{11}}$ inches.

It appears from Boyle's theory of coils that his hypothesis would not be accurately obeyed at very small and very large pressures, as the coils could not go on expanding indefinitely or go on contracting indefinitely. We shall see that this was pointed out in 1702. With regard to large pressures, Boyle is content to state:

"For no man perhaps yet knows how near to an infinite compression the air may be capable of, if the compressing force be competently increased."

Boyle was, of course, aware of the fact that air expanded when heated, and performed experiments to illustrate this phenomenon. His explanation is, however, very interesting:

"For the atoms of fire flowing in in great numbers, and passing through with a very rapid motion, must needs accelerate

the motion of these particles (the coils of air); from which acceleration their spring, or endeavour outward, will be augmented; that is, those zones will have a strong nitency to fly wider open (for we know that the swifter any body is moved circularly the more do the parts of it endeavour to recede from the centre of that motion) from whence, if it has room, will follow a rarefaction."

It is interesting to note that even Boyle, who rebuked the Peripatetics and Spagyrists for their reliance upon authority, placed the responsibility for the statement that the particles have an innate motion upon Epicurus, who had previously asserted this principle.

From his considerations of the effects of heat on gases, Boyle was led to the following speculative suggestion, which is of especial interest to us, as it contains the germ of the kinetic theory of gases:

"I will allow you to suspect that there may be sometimes mingled with the particles that are springy . . . some others that owe their elasticity not so much to their structure as to their motion which, variously brandishing them and whirling them about, may make them beat off the neighbouring particles, and thereby promote an expansive endeavour in the air, whereof they are parts."

Mariotte's views have been mentioned in a previous article. He too thought that air consisted of coils or springs, and that if air was allowed to expand sufficiently it would arrive at its "extension naturelle." Air at the top of the atmosphere has its "extension naturelle," but near the surface it is compressed by the air on top of it. From which it follows, although Mariotte did not draw the conclusion, that his law would only be an approximation.

Newton in 1687 started the "static" theory of air, in which he put forward the view that air was composed of particles at rest kept apart by their mutual repulsion; and this repulsion accounted for the pressure a gas was capable of exerting. The nearer the particles were together the greater the force of repulsion, and consequently, the smaller the volume, the greater the pressure. Newton's theory was still fashionable at the time of the rise of the kinetic theory.

The first conscious attempt to disprove Boyle's Law deductively was in 1702 when, together with Amontons' "Memoire" in the Journal of the Paris Academy, was published a commentary upon it. Mariotte's doctrine of the air being composed of "petites lames à ressort, soit spirales" was reaffirmed, but it was pointed out that Mariotte's Law could not be exactly correct, since if the pressure became infinite, the volume could not become zero; for once the coil had assumed its maximum

compression, an increased pressure could cause no further diminution in volume. But it was recognised that such considerations would not affect the experimental results to any measurable extent.

Boerhaave considered that Boyle's Law would not be obeyed at high pressures owing "to the aqueous, spirituous, oily, saline, and other particles scattered through it," which are not elastic, and which would ultimately form a solid incompressible body.

As early as 1738 D. Bernouilli put forward some of the fundamental ideas in the kinetic theory of gases. He supposed the molecules to move in straight lines in all directions with great speed, and the pressure on the walls of the vessel was due to the impact of the molecules against it. Bernouilli deduced from this Mariotte's Law, assuming that the volume occupied by the molecules themselves was negligible compared with the space occupied by the gas. He remarked that if the particles have an appreciable volume, Mariotte's Law will cease to be accurate at very high pressures. The value of a hypothesis depends upon the capability of furnishing deductions which can be verified by experiment; and Bernouilli's theoretical deduction preceded its experimental verification by over 100 years.

Mussenbroek, in his *Cours de Physique*, 1769, stated that he found Boyle's Law did not hold for pressures greater than four atmospheres. He pointed out that on theoretical grounds one would not expect the law to be quite accurate; for if air was compressed until its parts touched, and so formed a solid mass, there would be no force in nature capable of reducing it to a smaller volume.

The idea of absolute cold was first introduced by Lambert in 1779, and at this temperature the volume of air becomes zero, "or as good as zero." Also at this temperature the particles of air fall together "and become, so to speak, waterproof," and it was this volume of the compact molecules which prevented the volume of the air space from entirely disappearing.

Properties of Gas explained by Caloric.—During the latter part of the eighteenth century, the caloric theory of heat was firmly believed in by the vast majority of physicists, the first blow against the theory being struck by Rumford in 1799, when he claimed from experimental work that heat was a form of motion. In spite of his sound arguments, the caloric theory was so firmly established that it was not before the experiments of Joule in 1842 that it was completely overthrown. During this reign of popularity caloric was used to explain the thermal dilatation and compressibility of gases. We get an excellent

glimpse of the ideas then held in Abbé Haüy's *Traité de Physique* of 1806:

"The elasticity of air depends on caloric, such that we can consider the caloric as forming little springs bent between the molecules of air."

In one passage he thinks when a gas is compressed a large quantity of caloric is expelled. In another he thinks it probable that the density of caloric is proportional to the spring (*i.e.* no caloric is expelled), although he admits he has no positive proof of this.

He found it easy to account for the expansion of a gas when heated at constant pressure—for the extra caloric supplied surely needed an extra volume; and if the gas was heated at constant volume then the extra caloric, being denied extra space, caused an increased "spring."

Thomas Thomson, in his *System of Chemistry* of 1807, said that philosophers had speculated a great deal concerning the cause of the elasticity of gases. After mentioning the view of the elasticity being due to the mutual repulsion of the particles, he added that the most prevalent view was that the repulsion resided in the substance called heat—a statement perhaps not quite as inaccurate as appears at first.

Early Attempts to establish Mathematical Formulæ.—The first person to arrive at a formula connecting pressure, volume, and temperature of a given mass of gas seems to have been Clapeyron in 1834. If P and V represent the pressure and volume at a temperature t, and Po and Vo the pressure and volume at to, then by combining Mariotte's Law with Gay-Lussac's, Clapeyron obtained:

$$PV = PoVo\,\frac{(267 + t)}{(267 + to)}.$$

Then by substituting a constant R in place of $\dfrac{PoVo}{267 + to}$ he obtained the equation—

$$PV = R\,(267 + t).$$

The figure 267 is obtained from Gay-Lussac's value of the coefficient, whose accuracy was undoubted at this time.

After having shown that Mariotte's and Gay-Lussac's Laws were not exactly obeyed by any gas, Regnault asked if it were not possible to express the relationship between pressure, volume, and temperature by another law. He added:

"Unfortunately, this appears very difficult in the present state of our knowledge; for this law must be rather complex, because it depends on many variables."

Had Regnault's theoretical insight and mathematical powers equalled his genius for accurate experimental work, he would have no doubt been able to devise from theoretical consideration a law whose predictions would agree with his experimental results. Being gifted mainly on the practical side, he was content to express his results by empirical formulæ.

Regnault's wide range of figures proved attractive raw material to many mathematical physicists who attempted to fit equations to his data. Apart from the inconvenient case of hydrogen, the product PV was less than what would be expected from the ideal equation PV = RT, and so many attempts to supply a correction resulted in an equation of the form—

$$PV = RT - e$$

where e was a function of the variables. But none was found to be completely successful.

In 1864 Dupré obtained—

$$P(V + c) = RT$$

as his fundamental gas equation. The value of c, the "co-volume," was positive for hydrogen but negative for other gases.

Avogadro published a paper upon the behaviour of gases in 1853, which contained the suggestion that the decrease in value of PV as P is increased is due to the approach to the liquefaction point, when the volume tends to decrease more than is expected. He explained the exceptional case of hydrogen by supposing that the effect of the sum total of the volumes of the molecules more than counteracted this tendency to contract when approaching liquefaction. This is perhaps the clearest anticipation of Van der Waals' idea that had been given up to 1853, but, in spite of his paper being translated and published in scientific journals in France and Germany, scant attention seems to have been paid to it.

Hirn's Equation, 1865.—A substantial advance was made by Hirn in the second edition of his *Théorie mécanique de la chaleur*, 1865. He showed that the variable volume of a gas is not the total volume, but the latter diminished by the volume of the atoms. Hirn contended that it was only the variable part of the volume which ought to enter into the expression of Mariotte's Law. Thus, if we neglect any internal pressure, we have—

$$P(V - b) = RT.$$

Hirn further introduced another factor to account for the effect of the internal pressure. The mutual attraction of the

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molecules diminished the pressure on the walls of the enclosure, for each molecule, in passing through the surface layer, was acted on by an attractive force directed towards the interior of the mass, by which its impact on the wall of the vessel was diminished. He introduced a term "p" to allow for this extra pressure which must be added to the external pressure. Hirn thus obtained the following:

$$\frac{(P+p)(v-b)}{T} = \frac{(Po+po)(Vo-b)}{To}$$

or

$$(P+p)(V-b) = RT.$$

It will be noticed that p was not treated as a constant but as a function of the variables.

Before the kinetic theory was generally accepted the prevalent idea was that the molecules or particles in a gas remained at rest, and the invariability of the distances between them depended on a repulsive force. The elasticity of a gas was regarded as being due to this molecular repulsion.

It is not within the scope of this paper to discuss the rise of the kinetic theory of gases and the labours of Clausius and Maxwell in perfecting it; or to relate the researches of Gay-Lussac, Meyer, and Joule upon the free expansion of gases; or the porous plug experiments of Joule and Thomson; but nevertheless these all had their direct effect upon the current theory of gases and upon the explanations that were offered to account for the deviations from the gas laws. Between 1857 and 1870 a complete change in outlook took place. The old "static" theory dating from Newton was completely overthrown by the kinetic theory of Clausius and Maxwell. Starting from certain assumptions connecting temperature and the average velocity of the molecules, both Boyle's and Gay-Lussac's Laws could be theoretically established on the new theory. After this triumph for the new ideas, attempts were made to make the new theory account for the deviations from the above Laws. The first of a long series of such attempts is that of Van der Waals who, basing his work on Clausius's equations, obtained in 1873—

$$\left(P + \frac{a}{V^2}\right)(V-b) = RT.$$

Without this fundamental change of ideas upon the structure and constitution of gases his equation would have been impossible, for it was on Clausius's calculations that he based his work. Although born of the new kinetic theory Van der Waals' equation appears much the same as Hirn's; the

variable "p" in the latter has become more clearly specified as $\frac{a}{V^2}$.

The new theory and equation were found capable of deductions relating to the critical point and liquid phase, deductions which agreed remarkably well with observation, and this startling success created confidence in the truth and finality of the new kinetic theory and Van der Waals' equation. A pragmatist philosopher might suppose that the theory was true because of its successful application, and that the essence of truth depended upon utility. But to the physicist of this period truth was an objective standard to which at last he was closely approaching.

But in its turn Van der Waals' equation has suffered the same fate as those which it was intended to correct, and it too, after a period of usefulness, has also been found wanting. Other attempts, all more or less successful, have been made to establish a formula, but no entirely satisfactory solution has yet been offered. Whether any such law or equation can ever be discovered, or indeed, whether any law in any branch of science can ever be said to represent reality wholly and completely, is a matter for conjecture.

"SYMBIOSIS: PROLEGOMENA TO THE STUDY OF ŒCOLOGY"[1]

By JAMES BAKER, B.Sc., A.R.C.S., University, Cambridge

Part I

SYMBIOTIC SYSTEMS IN GENERAL

"We want" (said Samuel Butler) "a measure which shall express, or at any rate recognise, the harmonics of resemblance that lurk even in the most absolute divergencies, and *vice versa*."

It is to this end that we shall attempt in the course of this essay to give form and precision to the notion of symbiosis.

Much of importance has hitherto been overlooked through concentrating the attention exclusively upon single relationships or groups of relationships. If we take a general view of symbiotic phenomena throughout the animal and plant kingdoms certain facts come into relief which might otherwise pass unnoticed or be regarded as merely fortuitous.

We find, for example, that wherever there exists a group of decided symbiotic relationships, in the more special sense, one or both of the participants usually shows capacity for entering into relations of a different or more general kind, commonly regarded as alien to the conception of symbiosis proper. The Ascomycetes, which are responsible for the formation of lichen thalli,[2] are notable as well for their out-

[1] A portion of a treatise on this subject.

[2] Nor are the algal constituents lacking in this respect, particularly the blue-green forms. Among these *Anabæna Azollæ* lives within the mucilage cavities of the leaves of Azolla; *Anabæna Cycadearum* inhabits the root tubercles of Cycas; certain *species of Nostoc* are found in hollows on the thalli of Blasia and Anthoceros; *Oscillospira Guillermondi* is a colourless form living in the cæcum of the guinea-pig. According to Bristol a symbiosis between the nitrogen-fixing bacteria and the blue-green algae of the soil is very probable (*Ann. Bot.* xxxiv. "On the alga flora of some desiccated English soils," p. 56, 1920).

Among the Chlorophyceæ: *Phyllosiphon Arisari*, one of the Siphonales, causes yellow spots on the leaves of *Arisarum vulgare*, while other species of Phyllosiphon are accorded as parasites on the leaves of various plants in Ecuador. The well-known *Chlorochytrium Lemnae* is an endophyte in the

standing success as parasites, both on other plants, and, to a lesser extent, on animals.[1] The same ants that protect and conserve their aphids as sources of nourishment have, within their own species, developed the "community" to an unparalleled degree of complexity.[2] The orchids, which have, of all plants, evolved the most specialised mechanisms for promoting insect cross-pollination are, in the majority of cases, so intimately associated with their mycorrhiza that they cannot reach maturity in the absence of the endophyte[3]; while the hymenoptera[4] and, in particular, the bees that have become, *par excellence*, the symbiotic partners of the flowering plants, share with the ants the distinction of priority in their social life.

It is just the failure to recognise how much there is in common between sexuality and gall-formation, the symbiotic complex and the disease, the cell-dominion and the society, that renders impossible any comprehensive grasp of organic inter-relationship. Let us consider, for instance, in this connection, the morphology of the ant community; it is well known that these insects produce, within the same nest, individuals of three types which are male, female, and neuter respectively, and that the neuter forms, called "workers,"[5] are responsible for carrying on all the everyday business of the nest: gathering building material, catching other insects and

leaves of the duckweed; while *Cephaleuros* (Trentepohliaceæ) is parasitic on the leaves of various tropical plants and causes a serious disease of the Tea (*Thea sinensis*).

[1] *Saprolegnia ferox* on salmon causes serious epidemics; *Aspergillus flavus* and *A. fumigatus* grow in the human ear, where they give rise to troublesome diseases; *Trichophyton* and other imperfecti cause ringworm, favus, and similar skin diseases; *Actinomyces* also gives rise to ringworm and to a disease of the tongue; *Laboulbeniales* on insects; etc.

[2] While there is reason to believe that the aphids themselves require the symbiotic co-operation of certain species of yeast. The latter constitute the so-called "green-bodies," and are probably handed down in the egg (*vide* Guillermond, *Yeasts* (translated F. W. Tanner), p. 125, "Symbiosis of Yeasts").

[3] Bernard (1900) showed that the seeds of *Cypripedium* did not develop at all under aseptic conditions; those of *Cattleya* survived for several months and showed some degree of tissue differentiation, but, lacking infection, developed no farther; *Bletilla hyacintha* would develop to a small plantlet consisting of a slender axis with a few green leaves, but no farther. The Japanese orchid *Gastrodia elata* cannot give rise to flowering shoots unless its tubers are infected by the rhinomorphs of *Armillaria mellea*.

[4] A further very important relation exists between the hymenoptera and the flowering plants in the form of *insect-galls*, which are chiefly associated with members of the Cynipidæ, or true "gall-wasps." The significance of these will be discussed later on.

[5] These "workers" are, in reality, imperfect females. Under certain circumstances they are capable of producing eggs, but, according to Lespes, these never come to maturity.

bringing them in for food, tending the young larvæ, and fighting any marauders that may chance that way. The workers are devoid of wings and, in certain species, are of more than one type, *e.g.* in the Mexican *Myrmecocystis* and the Australian *Camponotus*,, both of which genera have, as well as their ordinary workers, other neuter forms in which the abdomen is vastly swollen and serves as a honey reserve.[1] In the South American *Œcodoma cephalotes* there are actually three types of workers, so that the nest contains no less than five classes of individuals. But the division of labour in the ant community does not end within the limits imposed by the species itself: many ants are known that make slaves of other species, carrying off the larvæ or pupæ, and rearing them in their own nest. The duties of the workers are then taken over, either wholly or in part, by the "slave" species, which henceforth perform the functions of cleaning the nest and tending the larvæ of their masters. To the latter remains, as a rule, only the capacity of fighting, and in some genera even this form of activity has been lost. Even more remarkable, however, is the use that many ants make of animals that are quite unrelated to them. Of these the most important are aphids and beetles, from both of which there appears to be derived a sticky secretion much valued by the ants, in return for which the insects are carefully tended and their larvæ reared within the nest. The number of myrmecophilons species was estimated by Wasmann as 1,246 in 1894, but since this time the list has been enormously extended, and, according to Donisthorpe,[2] the figure must now be placed nearer 5,000. Märkel, moreover, satisfied himself that large nests of *Formica rufa* might contain at least a thousand aphids, so we see that these aliens form a very important and integral part of the ant community, and in many cases they have evidently evolved in relation to their new mode of life. Thus the two beetles, *Atmeles* and *Claviger*, often found in ants' nests, are blind, and the same is true of the primitive *Beckia*, an active little insect allied to Podura, and the "domestic" crustacean *Platyarthrus Hofmansegii*.

Without going more deeply into the question, it is easy to see that the ant community is not founded on any basis of genetic similarity or dissimilarity, but that the labour is distributed, without fundamental preference, among individuals that are either genetically identical or else very far removed from one another. There is, in fact, no distinction between their purely social activity, represented by morphological separation into males, queens, and workers and their

[1] *Vide* Lubbock, *Ants, Bees, and Wasps*, London, 1904, p. 19.

[2] Donisthorpe, H. St. J. K., *The Guests of British Ants*, London, 1927, p. xxiii.

specifically "symbiotic" activity, expressed in their relation to aphids and beetles.[1]

Nor is this all, for we can easily understand how, by disturbance of the balance between any group or set of individuals, a condition of mutual advantage is transformed into one of pure parasitism. This, indeed, is the state of affairs that obtains among the different genera of slave-making ants represented by the series *Formica sanguinea—Polyergus refescens—Strongylognathus—Anergates.*[2] Of these the first-named have not materially degenerated through their association with slave-species: they remain good fighters and have not lost the instinct for work, which they share in common with their slaves (in this case *F. fusca*). Both *Polyergus* and *Strongylognathus*, however, have become quite dependent on the species of *Tetramorium* that they respectively enslave, and by which they are both cleaned and fed; *Strongylognathus*, moreover, is no longer able even effectively to fight. In the last stage, represented by *Anergates*, the master ants depend exclusively on the workers of *Tetramorium cæspitum*, having no workers of their own, and being in every respect feeble and effete[3]—capable of no other independent act than that of reproduction (the one act, be it observed, that the Tetramorium workers are not capable of).

We see, then, that parasitism may arise by the disturbance of an equilibrium that originally conferred reciprocal advantage—a process that is not without its counterpart in the scale of human affairs. In particular has this type of parasitism arisen in connection with the sexual symbiosis, where one sex (as a rule the male) comes to be parasitic on the other. This peculiar state of affairs can best be described as "sexual parasitism," and, as we shall see, it is a feature of groups that may bear no sort of "taxonomic" relation to one another. In the parasite *Bilharzia*[4] responsible for the disease of that name, the male

[1] The genetic disposition of the ant community may accordingly be represented in the following manner:

Genetically identical	*Genetically similar*	*Genetically dissimilar*
males	slaves	aphids
queens		beetles
workers (of one or more types).		crustaceans, etc.

[2] An example of an ant very decidedly parasitic on another species is the little *Solenopsis fugax* that makes its nests in the walls of the galleries of its host. In this case, we may suppose, parasitism has arisen in a different way, *i.e.* not by disturbance of a symbiotic equilibrium but rather by failure to attain such an equilibrium (*vide* below). It is obvious that this explanation could not possibly apply to the relations of the slave-making ants discussed in the text, *vide* Donisthorpe, *op. cit.*, p. 80.

[3] *Vide* Lubbock, *op. cit.*, p. 89; Donisthorpe, *op. cit.*, p. 82.

[4] Geddes and Thomson, *Evolution of Sex*, p. 71.

carries the female about with him in a "gynæcophoric canal," formed from folds of skin; while in another worm, *Bonellia*,[1] the relations are reversed and, at the same time, carried farther, for the male, which is microscopic in size, lives parasitically on or within the female. Almost the same thing occurs in certain Rotifers—a group that shows a decided tendency towards degeneration of the males, which are always smaller in size—for in some species the males penetrate the body-wall of the female. Among the crustacea the female *Chondracanthus*, a parasitic form, carries the male attached to her reproductive organs.[2] Again, among the Zygomycetes, *Parasitella* and *Chætocladium* are parasitic upon other members of the Mucorineæ, but the interesting fact has been revealed that the positive strain of the parasite can only parasitise the negative strain of the host, strongly suggesting that, in this case, parasitism has arisen as a modified sexual impulse.[3] In other cases all trace of the original sexuality has been destroyed, as in *Piptocephalis*, which can parasitise positive or negative strains indiscriminately. A very decided step in the direction of sexual parasitism has been taken by the mantis, for the female of this species, as Fabre was able to show, devours the male immediately after he has performed the function of fertilisation.

It is hardly necessary to remark that this kind of parasitism is not confined to the sexual relationship: we have already mentioned the counterpart provided by the degeneration of the slave-making ants, and we should probably add those cases among the lichens where one of the symbionts noticeably dominates the other.[4] Quite comparable are those instances,

[1] *Op. cit.*, p. 20.

[2] *Vide* illustration, G. and T., *op. cit.*, p. 17.

[3] It is well known that many species of Zygomycetes are heterothallic, that is, they exist as two separate strains, both of which are necessary for the formation of zygospores. Even, however, if opposite strains of different species (say *Mucor hiemalis* and *Phycomycesnitens*) are grown together, we get an abortive attempt at the production of gametangia and zygospores—though actual fusion never occurs. By making use of the homothallic species *Zygorhynchus Mælleri*, in which there is a definite sexual differentiation, Blakeslee was able to identify the positive and negative strains of the heterothallic Mucorineæ as female and male respectively. Accordingly we are quite justified in regarding *Chætocladium* and *Paraellasit* as "sexual" parasites. The only respect in which they differ from the other cases mentioned is in the "hybrid" nature of their parasitism. Burgeff, "Über den Parasitismus von Chactocladium," *Zeitschr. f. Botanik*, **18**, 1–35.

[4] This, of course, depends on the view that one takes of the evolution of the group. It is usual to assume that the fungal constituent was primitively parasitic on the alga, from which point of view we should regard as derivative only those forms in which the relation is definitely reversed. This appears to be the case in several species, *e.g. Xanthoria parietina*, *Cladonia furcata*, and *Cladonia pyxidata*, in which the gonidial elements Cystococcus derive their organic food from the fungus. (Letellier, *Étude de quelque gonidies de Lichens*, *Genève*, 1917, p. 15; Moreau, "Les Lichens," *Le Chevalier*, Paris, 1928, p. 93.)

that of the midge *Miastor*, for instance, wherein the young are parasitic on their parent. "In 1865 Prof. N. Wagner observed . . . that in the larvæ of some two-winged or dipterous midges, the cells of the reproductive rudiment develop into larvæ within the mother larva's body. The mother falls victim to her precocity, for the brood of seven to ten larvæ literally feed upon her to the death. They finally leave the corpse and begin life for themselves, only, however, to fall themselves victims to a similar fate. The process may thus go on for several generations, during which the ova, or pseud-ovas some would insist on calling them, become smaller and smaller. Eventually the larvæ become too constitutionally poor to be precociously parthenogenetic, and develop into adult midges—male and female, the latter producing, however, only a few eggs."[1]

It is not less evident, however, that parasitism may arise in a different manner—that the parasite may represent the failure on the part of an organism to achieve any more refined relationship with its "host."

Such a condition is at least suggested in the Peronosporineæ, where, from those genera which we are accustomed to regard as primitive,[2] to the more highly developed forms, there can be traced a progressive refinement in the relation between parasite and host. So that whereas *Pythium* and *Phytophthora* kill the infected tissue, thereby usually killing the plant, *Albugo* and *Peronospora* only stimulate it to hypertrophy, or to the storage of food,[3] and we are in a position forcibly to contrast the wholesale devastation that attends an inroad of the Phytophthora, responsible for the Irish potato famine of '45, with the comparatively mild and harmless attacks of Peronospora among those plants (mustard, spinach, onions, etc.) that it ordinarily infects.

Of some interest in this connection is the work of Marryat (1907) on the susceptibility of wheat to rusts. He found that so-called "immune strains," such as "Einkorn," are in reality so susceptible that the tissues in the immediate neighbourhood of the infection are killed, and with them perishes the mycelium of the fungus. Evidently this is not only disadvantageous but actually fatal to the parasite, which depends for its propagation on maintaining a nice balance with its host, though ultimately to the detriment of the latter.

[1] Geddes and Thomson, *op. cit.*, p. 173.

[2] Most species of *Pythium* and *Phytophthora* are pleophagous and can furthermore be cultivated on artificial media; *Albugo* and *Peronospora* are much more specialised and will not grow saprophytically.

[3] *Vide* Gaumann and Dodge, *Comparative Morphology of the Fungi*, McGraw Hill Book Company, pp. 73, 74.

It should by this time have become clear that the inter-relationships which we are accustomed to set apart as sexual, social, parasitic, or symbiotic are actually particular or limiting cases of the same impulse—an impulse that is revealed in nature as a tendency towards the establishment of new relations, and at the same time towards the foundation of new "groupings" upon individuals of a lesser order.

The position of the multicellular organism is thus indicated at once as a particular case of a symbiotic community. Nor must too much emphasis be accorded to the reflection that, whereas the metazoan (or metaphyte) commonly perpetuates itself through the medium of a single cell (whether zygote or spore), associations of unlike members require for their reproduction—if they are capable of it at all—a group of units. This is a limitation engendered by the genetic constitution of the community.[1] Genetically heterogeneous associations are, for the most part, unable to propagate autonomously: a single generation of ants can reproduce entirely that part of the community which is genetically homogeneous—queens, workers, and males—but it cannot of itself give birth to a new generation of aphids, beetles, and crustaceans. The association must be established afresh by each generation in turn. Wherever vegetative reproduction is possible the difficulty is overcome by making use of "complex" propagative units: the soredium, for instance, of a lichen, which involves both fungal hyphæ and gonidia, or "cuttings" taken from a chimæra so as to incorporate the tissues of both potato and tomato. Where this is not possible special measures are often made to ensure that the association is renewed by the next generation, as in the case of the ants that carefully tend and protect the eggs of their aphids whose larvæ they raise within the nest. It is an incontestable fact that all those theories which have made use of cytoplasmic or nuclear amalgamation for the reproduction of a heterogeneous system, have so far proved untenable—witness, for instance, the mycoplasm theory of Erikkson,"[2] and the analogous sup-

[1] . . . Whereas many genetically homogeneous systems propagate exclusively in this manner. Among the Cyanophyceæ, for instance, several forms (*e.g. Cœlosphærium*) reproduce by budding off a portion of the colony, which then proceeds to grow by cell-division. It is needless to remark that as an *alternative* method of reproduction this is exceedingly common (cuttings, bulbils, etc.).

[2] Erikkson held that among the rust fungi, "besides the well-known vegetative stage," there exists "another vegetative stage, when the fungus exists in the cells of the host plant as a formless plasma body, a sort of plasmodium, symbiotically fused with the protoplasm of the cells, and forming together with these a 'mycoplasm.' At one stage these fungal elements separate out as 'corpuscles,' which unite to form fungal hyphæ.'"

According to this theory epidemics of rust found their origin in the presence of mycoplasm within the seed, so that the next year's crop was

positions of Elenkin.[1] Nevertheless, it must be borne in mind that, according to the most modern cytological observations, sexual polarity is established on the basis of a certain genetic dissimilarity, the nature of which varies in different organisms but depends, in general, on the presence, absence, or different forms of a particular chromosome known as the " allosome."[2] Hence we should say, strictly speaking, that the sexual relation itself demands a certain measure of genetic heterogeneity, though it is active only throughout a very narrow range of this disparity.

The predominance of the genetically homogeneous association as an evolutionary medium is directly associated with its unique reproductive privileges—*i.e.* the power of reproducing the whole community from a single constituent unit—and the capacity for perpetuating variations that this facility engenders.

There is hardly room for doubt, moreover, that the same impulse that has given us the transition from monad to cœnobium, and cœnobium to cell-dominion, has given us the parallel transition from the solitary habit to the gregarious and from the gregarious to the social. Each step the evolutionary impetus is transposed to a higher order of units, which themselves undergo simultaneous integration and differentiation—increasing individualisation and increasing division of labour. Just as the simpler animals and plants, cœnobia and cell-dominions, are to be regarded as symbiotic associations of cells, so must the higher forms take their place as associations of organs, likewise symbiotic, each capable of independent evolution within the limits imposed by its outward relations.

It is indeed this point of view that underlies the idea of " homology " in organs of widely separated species—an idea that marks the transition from a mode of thinking proper to physiology, physics, and phenomena in their mechanical aspects ; to that proper to morphology, to natural history, and to phenomena in their organic aspects. These two aspects, it is true, enter everywhere into the most intimate possible relations ; each is necessary to the appreciation of the other, as light and shade to the demarcation of a surface. Yet, that

already infected. It was discredited by Marshall Ward, who showed that Erikkson's " corpuscles " were, as a matter of fact, the intra-cellular haustoria of the fungal hyphæ. *Vide* Erikkson, J., *Fungous Diseases of Plants*, transl. Goodwin (Baillière, Tindall & Cox), 1930, p. 147.

[1] Elenkin, believing that the fungal constituents of lichens were parasitic on the algæ, thought that part of the fungal protoplasm was left within the gonidia, so that the latter were capable of reproducing the whole thallus independently. *Vide* Elenkin, " Zur Frage der Theorie des Endosaprophytismus bei Flechten," *Bull. Jard. Imp. Bot. St. Petersbourg*, t. 2, pp. 65–84, 1902.

[2] *Vide* Babcock and Clausen, *Genetics in Relation to Agriculture* (McGraw Hill Book Company), 1927, 9, 71.

which is essential to the former, that is to say, to the notion of "Law," is foreign to the latter, to the notion of "Type," and *vice versa*: a fact established long ago by Whewell in his *History of the Inductive Sciences.* If we assert, with one school of entomologists, that the wings of an insect are "homologous" with the gills of its larva, or of its phylogenetic prototype, we thereby impart to these organs an individuality of their own, that is maintained throughout any subsequent modifications that they may undergo. In particular, it was this point of view that gave rise to Van Tieghem's conception of the "stele" as a discrete vascular organism, so to speak, that could be regarded as evolving at once individually and yet conjointly with the rest of the plant—evolving, in fact, as a symbiont. The "Stelar Theory" signified the introduction of morphology, as distinct from anatomy, into the inward aspect of the plant.

An illustration will doubtless make this point clearer. The process known in botany as "cephalisation" represents the tendency towards the aggregation of reproductive structures that gives us, among the fungi, the various types of composite fructification known as coremia, acervuli and pycnidia and which, in the higher plants, leads to the appearance, first of the strobilus, and then of the flower. In this transition we see the symbiotic impulse evoking in conformity with plan: the sporangium that was at first the fruiting element itself becomes only a unit in a new organ—the "fruit body." In its early stages undifferentiated, this new organ occupies the morphological horizon on the one hand of the cœnobium, on the other hand of the associations of a gregarious organism (such as the locust); it is represented by the "cones" of *Lycopodium clavatum* (as contrasted with the diffuse strobili of *L. Selago*). From this condition we pass to one in which division of labour plays a part—certain sporophylls become sterile and fulfil a protective or attractive function (petals); others are modified for purposes of reception (stigmas)—and the infructescence, now called a flower, assumes a position corresponding to that of the "cell-dominion," and of the communities of a social organism (*e.g.* ants or bees). The Phanerogams are particularly interesting in so far as they have superimposed on their reproductive organs an even higher order of individuality—the "inflorescence"—which culminates in the highly specialised Compositæ that have reproduced almost exactly, in their "capitula," the original complexion of the flower, as it is represented in lower groups. An intermediate stage is represented in inflorescences of the type of *Viburnum opulus*, where the outermost flowers are sterile and serve purely for purposes of advertisement.

Accordingly, from the point of view of the theory of symbiosis, we may represent the association of the reproductive organs into communities, characterised by a greater or lesser degree of division of labour, by the following scheme :

Morphological Unit.	*Degree of Integration of Community.*
1. Sporangium →	Diffuse Strobilus → Cone → Flower.
2. Flower →	Loose Inflorescence → Umbel → Capitulum.
3. Monad →	Loose Colony → Cœnobium Cell-dominion.
4. Solitary Organism →	Gregarious Organism → Social Organism.

If there should be any doubt that organs can, and actually do, assume the status of individuals, it is fortunately removed by incontestable evidence : in certain of the higher Cephalopods, for instance, the male arm, or "hectocotyl," may become detached and swim about as an autonomous organism until it encounters the female, when it becomes attached and performs the act of fertilisation. In this state it was, indeed, originally regarded as an independent animal parasite.

The relation of the organ to the organism is to be regarded in the same perspective as that of the individual to the society, and that of the unicellular organism to the multicellular. In each case it is the relation of the unit to the community of which it is a member.

Before finally taking leave of the genetically homogeneous system we may for a moment direct our attention to an aspect of the discussion that is sometimes overlooked. Prof. Bower[1] is of the opinion that the prominence of the sporophyte among the higher plants can be attributed to the assumption of the terrestrial habit, having appeared as an intercalated post-sexual generation, representing the first step in the elimination of water as a necessary stage in the life-cycle of the organism. At that evolutionary horizon represented by the Bryophytes and Pteridophytes the only aquatic phase is that of fertilisation itself, which still remains dependent on the presence of water. In this respect the two groups in question are amphibious, and only the introduction of simphonogamy among the Phanerogams has rendered the plant genuinely terrestrial. The steadily increasing prominence of the sporophyte has been accompanied by certain *well-marked changes involving the economy of the spore-output.* Among primitive forms the spore-output is very great—a feature that tends to compensate for the difficulty of effecting fertilisation under aerial conditions—but, with the introduction, first of heterospory, and then of the seed-habit, a considerable economy has been effected in this direction. In the Lycopodiales, for in-

[1] *Vide* Bower, *Origin of a Land Flora.*

stance, we can trace the progress of the decreasing output of megaspores: whereas in *Isoëtes* these are produced in considerable numbers, in *Selaginella apus* they are reduced to eight, in *S. spinosa* to four, in *S. rupestris* to two, while in the fossil genera, *Lepidocarpon* and *Miadesmia*, only one megaspore develops (through abortion of the other three). Moreover, if, as it is thought, fertilisation in *Lepidocarpon* took place *in situ*, this would represent the first traces of the seed-habit, which, even in the most primitive Phanerogams (Cycadofilicales), appears fully evolved. *The last stages of the economy in spore-output is provided by the appearance of insects as agents in fertilisation and, in particular, of plants which are adapted to one class, genus, or even species of insect.*

Now, as a study in evolution, and in particular of symbiotic evolution, this series is peculiarly instructive, for whereas, in all the early stages elaboration proceeds in the medium of a genetically homogeneous system (that of the cell community), as soon as the resources of this form of evolution have been entirely spent, the last refinement is imported by the introduction of a genetically heterogeneous system—that of the plant-insect. It is to the discussion of these systems that we shall now proceed.

ON INCUBATION-TIME AND GESTATION-TIME

By JOSEPH NEEDHAM, M.A., Ph.D.
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"The Magnitude of Animals is by Ariſtotle aſſigned as the chiefest cauſe of the variety of the times of bearing. For, saith he, the great Fabrick, either of Animals, or any thing elſe, cannot be eaſily abſolved in a ſhort ſpace: wherefore Mares, and thoſe Animals that are of kin to them, though they live but a ſhorter time than man, yet are they longer in bringing forth: and therefore the Elephant (as they ſay) is two years in her production, becauſe of its excessive Magnitude. But every Animal hath certain Bounds which it cannot exceed."

William Harvey, *De Gen. Anim.*, 1653, p. 474.

When we consider the nature of the laws which govern the length of embryonic life we meet with a remarkable degree of obscurity. Roughly speaking, the common-sense rule, known to Aristotle and to Harvey, that the larger an animal is, the longer its embryonic life must be, is borne out by the figures. Thus in the tables of Milne-Edwards (19), Schenk (31), Vignes (35), and Przibram (28), the weight of the adult mammal varies between 0·014 kilo and nearly 4,000 kilos, while the gestation-time varies between 21 and 600 days. And here the largest mammals have the longest gestation-times, although there are several cases where animals of the same weight have different gestation-times, *e.g.* the pig and the deer, and animals of the same gestation-time, which reach, when full-grown, very different weights, *e.g.* the antelope and the hippopotamus. However, if we plot the adult weights against the gestation-times on double-logarithmic paper (so as to get all the data on to the same graph) we obtain a straight-line relation, the individual points not lying very far away from the mean. Now this line does not meet the co-ordinates at a zero point, it cuts the time scale, no matter what units are taken. This means that no matter how small the mammal, an appreciable time has to be taken in development, and in the case of a mammal as small as a gnat, a surprisingly long gestation-time would be observed. Thus the mouse, which is 259,000 times as small as an elephant, does not have an incubation-period 259,000 times as short, but only 31·6 times as short. A mammal as small as a gnat,

therefore, would probably have a gestation-period of 8 or 9 days. The explanation of this must lie in the time-requirement of differentiation.

Another way of looking at the data appears in Fig. 1. In order to eliminate the factor of weight, the gestation-time in days per kilo of adult animal was calculated, and this was then plotted on double-logarithmic paper against the weight of the

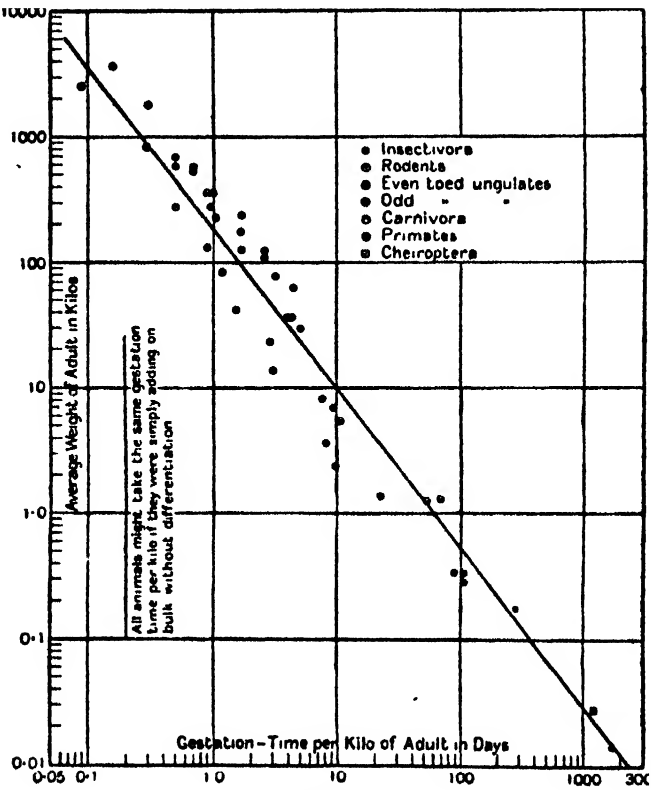

FIG. 1.

adult animal in kilos. Evidently the relation is linear here also, but the interesting thing is that the smallest animals take the longest time to construct unit weight. Thus the mouse performs the feat of producing a kilo of mice in 1,790 days, while the elephant produces a kilo of elephant in 0·16 day. This must be due to the fact that contained in 1 kilo of mouse there is a great deal more organisation and differentiation than in 1 kilo of elephant, in other words, that the degree of heterogeneity is greater. Whether all mammals can make unit amount of differentiation in equal time is a question one would

much like to see answered, but which appears to be at present unanswerable. Presumably the line in Fig. 1 would be straight and parallel to the vertical axis if embryos grew simply by adding on bulk without differentiation, for then a kilo of

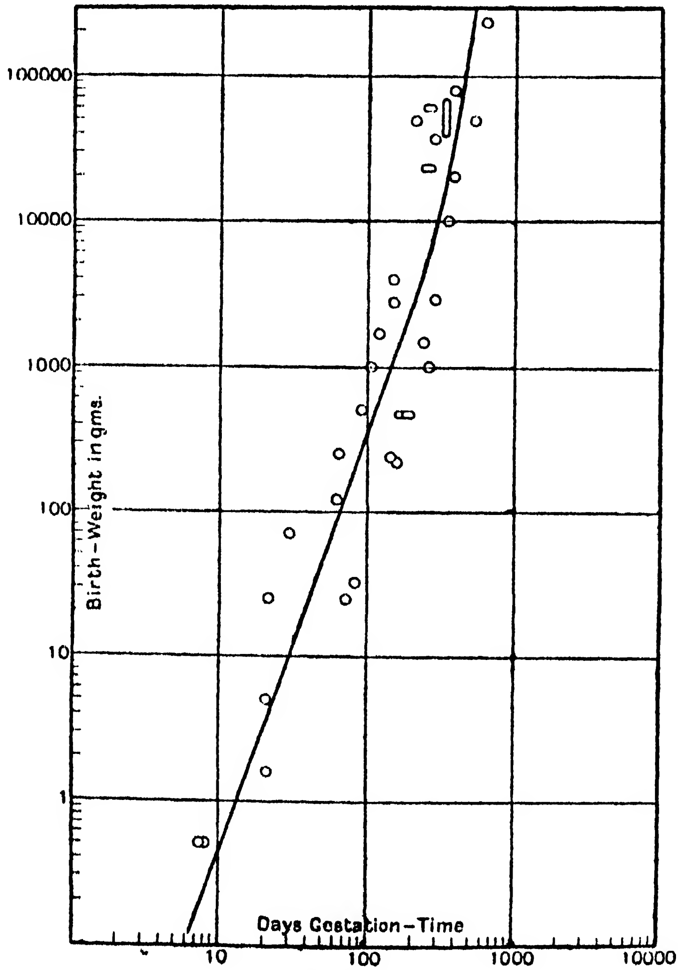

FIG. 2.

elephant protoplasm would be no easier to make than a kilo of mouse protoplasm.

So far we have only considered the relation between adult weight and gestation-time. It would obviously be more interesting to use birth-weights for this purpose, but unfortunately only a few are known. When those which have

been collected by Przibram (28) are plotted against the gestation-time on double-logarithmic paper, a straight-line relation is found, except for a slight deviation in the case of the heaviest animals ; this is shown in Fig. 2. The scattering of the points is evidently considerable, but we may say that there is some law which ensures that certain limits shall be held to. Thus if an animal proposes to weigh 100 gms. at birth, it must resign itself to an incubation-period of between 40 and 150 days, while if it is to weigh 1 gm. it may be between 10 and 30 days *in utero*. Within these wide limits, the individual species evidently have the power of making drastic shortenings and lengthenings.

The gestation-time alone may not be a very fundamental constant. In the first place, there are great differences in degree of development at birth between such animals as the pig on the one hand and the rat on the other, the former being born almost ready to assume complete motor control of its musculature, the latter by no means ready to do so, the former able to see, the latter blind, the former covered with hair, the latter hairless. Any relation between weight and gestation-time can therefore only be approximate, and the general law for animals as a whole must be modifiable, as it were, by local by-laws to a considerable extent. Again, the difference between polytocous and monotocous animals will make itself felt, and the large differences between the relative weights of new-born and mother. The following table, constructed from information given by Franck (9), shows how large these are :

	Total mass of fœtal tissue formed : Weight of mother 1 : x	Weight of one new-born : Weight of mother 1 : x
Man	19·1	19·1
Horse	14·6	14·6
Cow	15·5	15·5
Sheep	12·9	12·9
Dog	7·5	23·5
Cat	8·9	37·3
Rabbit	8·5	43·1
Pig	8·2	98·0

And there is also the consideration that gestation-time in some animals must be arranged to suit the grazing season. This factor probably accounts for many of the divergences of species from the line shown in Fig. 2. Again, within the individual species, birth can be shifted backwards and forwards to some extent, as has been familiar in the case of man from the time of Hippocrates onwards (see the treatises περὶ ἑπταμήνου and περὶ ὀκταμήνου in the Hippocratic Corpus). Bluhm's work (2) shows that the opening of the eye, the appearance of the

ears, and other marks of increasing differentiation in the mouse, occur at a fixed time after conception, so that the smaller the birth-weight, the longer the time elapsing between birth and the appearance of the mark in question. This relation is illustrated by Fig. 3, which is constructed from Bluhm's data.

What governs the incubation-times of birds? The problem has been much discussed, but by far the best treatment of it in the literature at present is the book of Bergtold (1). Of the 19,000 species of birds known, we have information concerning the incubation-periods of only 625. As might be expected, the length of the incubation-period varies more or less with the

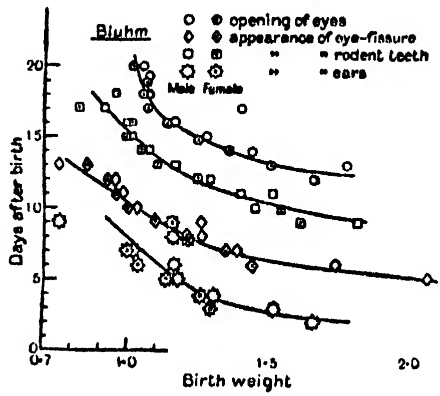

FIG. 3.

size of the bird. And as Féré (7) pointed out long ago, the smaller the egg the smaller the incubation-time; thus:

	Days' incubation.	Wt. of egg in gms.	Ratio.
Duck	25	73·9	1 : 0·84
Hen	21	60·2	1 : 0·81

However, this strict relation only holds for a few birds, and no close study of the tables of Bergtold is required to show that the relation is apparently even more elastic in birds than it is in mammals. Thus in spite of their different sizes the swift and the raven have the same incubation-period, while the kiwi and the hen are very similar in size but have quite different incubation-periods. The lapwing, again, though smaller than the woodcock, undoubtedly has a longer incubation-period. Nevertheless, when a broad view of the whole subject is taken, and the incubation-times are plotted against the adult weights on double-logarithmic paper, a definite trend does appear (see Fig. 4), and a similar picture is obtained when

the incubation-time is plotted against the egg-weight (see Fig. 5). The most interesting thing to notice is the slope of these two lines or zones, which is in both cases less steep than the mammalian line of Fig. 2. In other words, if the weight of any mammal is multiplied one thousand times, the gestation-period will be prolonged by about ten times, but if the weight

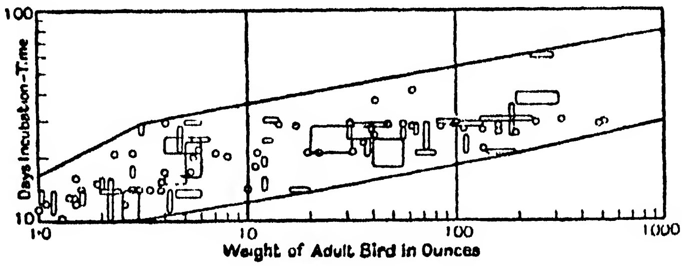

FIG. 4.

of any bird is multiplied one thousand times, the incubation-period will only be prolonged about four times.

Gurney's theory (14) was that incubation-time depended on longevity. The view of Gadow (12)—at first sight more acceptable—was that the developmental period as a whole was uniform and the longer the egg period the shorter the nest period. Yet this simply raises a further question, and while it

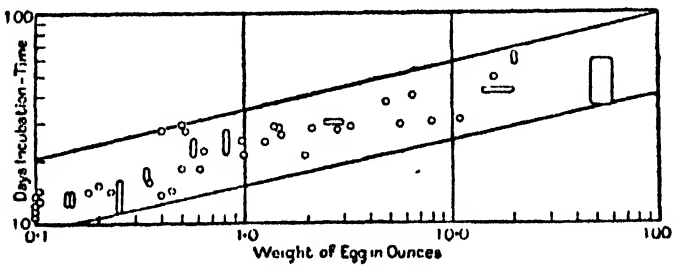

FIG. 5.

is more difficult to determine the total developmental period than the incubation-period alone, the difficulty of relating the preparatory period, whatever it is, to the causal factor, still remains. No doubt the differences between nidicolous and nidifugous birds are removed by this means. Claus's theory (5) was that incubation-period depended on egg-size, *i.e.* egg-weight, but this can only be true within wide limits, for the ostrich and the kiwi have equal incubation-times, yet the ostrich's egg weighs 3¾ lb. while that of the kiwi weighs less than 1 lb. It is natural that if incubation-time depends to some extent directly upon

egg-weight, it should also depend upon bird body-weight, for as Huxley (17) has shown, the egg-weight varies closely with the body-weight, though the eggs of large birds are not as large as they should be in proportion. Lastly, Pycraft (29) had a theory that incubation-time depended on yolk-weight, but as neither he nor anyone else accumulated any data with which to test the hypothesis, and as it is not in any case a very attractive one, it may be dismissed at once.

In Bergtold's view (1), the body temperature of the parent bird is an important factor. It is likely *a priori* that the larger the bird the lower its body-temperature, and a degree or two here might make a large difference. Bergtold gives in his book a list of avian temperatures, and it certainly seems that the smaller the bird the higher the reading, but the data are as yet too few for it to appear whether the exceptions noted above as destroying other theories are abolished on this one. Bergtold's theory is complicated by various taxonomic considerations, in which he supposes, following Sutherland (34), that the higher a bird is taxonomically, the higher its temperature. As the smaller birds (and mammals) are believed to be the most recent palæontologically, this may well be the case. In favour of Bergtold's view are the experiments of Heinroth (16), who reported that the eggs of the Egyptian goose hatch in 28 days under a common hen and in 30 days under a Muscovy duck. It is definitely known, since the work of Brody and Henderson (4), that within narrow limits the speed of embryonic development, as judged by wet body-weight, in ordinary hens' eggs, can be controlled by temperature-regulation. "The diminishing size of birds," says Bergtold, "accelerated their metabolic rate, elevated their body-temperature, and so shortened their incubation-period." According to Bergtold, the scanty data on reptilian incubation-times support his theory.

Let us now return to the comparison between mammals and birds which was raised by Figs. 2 and 5, in which the slope of the weight/incubation-time lines was seen to be different, and plot the two on the same graph (Fig. 6). It can be seen that not only are the slopes different, but the absolute values are also different, so that on the whole it takes less time to make an equivalent birth-weight of bird than of mammal. (The hatching-weights are here obtained by taking 75 per cent. of the egg-weight in grammes, the remaining 25 per cent. being divided between shell-weight, weight of membranes left behind, weight of water-vapour evaporated during incubation, and weight of material combusted in the same period.) It is also evident that the largest bird is, as regards birth-weight, 250 times as small as the largest mammal.

We are thus left with the following three reflections arising out of birth or hatching-weights :

(1) Although there are mammals as small as the smallest

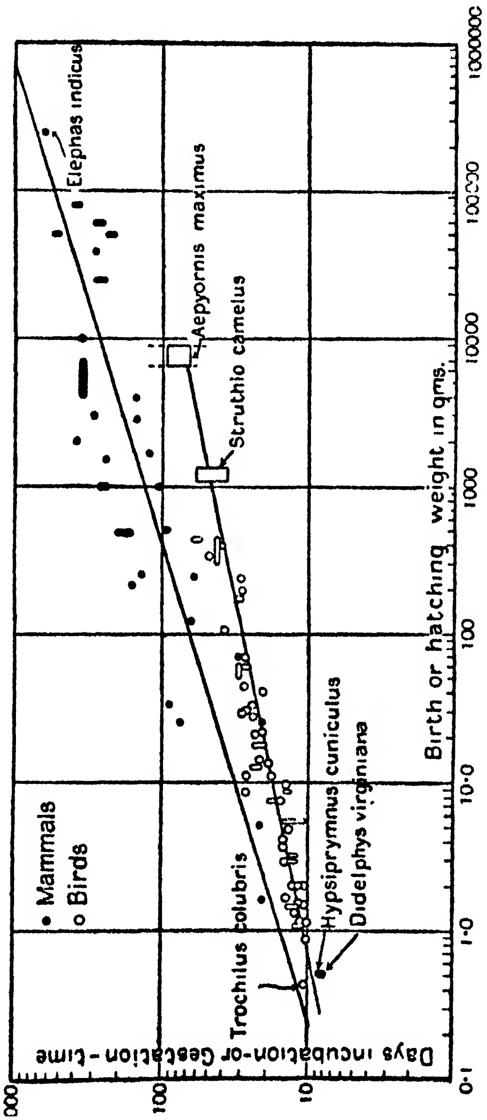

FIG. 6.

birds, there are no birds as large as the largest mammals. In fact, the largest bird is only a little larger than the half-way point on the mammalian line.

(2) The time required to make a given weight of bird is

always less than that required to make a given weight of mammal, as may be roughly expressed in the following table:

Birth- or hatching-weight in grms.	Bird incubation-time in days	Mammal gestation-time in days
100,000	—	600
10,000	—	260
1,000	45	150
100	30	55
10	17	32
1	11	14

(3) The prolongation of the incubation-time caused by raising the hatching-weight a given amount is not so considerable as the prolongation of the gestation-time caused by raising the birth-weight by the same amount.

The bird is thus clearly more rapid than the mammal in its development, and it may be suggested that this is an adaptation to life within the egg. It has been shown (Needham (22), (23), (25)) that eggs such as those of reptiles, birds, and insects, with their isolation from the terrestrial environment, quite unlike the close dependence of many aquatic eggs upon the sea, are closed systems, characterised, as it seems, by a definite type of metabolism in which the oxidation of fatty acids is accelerated, protein break-down suppressed, and uric acid made to take the place of urea and ammonia as nitrogenous waste-products. If, then, there are serious metabolic problems confronting animals which make their embryos develop in closed boxes, especially with regard to the disposal of incombustible waste, is it not possible that their incubation-time would naturally tend to be shorter than that of beings which excrete their embryonic waste-products through the placenta and the maternal kidneys? It is perhaps justifiable, therefore, to see in Fig. 6 the results of the closed-box or cleidoic system, development inside it being adaptively hastened. It would be very interesting to have parallel sets of data for insects and reptiles, and one might predict that they also would take relatively shorter times than the mammals, but so far I have not succeeded in finding any data from which graphs could be constructed.

As for the comparatively mean size of the largest bird at hatching compared with that of the largest mammal at birth, it has no doubt often been suggested that eggs above a certain size would begin to suffer from prohibitive mechanical difficulties. An egg large enough to produce a bird as big as an elephant at birth would require either internal struts, which would be impracticable, or else an extremely thick shell, which would raise great difficulties with respect to gaseous exchange. It is interesting that Friese (11) has shown that the larger the

egg the more shell it has to have; thus the canary's egg, weighing just under 2 grms., has 4 per cent., while the goose's egg, weighing 137 grms., has 14 per cent. It is likely, therefore, that 100 days is the extreme limit to which oviparous animals can prolong their incubation-time (unless they fall back on suspended animation and hibernation) as against the 600 or more which are possible to mammals. Is this connected with the extinction of the *Aepyornis*?

It might well be asked at this point how the extinct reptiles attained their prodigious size if they were oviparous, and the answer seems to be that for the most part they were not. Judging from the numerous finds of small skeletons within the abdominal areas of larger ones, some form of ovoviviparity was common. Whether these remnants were really embryos or perhaps rather traces of undigested food is not yet, and probably never will be, certain, but the question has been discussed by Fraas (8), Liepmann (18), van Straalen (33), and others, and the general opinion is that they should be regarded as embryos. We may conclude that the relatively rapid development of birds is an adaptation to cleidoic life.

It is interesting that the hibernation of embryos, far from being unknown, is quite common. The best known case is probably the silkworm, the embryo of which spends about 8½ months in a more or less quiescent state not advancing to any extent with its development. Dendy (6) reported in 1898 that the embryo of *Sphenodon*, the tuatara lizard, had an incubation-period of thirteen months, of which something like nine were spent in a hibernatory state. Boulenger (3) observed much the same thing in the case of the European pond-tortoise, *Emys orbicularis*, which has an apparent incubation-period of no less than 23 months. Still more extraordinary is the case of some mammals which possess the power, according to Prell (26, 27), of hibernating in the embryonic, partly completed state (mole, roedeer, bear, badger, pinemarten and stone-marten). And as for the insects, Regen (30) has shown that the eggs of a locust, *Thamnotrizon apterus*, laid in September, hibernate *two* or *three* winters, and finally hatch out in March. The Orthoptera in general commonly hibernate within their eggs, and other groups less often; thus the mosquito (*Aedes flavescens*) occupies 7 months in its egg (Hearle (15)).

The inner significance of the length of embryonic life relative to the life-span is most obscure. Some interesting observations have been made by Moulton (20), who has pointed out that over the whole life-span the chemical changes are much more intense in the earliest periods, pre-natal and to a slight extent post-natal. Thus by the 780th day from conception in man, the percentage of nitrogen in the body has reached 90 per

cent. of the value which it will have at the 7,800th day from conception, and at the same time the water-content has reached 80 per cent. of its final value. About the 500th day from birth there is a sharp inflection in these curves, and this appears in many other mammals as well as in man. Moulton named it the point of chemical maturity, since it marked the sudden cessation of intense chemical redistribution. And he found that it bore a fairly constant relation to the total life-span, (see Table I). This, however, is the only relation which does show constancy, and very little can be deduced about an animal if only its gestation-time or conversely only its average life-span is known. These facts illustrate that the act of birth or hatching may be a comparatively unimportant event in the life of the organism, the invisible processes of physico-chemical development unrolling inevitably beneath the surface not being affected thereby. Probably the time at which birth or hatching takes place has been much involved with the adaptations of the species to its environment.

TABLE I

Species.	Average length of gestation-period in days.	Average length of life in years.	Conception-age at chemical maturity in days.	Part of the life-span passed at chemical maturity (per cent. of the total life-span).	Constitution of the body at birth.		
					Water per cent.	Protein per cent.	Ash per cent.
Man	285	80	1,285	4·4	82	14	3
Cow	285	25	435	4·6	76	18	4
Pig	120	20	345	4·6	82	13	3
Guinea-pig	64	7	114	4·6	78	17	4
Dog	61	17	261	4·3	82	14	3
Cat	60	11	160	3·9	83	13	3
Rabbit	31	10	—	—	84	13	2
Rat	25	4	75	4·5	88	10	2
Mouse	20	4	—	—	86	11	3
				average 4·4			

This must clearly be the case as regards the distinction between tree-nesting and ground-nesting birds, for this difference is usually regarded by ornithologists as the cause of the division into nidifugous and nidicolous young. The hen, with its relatively prolonged incubation-period, which hatches out as a down-feathered and active chick, is more adapted to the greater danger of ground-nesting than the pigeon, which stays within the egg a much shorter time, and hatches out as a defenceless squab. These differences give one reason for the wideness of the zone in Figs. 4 and 5. It is interesting to reflect that we have here a difference involving the compression of

a larval stage within the egg, and thus an adaptation essentially similar to the suppression of larval stages observable as marine animals colonise the fresh water and as these in turn colonise the land (see Sollas (32), Giard (13), and Needham (24)).

The adaptive significance of incubation-time has been remarkably demonstrated in a paper by Friedmann (10), who found that the incubation-times of the cowbirds, such as *Agelaioides*, *Molothrus*, and *Tangavius*, varied according to their degree of parasitism. *M. ater*, which is very parasitic, has an incubation-time of 10 days (the shortest known), *M. bonariensis*, less parasitic, takes 11·5, and *M. rufo-axillaris*, still less so, takes 12·5 to 13 days.

With a view to ascertaining whether the rhythm of chemical differentiation went on similarly in the eggs of different birds, experiments on the non-protein nitrogen of various bird embryos have been done (Needham (21)). The standard curve for non-protein nitrogen content of chick embryos having been obtained, estimations were made on pigeon, guinea-fowl, turkey, duck, and partridge embryos. When the results were related to weights of embryo equal to those of the chick embryos on the different days, the values for the other embryos fell exactly on the chick curve. The length of incubation-time might thus appear to be a tune played, as it were, "largo" in the turkey and "allegro" in the guinea-fowl. For this it was necessary to assume that the pre-natal growth-curves for wet weight were alike, an assumption which has recently been experimentally verified by Kaufmann (36).

Summing up what has been found, then, the logarithm of the total time taken in embryonic development (*i.e.* the time which elapses between fertilisation and the exhaustion of the egg's reserves, roughly, up to hatching or birth) is directly proportional to the logarithm of the birth-weight or hatching-weight of the animal in question, but the logarithm of the time taken to make unit weight of the animal is inversely proportional to the logarithm of the birth-weight of the animal in question. The time required to make a given weight of bird is always less than the time required to make a given weight of mammal, and the prolongation of the incubation-time caused by raising the hatching-weight a definite amount is never so considerable as the prolongation of the gestation-time caused by raising the birth-weight by the same amount. This is explained by the peculiarities inherent in cleidoic life; the avian embryo having adaptively accelerated its differentiation-rate in order to escape the sooner from the closed box, permeable only to matter in the gaseous state, in which its species has seen fit, for other reasons, to confine it. It is much to be wished

that data for other groups, such as reptiles and arthropods, could be collected and subjected to the same type of examination as that attempted in this paper.

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THE ACTION OF LIGHT ON BACTERIA

By AUSTIN FURNISS, L.R.C.P., L.R.C.S., L.D.S., D.P.H.

While light in general is favourable to the growth of plant life, the opposite is true as regards bacteria. All known pathogenic bacteria are killed by light, and the same is true of pathogenic protozoa. In general, the vegetative forms are most sensitive to light and are most easily killed; but in some species, the spores are most susceptible to the destructive influence of light rays. In 1877 Downes and Blunt made their classical experiments and proved the bactericidal action of light. Two sets of culture tubes were subjected to irradiation. One set was surrounded by lead-foil, which prevented the action of light, but not that of heat. The other tubes were left uncovered, and it was found that it was only in the tubes of the first set that the organisms grew. The media in which the bacteria were killed was again inoculated with organisms, which grew normally, and thus proved that the light acted on the organisms themselves and not on the media, and that heat alone had no bactericidal effect. In 1890 Robert Koch showed that tubercle bacilli were killed by light. Sputum bacilli spread in a thin film on glass slides and exposed to direct sunlight are killed in ten minutes. The same observer found that ultra-violet rays destroyed not only bacteria but bacterial toxins, and the venoms of serpents. Browning and Russ in 1917 applied the radiation from a tungsten arc lamp, through a quartz prism, to a plate of gelatin inoculated with micro-organisms. Growth occurred where some parts of the spectrum fell and at others no growth took place. The most active bactericidal region was between 2,960 A.U. and 2,100 A.U. The ultra-violet region was divided by these experimenters into that between 3,800 A.U. and 2,960 A.U., which is bactericidal and has most penetrating powers, and that between 2,960 A.U. and 2,100 A.U., which is more bactericidal but does not penetrate a tenth of a millimetre of skin. It is generally agreed that the shorter wave-length region of the ultra-violet is the more powerfully bactericidal. Dealing with the transmission of the ultra-violet rays through skin, T. Takahashi has recently shown that living rabbit skin, which is thicker than 0·1 mm., can hardly transmit the rays

of wave-length shorter than 2,900 A.U. He has also shown that removed human skin transmits relatively more ultra-violet rays than that of rabbits. When, however, the tissue is thicker than 1 mm., it hardly transmits more than 1/1,000 to 1/20,000 part of incident rays of wave-lengths between 3,650 A.U. and 3,130 A.U. The shortest wave-length transmitted through thickness of 1 mm. was 3,130 A.U. Returning to the above experiment the sharply defined lines of demarcation which were formed suggested selective absorption, and this was proved by examining absorption spectra of suspensions of organisms used in the experiments. The absorption of ultra-violet rays by bacteria is principally due to the large amount of tyrosin and phenylalanin that they contain, and epithelial cells may often be stimulated by rays that are markedly bactericidal. The length of time required for the destruction of various organisms differs greatly. Most pathogenic organisms are killed in ten to twenty seconds, while yeasts and moulds and the tetanus bacillus require a full minute. The typhoid and colon bacilli and the organisms which give rise to dysentery and cholera are quickly destroyed by the ultra-violet light. Some bacteria which resist strong solutions of germicides, and even a prolonged boiling temperature, are rapidly killed by exposure to light. It is interesting to note that bacteria do not vary in their resistance to ultra-violet radiation as they do to chemical agents and to heat. For instance, spores, which are often twenty times more resistant to other agents than ordinary unprotected water bacteria, are only one and a half to five times as resistant to ultra-violet radiation. Experiments made with tetanus germs showed that they were more readily killed by direct sunlight than by a one to one-thousand solution of bichloride of mercury. The virulence of many pathogenic germs is attenuated if they are kept in the light. Diffused light acting during a long period is destructive to germs, but nearly all bacteria perish quickly when exposed to the direct influence of the sun's rays. Finsen found that bright sunlight killed plate cultures of bacillus prodigiosus after an hour and a half exposure. It required eight to nine hours' exposure with an electric arc lamp of 25 ampères to kill a plate culture of this same bacillus at a distance of 75 cm. from the arc. Finsen then tried the effect of concentrated light upon the same bacillus and also on the typhoid bacillus. He found that concentrated sunlight killed the germs fifteen times more quickly than ordinary sunlight, and that the influence of concentrated arc light was still more powerful.

Dieudonné's observations showed somewhat similar results. He found that bacteria were killed in half an hour by direct

sunlight, in six hours by diffused daylight, and in eight hours by an arc light (900 candle-power). Bang showed that the unconcentrated light from a 30 ampère arc lamp will kill a surface layer of tubercle bacilli at a distance of 30 cm. (12 inches) in six minutes. Von Jansen inserted between the light rays and the bacteria a piece of skin 1·2 mm. thick, and found that destruction of bacteria occurred in one and a quarter hours. With skin 1·5 mm. thick the bacteria were no longer destroyed. To show the effect of sunlight on sewer water, Procaccini exposed to sunlight sewer water containing 300,000 to 420,000 bacteria per cubic centimetre. After a day's exposure the water was sterile.

Bacteria are readily killed by light at the surface of the soil, although 20 inches below the surface they may resist destruction for four or five months. Ultra-violet rays are absorbed by the protoplasm of the organism, and in a culture preparation, or in the case of germs on the surface of a wound, one bacterium will protect a second lying under it ; so in a condition such as lupus, little beneficial effect (therapeutic) can be considered to be due to the direct bactericidal effect of the rays. In this condition the excellent therapeutic effects obtained by the use of ultra-violet radiation, local and general, are due to an increased bactericidal property of the blood itself, increased lymphocytosis in the part, and stimulation of cicatrisation. It is impossible for bactericidal radiations to penetrate in sufficient intensity to destroy bacteria in the epidermis or dermis. The fact that general irradiation will cure skin lesions shows that the destruction of bacteria is due to a secondary reaction which follows the absorption of radiation by the living cells. Irradiation of the skin by rays shorter than 3,100 A.U. increases the bactericidal power of the blood and serum, the phenomenon originating at the site of radiation. Dealing with the experiments above mentioned it may be stated that many similar ones have been made by research workers, usually with the same result. Many years ago Marshall Ward threw the spectrum on an infected agar plate, and showed that no growth occurred in the parts of the plate irradiated by the ultra-violet end of the spectrum.

Drs. H. and W. K. Russell exposed Petri dishes containing agar sown with fresh cultures of staphylococcus aureus and bacillus coli to the radiations from a mercury vapour lamp at a distance of 10 inches. They found that all the organisms were killed after five seconds' exposure. They repeated the experiment, placing screens of various materials between the lamp and the organisms, and found that no inhibition of growth took place when the rays passed through glass, only very slight inhibition when they passed through celluloid, and marked

inhibition with gelatin. These experiments showed that glass cuts off rays having wave-lengths shorter than 2,960 A.U., whereas celluloid allows some bactericidal rays to pass through, and gelatin is even more permeable to these rays. To show that the rays act on the bacteria themselves, and not on the medium on which they are sown, the agar plates on which the growth of bacteria had been killed were again inoculated for twenty-four hours at 37° C., and normal growth was found. Dr. H. Goodman mentions his results in somewhat similar experiments. "With a mercury vapour lamp set inside a case which had two apertures, I exposed the unsunburned back of a patient to the radiation through the front window. A Petri dish of freshly streaked viable staphylococcus aureus was exposed to exactly the same radiation at the same period of experimentation for the same duration of time through the second aperture at the bottom of the case. The regular Petri dish cover was replaced by a piece of fused quartz. I found after incubation that no culture plate was rendered sterile despite numerous variations of time and distance up to the time and distance factors to which the skin of a white person could be exposed with safety." Dr. Goodman's contention is that radiation did not prove as efficient as reports in the literature would indicate. In the experiment, the staphylococci were streaked on the surface so that the lack of permeability of the media should not have been a very great factor in the lack of results. In the case of a culture, or the surface of the body, one bacterium will protect a second lying under it, as has been mentioned before. The result of Dr. Goodman's experiment will certainly cause this type of experiment to be repeated.

R. G. Perkins and H. Welch studied the bactericidal effect of ultra-violet light from a series of carbon arcs. Their findings were in agreement with previous workers in that the light with the highest germicidal power is that with the strongest ultra-violet emission, particularly of a wave-length of between 2,500 and 2,600 A.U.; the bactericidal action is also less with heavy bacterial suspensions than with a more dilute suspension. Observations previously published have dealt almost entirely with exposures of five seconds or longer and the authors found that an exposure of this length invariably caused a diminution in the viable bacteria present of between 80 and 90 per cent. A shutter capable of giving measured exposures of fractions of a second was then used and they found that an exposure of 0·2 second caused a reduction of 40 per cent. This rapid initial bactericidal action is followed by a slower action gradually tailing off to approximately thirty seconds, which may be considered the end point. When the organism under investi-

gation was a staphylococcus aureus which required two or three days for pigment formation, it was found that persistent forms (some 1 per cent. which seem to be specially resistant to the lethal effect of ultra-violet radiation) showed pigment consistently in twenty-four hours and retained the property for a number of generations. The authors are making the question of these resistant forms a subject of further investigation.

H. and W. K. Russell, quoting Angus, write as follows :

Using the following sources of radiation, and experimenting with hay infusoria, it has been found that the relative power required to produce equal lethal effects is as follows :

Source.	Relative Watts.
1. Mercury vapour lamp	1·0
2. Tungsten arc	1·1
3. Carbon with pulverised iron core	2·2
4. Carbon with cerium and iron core (1)	2·2
5. Hollow plain carbon	2·7
6. Carbon with cerium and iron core (2)	2·8
7. Carbon with cerium core	3·6

Opinions on the action of rays of longer wave-length than 3,800 A.U. are diverse. Some scientists think that these rays favour the growth of bacilli ; but all are in agreement in recognising the powerful germicidal action of the shorter ultra-violet rays. It has been suggested that this is caused by the production of ozone and peroxide of hydrogen in the medium which would kill the organisms ; but the short length of time necessary (a few to sixty seconds) for the lethal effect proves this theory to be incorrect, because it takes a much longer time for even the slightest trace of these substances to be formed ; and, furthermore, if the medium is inoculated after the irradiation there is no effect on the organisms.

Mode of Action of Ultra-Violet Radiation on Bacteria.—Numerous theories abound as to the exact method by which ultra-violet radiation acts on bacteria. It is generally conceded that the deadly action is due to some unknown quality inherent in the rays themselves, and not to the action of some poison formed by them. Burge showed that while liquefying bacteria are killed by ultra-violet radiation the enzyme which produces the liquefaction is almost unimpaired. He also tried to demonstrate that the destructive action is due to coagulation of the bacterial protein. This coagulation was not always demonstrable. The consensus of opinion seems to be that ultra-violet radiation kills living cells and tissues by changing in some way the protoplasm of the cells, so that certain salts can combine with the protein of the protoplasm to form an insoluble compound. Another theory is that the cause of

death of the bacteria is the heat coagulation that follows sensitisation by ultra-violet radiation. Ultra-violet radiation destroys the secretions and toxins of organisms, but only after a longer period than is necessary to kill the organisms themselves. A wide field for research remains in the action of ultra-violet radiation as a means of sterilising suspensions for use as antigens.

There is reason to believe that ultra-violet radiation of bacteria tends to favour a retention of antigenic properties while diminishing the toxicity of the organisms. It is well known that organisms, particularly those of the Gram-negative group, such as bacillus coli, etc., tend to produce rather disagreeable local and general reactions when killed by heat and used as a vaccine. The inhibitory power of ultra-violet radiation on a large mass of organisms in broth suggests the utility of this bactericide in preparing toxic bacteria, such as bacillus dysenteriæ, Shiga, for antigenic purposes. Shiga toxin loses its toxicity after irradiation. Diphtheria toxin is stated to behave in a similar fashion (Dr. McCaskey, New York). Diphtheria antitoxin, on the other hand, is most resistant to radiation. Agglutinins and antitoxins seem to be the most resistant anti-bodies to ultra-violet radiation.

The industrial application of the abiotic property of ultra-violet radiation was closely associated with the development of the means of production of the rays, and advanced rapidly with the evolution of the quartz lamp. To-day ultra-violet radiation is used extensively for sterilising water. Water, which is probably more transparent to ultra-violet rays than any other liquid we know, is sterilised by passing through an apparatus where it comes into contact with a quartz plate separating it from a mercury vapour lamp. Milk can also be sterilised (and incidentally rendered antirachitic) by ultra-violet radiation, there being now at least six recognised methods —known by the names of Scholl, Scheidt, Buhtz, Wamoscher, Hickman, and Vita ray lamps.

NEW IDEAS ABOUT COLOUR VISION

By R. A. HOUSTOUN, D.Sc.

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Last year I made a discovery in connection with the Weber-Fechner law which has had a number of interesting consequences. These are being described in papers in the *Philosophical Magazine*, but as the matter is of interest to physiologists, psychologists, and statisticians, as well as to physicists, I think it desirable to give a simplified account of it here.

E. H. Weber stated the law: The just appreciable increase of stimulus bears a constant ratio to the original stimulus. For example, if I denotes the brightness of a surface, and $I + \Delta I$ the brightness of an adjacent surface, and if the difference in brightness of the two surfaces is just perceptible, the ratio $\Delta I/I$ is constant, no matter what the magnitude of I is. The law has been referred to as the psychophysical law, and was supposed to apply to other sensations as well as to vision. It has a very extensive literature.

Fechner attempted to express sensation in terms of quantitative units. He assumed that all just noticeable differences of sensation contain an equal number of sensation units. Thus if ΔS is the increase in sensation, $\Delta S = k\Delta I/I$. This is Fechner's fundamental formula.

The most important investigation of Weber's law was made by König and Brodhun[1] in 1888 and 1889. They worked over the whole range of which the eye is capable, their highest value of I being 10^6 times their lowest, and they each measured $\Delta I/I$ for white light and for six monochromatic regions of the spectrum. They found that the ratio was constant through the middle of the range, but that it increased towards each end. This had been noted previously by Fechner; he considered that these deviations were due to disturbing factors, to dazzle at high intensities and to the intrinsic light of the retina at low intensities.

I noticed last year[2] that when $I/\Delta I$ was graphed against log I, the results formed a Gaussian curve of error. Fig. 1,

[1] *Sitzungsber*, Berlin Akad., p. 917, 1888; p. 641, 1889.
[2] *Phil. Mag.*, 8, p. 520, 1929.

for example, gives the mean of König and Brodhun's results for white light. The smooth curve is a probability curve $y = exp(-x^2/2)$ adjusted by trial and error to fit the data.

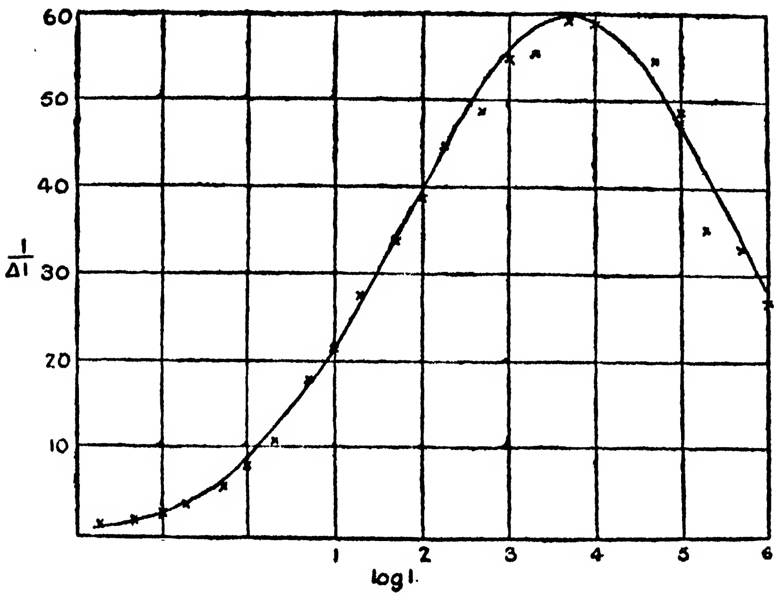


FIG. 1.

The fit is a good one ; it shows that there can be no question of the validity of Weber's law even for a limited interval. Together with Mr. Jas. F. Shearer I have made measurements of $I/\Delta I$ this past winter, and our results also agree with the Gaussian curve. But König and Brodhun's data are better evidence, for they had the other law in mind when they were making the measurements.

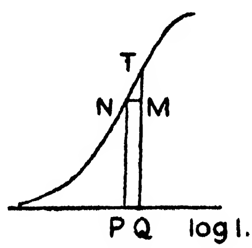


FIG. 2.

Let us suppose (Fig. 2) that the abscissa of P is log I, that the just perceptible increase in brightness is ΔI, and that the abscissa of Q corresponds to $I + \Delta I$. Thus

$$PQ = \Delta \log I = \Delta I/I.$$

Let the ordinate PN be $I/\Delta I$. Thus the area of the rectangle PQMN is unity. In practice PQ is small, and there is no appreciable error introduced in neglecting the area of the triangle NMT. So that whenever the brightness increases by a just perceptible step, the area of the curve goes up by unity. Thus the sensation at any point on the axis of abscissæ is

proportional to the area to the left of that point. So far we are on certain ground. We now proceed to make an assumption.

The Gaussian curve occurs frequently in statistics and usually specifies the distribution about a mean of some quality of a homogeneous population. It is natural, therefore, to interpret it here in the same way. We think, therefore, of the cones and possibly also the rods in the retina as each connected to a fibre of the optic nerve and constituting a population with different thresholds. As log I is raised, each passes in succession across its threshold. The area to the left of any point is proportional to the number above the threshold at the point. Thus the sensation of brightness, S, is strictly proportional to the number of percipient elements, *i.e.* to the number of fibres active.

This is an assumption. There is, however, a means of checking it. The resolving power of the eye, its ability to discriminate detail, increases with the brightness of the light. Now the resolving power must depend on the grain of the receiving surface. As the intensity increases, more elements come across the threshold, and the grain becomes finer. If the test object is a series of equal black lines and spaces, the resolving power or acuity is taken as the reciprocal of the angle subtended by the breadth of a line or a space, expressed in minutes, when they can just be resolved. It is natural to take it as proportional to the fineness of grain, to the number of elements per unit length or the root of the number of elements per unit area. Thus the resolving power should vary as $\sqrt{S}$.

Fig. 3 puts this conclusion to the test. The circles represent the acuity in white light determined by Mr. Jas. F. Shearer and myself during the past winter. The black discs show the acuity in white light as determined by König [1] thirty-three years ago. As his observations were very numerous, they have been averaged in groups of six. The agreement between his results and ours is not good, but his were obtained under totally different conditions with Snellen's test figure as object, whereas we used parallel lines and spaces. The smooth curve gives $\sqrt{S}$ as determined by our observations on Weber's law on an arbitrary scale of ordinates. The agreement of the curve with the observations is satisfactory enough, because we have not yet discovered how far the S curve varies from observer to observer, and acuity should depend on the degree of contrast in the object as well as on the grain of the retina, a factor we have left out of consideration. The resolving power of a photographic plate depends not only on its grain, but also on whether it is clean working and free from fog.

[1] *Sitzungsber*, Berlin Akad., p. 559, 1897.

When a rotating sector alternately stops and transmits the light, the angular speed at which flicker vanishes increases with the intensity of the light. The graph connecting angular velocity with intensity is regarded usually as consisting of two straight lines. The critical angular velocity as determined by T. C. Porter [1] for white light is represented in Fig. 3 by crosses on an arbitrary ordinate scale. It will be observed that it agrees with the acuity except at low intensities, and consequently also with $\sqrt{S}$. We have not yet gone into the theoretical significance of this agreement. In Fig. 3 log I is corrected for the variation of the pupil of the eye.

The assumption, then, that the intensity varies as the

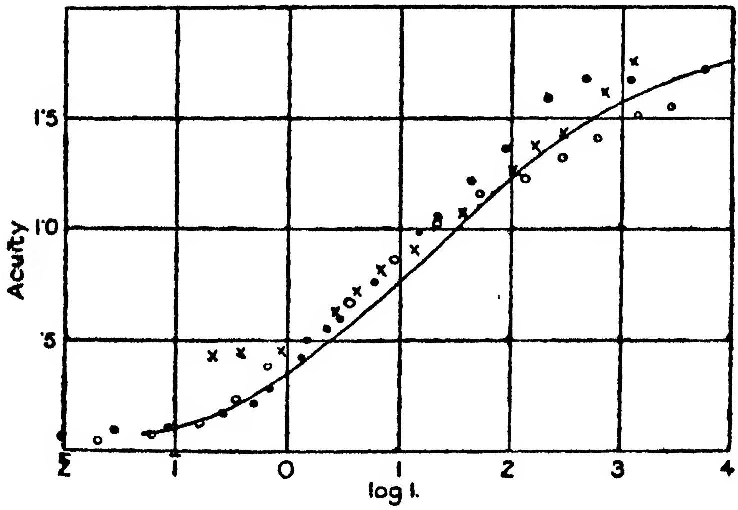


FIG. 3.

number of percipient elements, except in so far as it is modified by memory, habit, association of ideas, etc., comes out of the test satisfactorily. It also accounts for the intensity of point sources. The stars have been grouped since ancient times in six magnitudes, each magnitude being just appreciably brighter than the one below it. They appear as points, with the exception of the brightest, which are just on the verge of having size. We have experimented with artificial stars the brightness of which could be increased by jerking resistance out of a circuit, and found also that they went up six just appreciable steps before ceasing to be points. Owing to the aberrations of the eye and diffraction at the edge of the pupil the image on the retina is never a point, but always a disc. This disc covers at least six percipient elements, and as each

[1] *Proc. Roy. Soc.*, 70, p. 313, 1902.

additional element comes over the threshold, the star goes up one magnitude.

We come now to the bearing of the assumption on the classic problem of colour vision. It is well known that a colour possesses three qualities, hue, brightness, and saturation. Any light can be matched in colour and brightness by a superposition of three colours, the so-called primary colours. According to Helmholtz, there corresponded to the primary colours three primary sensations. Can the percipient elements be divided into three classes?

The work of König and Brodhun on Weber's law enables us to answer this question in a definite manner. S was the area of the curve $y = exp(-x^2/2)$ to the left of a particular ordinate. Let us graph S as ordinate against log I. We obtain a curve of the shape shown in Fig. 4. Let us suppose that Helmholtz's assumption is true, and that the eye is stimulated with red light up to the intensity represented by P. Then the sensation is PQ. Let us next suppose that the eye is stimulated with green light up to P; the sensation is again PQ. Suppose the intensity of the red light diminished to one-half; the sensation is TR where TP = log 2. Suppose the intensity of the green light diminished to one-half; the sensation is again TR. Let the red and green be applied simultaneously at half intensity. They produce a sensation of yellow of intensity 2TR. But according to the results of König and Brodhun $I/\Delta I$ is independent of colour. The sensation should in this case be PQ. Hence PQ = 2TR for all positions of P, which is absurd. The percipient element cannot be divided into three classes in this manner.

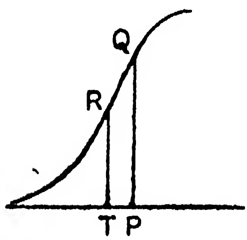


FIG. 4.

Let us state the conditions which a theory of colour vision must satisfy.

(1) The sensation of light is mathematically a function of three variables. All sensations can be produced by combining three stimuli.

(2) Although the sensation is a function of three variables, only one of these variables can represent brightness, as the preceding reasoning shows. The other two must represent colour, or colour and saturation.

(3) The colours red and green and yellow and blue are connected in pairs. This is required by the great body of tradition associated with Hering's theory.

(4) Yellow, according to the psychologists, is a fundamental sensation. We cannot see red and green in it. Here we are

on less certain ground, because for me the violet of the spectrum has always been a fundamental sensation, and the psychologists appear to regard it as composite. But the case of yellow is, I think, beyond doubt.

(5) Anatomically there is no evidence for three photochemical substances or three nerve systems. But there are two nuclear layers and two reticular layers in the retina.

It is in this last condition that I think the solution lies. According to the usual view the nervous impulse originates in the cones, passes through the outer nuclear layer, crosses a synapse in the outer reticular layer, passes through the inner nuclear layer, and then crosses a synapse in the inner reticular layer. There are thus two synapses in the visual path. But as far as I can make out from inspection of the drawings and conversation with experts, it is impossible to trace the single fibres with certainty. An alternative interpretation is equally probable, namely that there are two different kinds of visual path, those with a synapse in the outer reticular layer and those with a synapse in the inner reticular layer. No path has a synapse in both layers. The distance between the two layers is so small, that a separate neurone seems hardly necessary to carry the impulse from the one to the other. I would suggest, with the ignorance of a physicist, that the cells are arranged in two layers, so that the cones can pack closer.

Let us assume that there are two different kinds of path. To satisfy the requirements of Hering's theory we have to make a further assumption, namely that after the synapse each path transmits only two kinds of impulse. Or in other words, there are red-green fibres which transmit either red or green, and blue-yellow fibres which transmit either blue or yellow. If R and P denote respectively the numbers of red-green fibres in the red and green states and B and Y the numbers of blue-yellow fibres in the blue and yellow states, since the number of fibres in each class is the same—

$$R + P = B + Y.$$

Since the four variables are connected by this relation, only three of them are independent. Thus the laws of colour mixing are satisfied.

It will conduce to clear ideas if we trace the process from the start. Light of a certain frequency falls upon a photochemical substance and electrons are ejected according to the equation of the quantum theory—

$$h\nu = \tfrac{1}{2}mv^2.$$

The velocity of the electron depends on the frequency of the light, and the number ejected on the intensity of the light.

These electrons set up pulses in the cones. R. S. Lillie has described a nerve model which imitates very closely the propagation of the pulses. If an iron wire is dipped into a strong solution of nitric acid, its surface becomes "passive," *i.e.* becomes covered by a sheath of oxide which protects it from the further action of the acid. If the wire is then immersed in a weaker solution of the acid and the surface touched with a zinc rod, the sheath is destroyed at the point of contact, and a pulse characterised by effervescence travels rapidly along the wire. The sheath re-forms behind the pulse, and after a short interval of time the wire is in a condition to transmit another pulse. Now the time of recovery will depend to some extent on the velocity of the electrons, *i.e.* on the discharging potential. We think, therefore, of a train of pulses of variable frequency approaching the synapse from the cone, the frequency varying according to the colour of the incident light.

Beyond the synapse there are only two possible frequencies, *e.g.* in the case of the red-green fibre the fibre must be in either the red or the green state. Perhaps the analogy of an organ pipe may be helpful. If we blow it gently, it sounds the fundamental. But if we blow harder, it gives the first harmonic.

Thus the light process and the colour processes are quite separate. One takes place in the layer of the rods and cones and the other in the reticular layers. If the light has a certain intensity, the same fibres are stimulated, no matter what its colour. But the proportions existing in the different states depend on the colour. This picture may be speculative and fanciful. But it is the only one I am aware of which is in harmony with Konig and Brodhun's work on the Weber-Fechner law; it is impossible to bring Helmholtz's theory into accordance with that work. I use the symbol P to denote the number of fibres in the green state, because I regard the sensation caused by this state to be peacock blue, not green.

It may easily be shown that in colour-mixing problems my scheme leads to the same result as the Young-Helmholtz scheme. For example, suppose that light specified by R_1P_1, B_1Y_1 is superposed on light specified by R_2P_2, B_2Y_2. The mixture is specified by R_1+R_2, P_1+P_2, B_1+B_2, Y_1+Y_2. Let the intensities of the components be m_1, m_2. Then—

$$m_1 = R_1 + P_1 + B_1 + Y_1$$

and

$$m_2 = R_2 + P_2 + B_2 + Y_2.$$

Let

$$x_1 = \frac{R_1}{R_1 + P_1} \qquad y_1 = \frac{B_1}{B_1 + Y_1}$$

$$x_2 = \frac{R_2}{R_2 + P_2} \qquad y_2 = \frac{B_2}{B_2 + Y_2}.$$

Thus when the intensity m and the colour valencies x and y are known, the light is fully specified, and the colour can be specified by a point with the co-ordinates x, y. Let the co-ordinates specifying the resultant colour be $\bar{x}$, $\bar{y}$. Then by definition—

$$\bar{x} = \frac{R_1 + R_2}{R_1 + R_2 + P_1 + P_2} = \frac{2(R_1 + R_2)}{m_1 + m_2}.$$

But $$x_1 = \frac{2R_1}{m_1} \qquad x_2 = \frac{2R_2}{m_2}.$$

Hence $$\bar{x} = \frac{m_1x_1 + m_2x_2}{m_1 + m_2},$$

and there is a similar equation for y. The centre of gravity construction holds for the mixing of colours. It is an immense simplification to be able to use Cartesian co-ordinates instead of the trilinears of the Young-Helmholtz theory.

When Newton first investigated the spectrum, he divided it into five colours, red, yellow, green, blue, and violet. These colours were for him presumably fundamental sensations. Orange and indigo were added to the list afterwards on account of a fancied analogy with the notes of the musical scale. We know now that there is nothing in this analogy. It was founded on inaccurate observations, but all the same it forms an interesting link with the speculations of the Middle Ages, and connects the spectrum with the seven planets of ancient astronomy.

There appears, however, to be sound reason for stating that there are seven colours of the spectrum. The rate of variation of hue in the spectrum with wave-length has been investigated very carefully by Steindler and L. A. Jones. Their results are in agreement. Fig. 5 gives the final results of L. A. Jones,[1] slightly smoothed. The abscissæ give wave-lengths and the ordinates the reciprocal of the smallest change of wave-length that could be detected at the wave-length in question. The curve has maxima of sensibility at yellow and peacock blue and two smaller maxima in the red and violet. There are thus seven turning values in all. These coincide with the principal colour names in use. If a student is asked to point out yellow or peacock blue in the spectrum, he points exactly to the two principal maxima. If he is asked to point out green, blue, or orange, he points to the three minima. If he is asked to point out red or violet, he points a short distance to the outside of the smaller maxima. Psychologically a fundamental colour is one that cannot be resolved by intro-

[1] *Jl. Opt. Soc. of America*, p. 63, 1917.

spective examination into other colours. This is a method of analysis for use only by psychologists ; I commend the coincidence of the colour names with the turning values to their consideration. I do not know whether I myself really see red and yellow in orange or whether it is an association of ideas due to mixing water-colour paints in childhood.

To return to the physical aspect of the curve : no physicist can fail to be impressed by its symmetry, especially when he knows that colour apart from intensity is a function of two

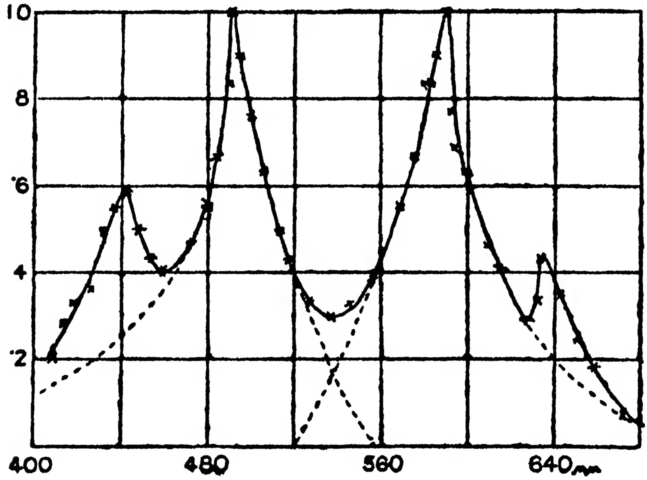


FIG. 5.

variables. The temptation to analyse the curve into the sum of four curves is a great one. I have done so by means of the dotted lines, drawing them so as to make the component curves as smooth as possible. It is natural to regard these component curves as the rate of change of the colour valencies and to associate each big maximum with a little one. In this way I have determined the colour valency curves as shown in Fig. 6. The curves of Fig. 5 are obtained by differentiating the curves of Fig. 6.

In drawing the valency curves the ordinates are indeterminate to the extent of an additive constant. I have chosen the latter, so as to make the valency curves cross the axis at the principal maxima of the hue discrimination curve. Then distance above the axis is proportional to R or B and distance below the axis is proportional to P or Y. It is more convenient for many purposes to take the line AB as axis, and denote the ordinates of the red-green and blue-yellow curves respectively by x and y. Then to every wave-length of the spectrum

there corresponds an x and y. If these are graphed on a co-ordinate diagram we obtain Fig. 7. This figure is a colour mixture curve similar to those obtained by Maxwell, König and Dieterici, and Abney. Only theirs were obtained directly by matching the colour and brightness of two adjacent fields, whereas Fig. 7 is obtained from hue discrimination data alone. Fig. 7 does not differ more from the results of Maxwell, König and Dieterici, and Abney than these results differ among themselves.

I have stated that the variation of $I/\Delta I$ with log I is independent of colour. This is only the case for the light-adapted

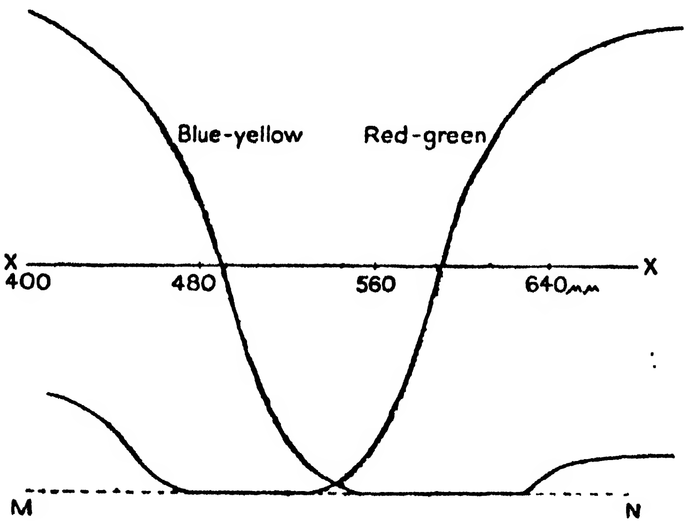


FIG. 6.

eye. When the eye is dark adapted, its power for discriminating difference in intensity at low intensities is increased at the blue end of the spectrum. This change is connected with the Purkinje effect. The brightest part of a bright spectrum is in the yellow near the D lines. If, however, as we look at a spectrum, the width of the slit is gradually decreased, the maximum of brightness shifts from the yellow, and when the spectrum has become very faint, it has reached the green. The phenomenon is known as the Purkinje effect, and it is to it that the greenish-blue appearance of objects in moonlight is due. The eye is like a measure of varying length. Objects appear blue in the moonlight, not because the light is faint, but because the eye is adapted to faint light, and consequently assesses the relative brightness of colours differently.

According to the duplicity theory of Von Kries the Purkinje

effect is explained by assuming that there are two different systems of receptors in the retina, the rods which are responsible for vision at low intensities and the cones which are responsible for vision at high intensities; the rods are not sensitive to colour, but have a maximum of sensitiveness at a wave-length in the green. When the eye becomes dark adapted, vision shifts from the cones to the rods.

A serious objection to this theory is that dark adaptation takes time to set in. We have therefore to postulate that the

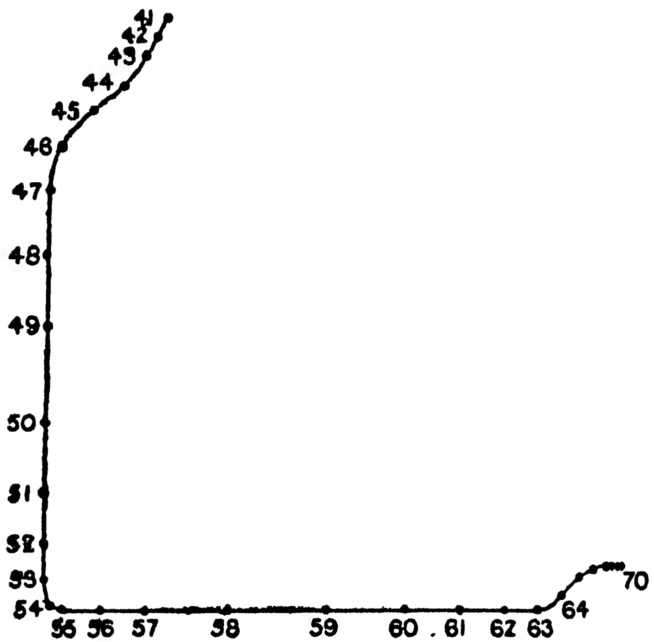


FIG. 7.

rods themselves exist in two states. When the eye is light adapted they are not sensitive to light, and dark adaptation is due to their passing from the one state to the other. The explanation is therefore not an explanation; it merely pushes the difficulty farther on. I am consequently inclined to suggest another reason for the change—better contact of the nerve endings in the synaptic layers. The retina is not a mosaic of independent receptors. There is lateral as well as forward conduction in the synaptic layers. When the eye is dark adapted, at the limit of visibility there is an enormous increase in lateral conduction. It is impossible to go into the evidence here, but this seems to be accompanied by an increase in forward conduction.

The statement is frequently made that one quantum of light delivered instantaneously is just sufficient to excite the sensation of vision. I have made some measurements on the subject in conjunction with Mr. Jas. F. Shearer, and find that this is not the case. For a flash to be visible, 1,400 or 2,600 quanta of green light are necessary according to the observer, and if the light is red or violet, a much greater number of quanta will be required.

ROBERT HOOKE (1635–1703)

By J. R. MORGAN, M.Sc., F.I.C.

PART I

HOOKE was born at Freshwater, Isle of Wight, in 1635, his father, John Hooke, being curate at the parish church. The boy was of a weak constitution, and was prevented from studying by headaches. Left to himself he sought diversion in constructing mechanical toys; among other things he built a clock "that would go," and a model of a ship containing a contrivance for firing guns. On his father's death in 1648, Hooke, who had shown a strong aptitude for drawing, was sent to Sir Peter Lely, the famous painter, to be apprenticed. The smell of paint, however, was too much for his delicate health, and he had to give it up.

Hooke's shrewdness came out even at this early age, if we accept Aubrey's[1] account; his father had left him £100, with which he came to London, and which apparently was the premium to be paid to Lely, but when Hooke saw the painter at work, he "quickly perceived what was to be done, so, thought he, why cannot I do this by myself and keep my hundred pounds?" At any rate, he did not stay long with Lely. Hooke also had some instruction in drawing from Mr. Samuel Cowper, whom Aubrey describes as "the prince of limners of the age." Later he entered Westminster School, and lived in the house of Dr. Bushy, its famous head-master. Dr. Bushy encouraged him in the study of mathematics, allowing him special time for the purpose.

From Westminster School he went to Oxford in 1653, becoming a student at Christ Church. He was elected M.A. in September 1663, on the nomination of Lord Clarendon, Chancellor of the University.

At Oxford Hooke assisted Willis in his chemical experiments, and was afterwards recommended by him to Boyle, to whom he taught Euclid's elements, and the philosophy of Descartes. Hooke, through his association with Willis and Boyle, soon became acquainted with the remarkable band of

[1] Aubrey, *Lives of Eminent Men*, ii, 403, London, 1813.

Experimental Philosophers residing at Oxford at that time, and "for his Facility in Mechanick Inventions was much prized by them." Hooke, giving an account of the meetings of the Oxford philosophers, states: "At their meetings, which were about the year 1655 (before which time I knew little of them) divers experiments were suggested, discoursed and try'd with various successes, tho' no other account was taken of them but what particular persons perhaps did for the help of their own Memories; so that many excellent things have been lost, some few only by the kindness of the authors have been since made publick; among these may be reckon'd the Honourable Mr. Boyle's Pneumatical Engine and Experiments."

Hooke led a very busy life at Oxford. He claims to have "contrived and perfected the air pump of Mr. Boyle." He invented many devices for flying and for moving quickly on land and water, several designs being shown to Dr. Wilkins, who had stimulated his imagination by lending him his book, *Mathematicall Magique*. The study of astronomy also attracted him, and at the suggestion of Dr. Seth Ward he perused the *Almagestum*, by Ricciolus, finding there some hints about the pendulum, which he tried to improve for taking accurate astronomical observations. Attempts to improve it further, for finding longitude, led him to use springs instead of gravity for making a body vibrate in any position, and he made various models, which proved so successful that he acquainted Boyle and others of his invention, and asked their assistance to make use of it. Hooke contrived several astronomical instruments about this time "for making observations both on sea and land," which were afterwards produced before the Royal Society. He also made experiments on the circular pendulum, which were continued in London in 1663, and to which many references are made in the *Journal of the Royal Society*.

The greater part of the Oxford society had migrated to London by 1659, and usually met at Gresham College in the city. Out of these meetings grew the Royal Society, which was founded in 1662 "for the Improvement of Natural Knowledge." Hooke took no active part in the foundation of the Society, and he was not present at any of the meetings that led to its formation; he was probably still resident at Oxford, although Boyle, his employer, was often in London. There is no record as to the terms under which Hooke was employed by Boyle. It seems probable that he was asked to investigate various problems in which Boyle was interested, the latter supplying the necessary apparatus. In addition to this Hooke undoubtedly carried out various investigations of his own,

independently of Boyle, and published them in his own name. Thus in 1661 he had printed a tract on capillarity. This, according to Waller, together with his former performances, made him much respected by the Royal Society.

At first the Society depended entirely on the good-will of certain Fellows to provide experiments at the meetings, but as this method was found to be somewhat unreliable, it was felt necessary to employ someone who would be able, and willing, to do this with regularity.

On November 5, 1662, Sir Robert Moray, one of the most influential of the Fellows, proposed that a curator of experiments should be appointed, but made it clear that no recompense could be offered, as the Society was not in a position to give any. The proposal was unanimously accepted, and Hooke was suggested. Moray formally proposed Hooke as curator at the next meeting (November 12, 1662), and he was duly appointed, and "it was ordered that Mr. Hooke should come and sit among them, and both bring in every day three or four of his own experiments, and take care of such others as should be recommended to him by the Society." Boyle was thanked for dispensing with Hooke's services; Aubrey says that Boyle recommended Hooke as curator, "wherein he did an admirable worke to ye common-wealth of learning, in recommending the fittest person in the world to them." On June 3, 1663, Hooke was elected a Fellow of the Society by the council and exempted from all charges.

On February 25, 1663, he read before the Society a comprehensive scheme of enquiries to find out the true nature of air, hoping the members would take the matter in hand. They did so by asking him to perform such experiments as might help to solve the constitution and substance of the air! Hooke was also charged to bring in at every meeting one microscopical observation at least. On April 15, 1663, he was ordered not to fail to bring in, for the next meeting, a list of experiments to be made in the air pump; later he was asked for experiments upon every head of his enquiries on air.

In spite of hard work, however, Hooke's enthusiasm for the new method was unabated. It equalled that of Bacon himself: "Talking and Contention of Arguments," wrote Hooke, "would soon be turned into labours; all the fine dreams of opinions, and universal metaphysical natures, which the luxury of subtle Brains has devised, would quickly vanish, and give place to solid Histories, Experiments, and Works."[1]

Not only was the new philosophy "a matter of high rapture

[1] Hooke, *Micrographia*, Preface, p. 7, London, 1667.

and delight of the mind," but also " a material and sensible pleasure," and he strongly recommends it to the gentlemen of the nation. Hooke, however, did not make the mistake of neglecting the theoretical aspect, for he says that " 'tis not enough to know how to manage an instrument, or to have a good eye or a dextrous and steady hand, but with these there must be joyned a skilfulness in the theoretical and speculative part."

Thus we have a real enthusiast filling the part of curator to the Royal Society—a man eminently suitable for the post not only because of his wonderful skill, but also because of his great faith in the method of experiment. He devoted his great energy to the work of the Society, both in defending it against various attacks on theoretical grounds, and in keeping the meetings alive by supplying experiments. In a draft of a minute by him, Hooke says: " This Society will not own any Hypothesis, System or Doctrine of the Principles of Natural Philosophy proposed or maintained by any philosopher ancient or modern. Nor dogmatically define, nor fix axioms of Scientificall things, but will question and canvas all opinions, adopting nor adhering to none, till by mature debate and clear argument, chiefly such as are deduced from legitimate experiments, the truth of such positions be demonstrated invincibly."

Hooke was a firm believer in collecting and classifying natural objects, and strongly advocated that the Society should have in the repository [1] " as full and complete a collection of all varieties of natural Bodies as could be obtained, where an enquirer might be able to have recourse, where he might pause, and turne over, and spell, and read the Book of Nature, and observe the Orthography, Etymologia, Syntaxis, and Prosodia, of Nature's Grammar, and by which, as with a Dictionary, he might readily turn to and find the true Figure, Composition, Derivation and use of the Characters, Words, Phrases and Sentences of Nature written with indelible, and most exact, and most expressive letters, without which Books it will be very difficult to be thoroughly a Literatur in the language and Sense of Nature. The use of such a collection is not for Divertisement, and Wonder and Gazing, as 'tis for the most part thought and esteemed, and like pictures for children to admire and be pleased with, but for the most serious and diligent study of the most able Proficient in Natural Philosophy—I could heartily wish that a collection were made in this Repository of as many varieties as could be procured of those kind of Fossile shells and Petrefactions which could be no very difficult matter."

[1] The West Gallery in Gresham College was selected as Repository.

Hooke also drew out several proposals for the good of the Royal Society,[1] many of which were ultimately carried out. He recommended that every member of the Society should be allowed to be present at all meetings, have free access to the Library and the Repository, and the right to use the instruments; if absent, members might have an account of all experiments, observations, discourses, information from foreign parts or any other matter discussed at the meeting. Only Fellows, however, were to have this information.

Every member should pay 52*s.* per annum, and do some duty demanded by the council, once a year; if anyone were to refuse then they would have to pay a forfeit of 52*s.* every year. These duties allocated by the council might be some particular trial or experiment; the history of some trade, manufacturing process, or country; correspondence; or taking care to provide some experiments for the meetings. There should be at least two secretaries and two curators by office, the curators' salaries to be small, but other encouragement to be given in the form of money, plate, medals, or gratuities for inventions or new discoveries. A certain number of the Society to be appointed to manage the prosecution of any new invention so as to bring it into use, and make it profitable for the Society and the inventor.

On July 6, 1663, Hooke, with the help of the operator, was asked to prepare some experiments for the King's entertainment; this involved the repairing of the air pump, so that experiments tried in it were certain to succeed, the provision of a "handsome book" of his microscopical observations, the weighing of air both inside and outside the pump, the breaking of glass bulbs by exhausting them of air, the getting ready of the barometer, and also the hygroscope made with the beard of a wild oat. This list was added to considerably later, and on October 14 he was desired to lodge some days in Gresham College so as to be able to see to these matters. On December 7 he was invited to live in Gresham College four days a week, and to be paid £1 a week for seeing to the above experiments for the King's reception. Hooke was asked to attend the council meeting every Monday to report progress. Experiments were to be considered by the Council on Monday, and to be made at the ordinary meetings of the Society on the following Wednesdays. Also experiments were required in reserve for any extraordinary occasions.

The registers of the Royal Society testify to the keenness and energy with which Hooke applied himself to one problem after another; he touched all topics of scientific interest in his day, and made definite contributions to most of them.

[1] Royal Society, *Classified Papers*, Dr. Stack's Collection, No. 50.

In the early days of the Society he tackled the problem of the function of air in respiration and combustion, the laws of falling bodies, the improvement of land carriages and diving bells, methods of telegraphy, the relation between the height of the barometer and weather conditions, temperature measurement, experiments on the pendulum, and the number of vibrations corresponding to musical notes, to name but a few.

The Fellows of the Society felt that they could hardly expect Hooke to devote his time to their interests without some recompense, so when the chair of geometry in Gresham College became vacant, they suggested Hooke as a suitable candidate for the post. Another candidate, however, a Dr. Dacre, was elected on May 20, 1664, by the casting vote of the Lord Mayor. This was strongly resented by the Fellows of the Royal Society, who protested against the decision, because the Lord Mayor, not being on the Gresham Committee, had no right to vote. A small committee was elected to go into the matter and to do justice to Hooke. Unfortunately, nothing came of it, but when Dacre resigned on March 20, 1665, he was succeeded by Hooke.

About this time Sir John Cutler, a wealthy city merchant, expressed his willingness to settle £50 per annum on Hooke for life, provided he delivered lectures at Gresham College on any subjects the Society might deem fit. On June 22, 1664, a committee was elected to decide what form the lectures should take.

On January 11, 1665, it was decided to pay Hooke a salary of £30 per annum, which with Cutler's allowance made £80 per annum. Although the Society now had a paid curator, it still observed the original practice of having special curators for particular experiments.

No meeting was held from June 28, 1665, to January 1666, because of the Great Plague raging in London, and the subsequent Great Fire. Hooke accompanied Sir William Petty and Dr. Wilkins, two of the founders of the Society, to the country, where he continued to carry out experiments. Since the City of London was in ruins after the fire, Gresham College was required for the use of the Lord Mayor and merchants, so the Society had no meeting-place. Mr. Howard, a rich nobleman, came to their rescue by offering them rooms in Arundel House in the Strand, which were accepted. It was proposed to build a college in the garden, and Hooke and Wren drew designs for the building.

About this time Hooke exhibited various instruments of his own invention to the Society—a screw-divided quadrant (the first ever made), an anemometer, and a weather clock; he also applied the circular pendulum to clocks.

During the early years of the Royal Society, Hooke devoted a great deal of his time to astronomical observations, which he communicated to the Society. In this sphere also he achieved some very remarkable things. He inferred the rotation of Jupiter from the movement of a spot which he first noted on May 9, 1664 (the period was determined by Cassini, the French astronomer). His drawings of Mars served Proctor, over two centuries later, to fix the exact rate of rotation of that planet. Hooke also casually noted the fifth star in the Orion trapezium. Careful observations of the comet of 1664 were also made by him, and he evolved a theory of comets out of his observations, which were communicated to the Royal Society in 1666.

By 1667 all the experimental work of the Royal Society fell on Hooke, and he was failing to cope with it. It is obvious that many members of the Royal Society felt that Hooke was at times neglecting his duties as curator, although there was a general feeling that he was the ablest man for the task. In fact they were relying far too much on one man, for when Hooke was absent no experiment was performed. Most of the hard work seemed to fall on Hooke; he was in charge of the Society's depository; the cataloguing of the library fell to his lot; he became secretary on Oldenburg's death (October 25, 1677), and this post involved editing the *Transactions* (Hooke called them *The Philosophical Collections*), translating and answering letters, reviewing books, and seeing to the collection of arrears due to the Society. In addition, he was to prepare one or two experiments for the weekly meetings, and for all this he received £30 per annum![1] The position was impossible. On May 8, 1672, Dr. Grew was appointed an additional curator for the anatomy of plants. Hooke declined the post of librarian to the Society in 1679, and pleaded for a full-time helper for preparing experiments. This was agreed to on December 17. He relinquished his post as secretary in 1682. In view of his other activities, the number of experiments carried out by Hooke can only be regarded as amazing.

The unpublished diaries of Robert Hooke throw new light altogether on his character and various activities, and depict a man in many respects very different from the one the historians have presented us with.

One of these diaries, a folio volume of seventy-two pages, covering the period March 1671 to May 1683, is in the Guildhall library[2] of the City of London, where it has been since May

[1] The income from Sir J. Cutler stopped in 1670.

[2] Dr. Hooke's Diary, MSS., 1758. It was brought to my notice by Mr. Robinson, of the Royal Society. His attention was drawn to it by Dr. Pilseneer, a Belgian student.

1891. Apparently after the auction of Hooke's effects it found its way to Moor Hall, Harlow, Essex, from where it was bought by the City of London.

The other diary, which Hooke wrote on small scraps of paper—there are ninety-six of them bound together—covers the period (with various gaps) from 1688 to 1693. This is in the possession of the British Museum,[1] and is catalogued as J. Petiver's diary, although it is obviously Hooke's, bound together by Petiver.

After the Great Fire, Hooke was appointed as Wren's assistant to rebuild the City of London, and this occupied a great deal of his time for many years. On September 19, 1665, he exhibited to the Society his model for the rebuilding of the City. Waller said that "this model designed the chief streets, as from Leadenhall to Newgate, and the like, to lie in a straight line, and all the other cross-streets turning out of them at right angles." The plan was also shown to the Lord Mayor and aldermen of the City, who "expressed a desire that it might be shown to his Majesty, they preferring it far before that which was drawn by the surveyor of the city." Christopher Wren, however, had previously drawn a model, and shown it to the King. Although Hooke's plan was not adopted, it secured for him the appointment referred to above. His activity in this connection is often referred to in his diaries: "At Grocer's Hall and valued it. Gave Milner draught of Merchant Taylor garden. . . . Increased my cold much by measuring wharfes. . . . Perfected module of Piller. With Mr. Story measuring at ye Physicians College ye stone-work. Made some views with Dr. Wren at Poultry and Guildhall. . . . With Dr. Allen at Bedlam; viewed more fields for new Bedlam, and drew a report for him. . . . Delivered report to town-clerk . . . with Captain Paggin to view building in Old Street." Even as late as 1691 he was actively engaged in building a hospital near Hoxton.

Hooke seems to have also taken on private contracts as architect and surveyor. Thus he was desired to draw a model for a school, school-house, and twenty almshouses for Sir W. Turner; he was asked to direct an observatory at Greenwich Park for Sir Jonas More; he built a new palace for Mr. Montague near Bloomsbury[2]; and also the College of Physicians. He was engaged not merely as architect and surveyor for these various buildings, but actually as contractor as well, for he bought lead, bargained as to the price of stone-work, gave actual estimates of cost, etc.

[1] Sloan MSS. 4024. That this was a diary of Robert Hooke was discovered by Mr. Robinson, of the Royal Society.

[2] Later the British Museum.

He was also interested in glass manufacture, and he applied for a patent on April 21, 1691, for making special kinds of glass. This was granted on May 19; a warrant was issued to "Robert Hooke Esq. and Christopher Dodsworth, merchants, their executors, administrators and assigns, of the license and privilege for the sole use of mixing metal, so as to make glass for windows of more lustre and beauty than that heretofore made in England, red crystal, glass of all sorts, and also the art of casting glass, particularly looking-glass plates much larger than ever blown in England or foreign parts." [1]

The trade of glass making was apparently not very flourishing at this time, and various manufacturers petitioned the Government to be incorporated together under the name of "the Company of Glass Makers of London." Hooke was named as the warden of this company.

He undoubtedly found these various activities very profitable, at least far more so than those of curator to a bankrupt society, whose payments were consequently almost invariably in arrears. In fairness to Hooke, it must be admitted that, had he regarded his office of curator as a full-time post, things would have gone very hard with him. As we have already seen, the Society decided to pay him £80 per annum, £50 of this being paid by Sir John Cutler; the latter income, however, ceased in 1670, and the remaining £30 was hardly to be depended on, especially as regards the time of payment. Small wonder then that Hooke devoted a large proportion of his energies to the rebuilding of the City, his work for the Royal Society being more in the nature of a hobby. Under the circumstances this was inevitable: Hooke, having no private means, had to work for his living.

As we have already seen, the Fellows were eager for more experiments than Hooke was able to provide, so the Council of the Society endeavoured to make him a little more assiduous. They passsed the resolution (June 1683)—"that Mr. Hooke receive every meeting day order for bringing in two experiments at the next meeting day, together with a declaration by word of mouth of the design and purpose of the experiments, and an account in writing of the history thereof, and the purpose as aforesaid, such as may be fit to be entered in the register: and that at the end of every quarter there shall be a meeting of the council, where his performance shall be considered and a gratuity ordered him accordingly; and that from this time he have no other salary." Hooke accepted these conditions, but he appears to have been slow in presenting the required account of his experiments. The Society asked for

[1] *Calendar of State Papers, Domestic Series* (1690–91), pp. 341, 381, 537. London, 1898.

them on December 12, 1683, and Hooke promised to deliver them by Christmas. They were again asked for on January 16, 1684. On April 16, 1684, Hooke was paid £7 10s. for experiments brought in till last Christmas, and it was pointed out that when he brought an account of experiments made since, the council would pay him accordingly. The Society, however, was very anxious to retain his services, and would have made it worth while for him to devote his whole time to the office of curator had its finances been in a more favourable state. At a meeting held on June 22, 1681, it was passed that "in consideration of the propositions made by Mr. Hooke for a more sedulous prosecution of the experiments for the services of the Society, and particularly the drawing up into treatises several excellent things, which he had formerly promised the world; the council, as an encouragement, according to the small abilities of the Society, agreed to add to his salary £40 for this year ending at Christmas Day." Again, on November 24, 1683, we find that the council resolved "that Mr. Hooke should be owned [1] and assisted by the Society as far as lawfully they may." They gave him £15 on account as part of what he was going to receive, when he brought in the account of his experiments. Hooke was asked on January 5, 1687, to make his own proposal regarding his experiments and salary; this he did at the next meeting. He proposed that he would produce one or two experiments and a discourse at every meeting, provided his salary be made £100 per annum. The Society decided to pay him £50 per annum and to give him lawful assistance to recover his promised income from Sir John Cutler. They voted that the payment of Hooke's salary should have preference over all other matters; in fact, the Society knew full well that Hooke was its most important asset, and that without his experiments it would have been dead long ago. Pious resolutions do not produce the cash, however, and Hooke's salary was soon in arrears once more. On July 6, 1687, the Society decided to pay him in copies of *The History of Fishes*,[2] but he was not very anxious, so he asked for six months to consider whether he would accept such payment or not. There is no further mention of this matter.

Hooke has been represented as a mean, avaricious man. Waller represented him as a miser, spending as little as possible, although he had a large iron chest, containing many thousands in gold and silver, in his possession. Hooke's diaries indicate

[1] This presumably means that they would like his services entirely for the Society.

[2] This was a book by Willoughby, of which the Society had several copies. Halley's salary as clerk was paid in the same manner.

a boon companion rather than a miser. In his prime, at any rate, he must have been a kindly man, for, having visited Dr. Whistler, he wrote in his diary what fine children he had; he was very concerned about the health of Madam Tillotson; he proposed a Mathematical School for instructing boys in the Principles of Astronomy and Navigation at Hoxton; he had meant to dispose part of his fortune in building a handsome edifice for the Royal Society "with a Library, Repository, Laboratory, and other conveniences for making experiments, and to found and endow a Physico-Mechanick Lecture of the Nature of what he himself read"; he wrote to his brother "proffering £4,000 for Avington," saying that he preferred to do good to friends while he lived.

Weld [1] regards Hooke as mean in his relation with the Royal Society, because he clamoured for an increase in salary when the funds were low. This is unfair to Hooke. All he did was to name his conditions for producing weekly experiments in answer to a request of the Society on the point. Hooke was undoubtedly a shrewd business man who knew his own value, and was not willing to work for anyone—not even the Royal Society—for a mere pittance.

In appearance Hooke does not seem to have been at all impressive; Pepys [2] refers to him as "Mr. Hooke, who is the most, and promises the least, of any man in the world." Waller describes him as pale and thin, and though he was moderately tall, yet he appeared short because he was so bent; his eyes were grey and full, with a sharp ingenuous look when younger; his mouth was wide, and upper lip thin; his chin was sharp, his forehead large, his head being of middle size, his long brown hair hung neglected over his face, "uncut and lank," until about three years before his death, when he cut it, and wore a wig. Aubrey's description is not much more complimentary: "He is of middling stature, something crooked, pale-faced, and his face but little belowe, but his head is large, his eye full and popping, but not quick; a grey eye. He has a delicate head of hair, browne and of an excellent moist curle."

The historians from Waller onwards have been very hard on Hooke, depicting him as being of a "melancholy, mistrustful, and jealous temper," and becoming more irritable as he grew older. "From his youth," said Waller, "he had been used to a Collegiate, or rather Monastick Life, which might be some reason of his continuing to live so like a Hermit or Cynick too penuriously—scarcely affording himself necessaries." This may have been a true picture of Hooke as an old

[1] Weld, *A History of the Royal Society*, London, 1848.
[2] *Diary*, iv. 354, Wheatley Edition, London, 1893–9.

man, but it is certainly belied by every page of his diaries. In fact, Hooke was anything but a hermit; although heavily handicapped by very poor health, he believed in having his share of the good things of life. Much of his ill-health seems to have been due to indigestion after over-indulgence in food and drink at his friends' houses or in taverns. On January 19, 1675, we find him taken with a violent fit of "ye Collick" after eating "neats tongue and cabbage and drinking sack, chocolate and tobacco." Again, on February 8, 1675, he drank eight glasses of wine with some friends at the Fleet Tavern, and having returned home took medicine.

Undoubtedly Hooke was a very sociable man; in fact, he was seldom at home, but could generally be found in some tavern or other with friends from the Royal Society or the City of London. He dined very often with Sir John Cutler, the city merchant, Dr. Tillotson, later Archbishop of Canterbury, Dr. Busby, the famous head master of Westminster School (Hooke had lived in his house when at school), Sir William Petty, Lord Brouncker, and many other notable figures of the day. When he called on the Lord Mayor of London on some city business, he was "earnestly invited" to dine with him. Again, Dr. Siddenham, the "father" of English Clinical Medicine, with whom Hooke was riding to Acton, invited him to stay with him six weeks, so pleased was he with his company. They discussed "Physick, Religion, and Philosophy." We have already seen that he stayed in the country with Petty and Wilkins during the Plague. He drank sack, claret, and small beer, even spirit of wine. He smoked a great deal—he just dropped on his friends for a smoke and a chat. He often called at the various taverns and coffee houses of the City, where he invariably met someone he knew, and consumed large quantities of coffee, tea, or chocolate while listening to the gossip of the day. Very many people called on him at Gresham College, and he must have had a fair cellar, as he often supplied them with wine. Thus on January 7, 1674, we find the entry—"W. Giles here, gave her sack, Mr. Kerry here, sack, Mr. Haak here, sack"; on December 13th, 1675, he took a Mr. Crisp to his dining-room and gave him a "bottle of Porto." He often put down a note about paying for or receiving wines—"I gave five shillings for three bottles of Port . . . paid Mayor for Brandy, three and a half shillings per gallon . . . bought of Blackgrave two quarts of Claret and paid him; . . . from Mr. Peirw, seven sack, seven claret . . . from Dr. Tillotson one dozen sack, one dozen white wine . . . from Mr. Bostock twenty-four bottles of Canary . . . Haak sent one gallon of brandy." He often played chess with Haak. Thus did

Hooke live his "Monastick Life . . . scarcely affording himself necessaries"!

The latter part of Hooke's life was nothing like as productive as the earlier, although he continued to give lectures and demonstrations before the Royal Society almost to the end of his days. Even as late as June 1698 he delivered a lecture, and also showed the Fellows a model of Saturn and its ring. Hooke's comparative lack of activity was largely due to a decline in his health, for he suffered greatly from giddiness, and was soon tired by any exercise. In 1697 he complained of swelling and soreness in his legs, and was overrun with scurvy. The swelling of his legs broke, and before his death they began to mortify for want of due care. He was present, however, at a council meeting of the Society held on April 1, 1702. The last thing he is believed to have written was on December 17, 1702, when he put down a memorandum of an instrument for measuring the horizontal diameter of the sun to the tenth of a second.

Hooke's great ability was recognised by his election as Doctor of Physic at Doctors' Commons in December 1691.

He died at Gresham College on March 3, 1703, and was buried at St. Helen's Church, Bishopsgate Street, "all the Members of the Royal Society then in Town attending his Body to the grave, paying the Respect due to his extraordinary Merit."[1]

[1] Waller, *The Posthumous Works of Dr. R. Hooke*, Introd., p. xxvi, London, 1705.

ESSAY

BARROW, NEWTON, AND LEIBNIZ, IN THEIR RELATION TO THE DISCOVERY OF THE CALCULUS [J. M. Child, University, Manchester].

Part I

BARROW

The year 1927 saw the honour due to the genius of Newton fittingly celebrated by the pilgrimage to Grantham. Eleven years earlier, the two-hundredth anniversary of the death of his great contemporary, Leibniz, occurred during the turmoil of the Great War. It seems, therefore, an opportune time to press the claims of another great English mathematician for due recognition ; one from whom both Newton and Leibniz could not have failed to have derived inspiration, Isaac Barrow (1630–77).

Overshadowed, maybe, by the wider and deeper genius of Newton and Leibniz, he yet stands pre-eminent as the greatest geometrician of his period. For Newton, the physicist and astronomer, the analyst and applied mathematician, the development of higher mathematics was a mere necessity, without which he could not proceed ; for Leibniz, the lawyer and logician, the scientist and philosopher, higher mathematics was but one branch of science in general. The sole aim of Barrow was the creation of an instrument wherewith he could find the tangent to, and the area under, any curve of which the equation was known, or any curve which could be constructed as *a combination of such curves.* This singleness of purpose, conjoined with the general opinion of the period that all proofs should be in geometrical form (to which opinion Barrow more than subscribed) prevented him from being universally acknowledged as the "Father of the Calculus."

Barrow's *Lectiones Geometricæ*, first published in 1670, as a supplement to the second edition of the *Lectiones Opticæ*, consisted of :

(i) *Five Preliminary Lectures, Lectures I–V.*—Apparently, to judge from Barrow's preface, these were written in 1669–70. They are philosophical in character at first, dealing with the

descriptions of areas by the parallel motion of lines, the description of curves by the intersection of two lines having parallel motion, and so on; and gradually merge into Cartesian geometry, and the proof that certain curves of the second degree, as given by their equations, are hyperbolæ.

(ii) *Seven Lectures, Lectures VI–XII.*—These were ready for press in 1669, being intended as a supplement to the Optical lectures. Prof. Love sets the date of *writing* as early as 1663–4; I do not see that there is any tangible evidence that this was the case, but it is not too much to suppose that the whole of the material had been worked out, even if not written up in form for press, at this time. It seems fairly clear that the matter was the result of his duties as Professor of Geometry at Gresham; and possibly Prof. Love is right.

(iii) *The Several Supplements.*—These were probably known at much about the same time as the seven lectures; but almost certainly were not written out for press until 1669–70. For Barrow states that these, "being particular theorems, were beyond the original intention," and that they were added at "the request of a friend who thinks they are worth the trouble."

(iv) *Lecture XIII.*—This lecture consists of geometrical methods of determining the roots of several sets of connected equations, which are not considered in this article.

Before I start trying to show that the seven lectures in group (ii), with the supplements, form a complete treatise on Differential and Integral Calculus, I wish to state clearly that which to me constitutes a "Calculus." A Calculus may be of two kinds:

(i) An *analytical* calculus, properly so called, that is, a set of algebraical working rules (with their proofs), with which differentiations of known functions of a *dependent* variable, of products, of quotients, etc., can be carried out; together with the full recognition that differentiation and integration are inverse operations, to enable integration from first principles to be avoided; the use of substitutions for the independent variable; and the integrals of the usual standard forms. It would be the more complete if the theorem for the rectification of an arc, and that for volumes of solids of revolution, were included; but these I consider only as applications.

(ii) A *geometrical* equivalent embodying the same principles and methods; this would be the more perfect if the constructions for tangents and areas could be immediately translated into algebraical form, if it were so desired.

Between these two there is, in my opinion, not a pin to choose theoretically; it is a mere matter of practical utility

that sets the first type in front of the second; whereas the balance of rigour, without modern considerations, is all on the side of the second. I do not consider that the invention of the calculus lies in the use of the differential triangle (Pascal, Fermat and others), nor in the use of the infinitesimally small (Archimedes, Cavalieri, Wallis, etc.), nor in infinite summations, embodied in the principle that an area was formed either by the parallel motion of a straight line, or as the sum of a number of parallel or rotating lines, or as the sum of a number of small triangles or rectangles, which Pascal showed to be one and the same principle with a different mode of statement only (Archimedes, Cavalieri, Pascal, etc.). Neither does it lie in Newton's method of series, which demanded no more than the integration of a power of the *independent* variable (except the case of an index equal to -1), or its differentiation, and which did not yield an exact result except when the resulting series happened to be known beforehand, nor even a workmanlike approximation except when the variable was really small; nor does it lie in the invention of suitable notations, for we see how far Leibniz could progress before he substituted his integral sign for the words "sum. omn."

It lies, firstly, in the complete recognition that integration and differentiation are inverse operations, *i.e.* that the drawing of tangents and the finding of areas are intimately connected; secondly, in giving the differentials and integrals of the usual functions of a *dependent* variable, *i.e.* of functions of a function, and especially dealing with the circular, logarithmic and exponential functions *per se*, *i.e.* accurately and without the necessity of expressing these as series; thirdly, in giving rules for dealing with products, quotients, and powers of such functions; lastly, and perhaps most discriminatingly, it lies in the fact that there is evidence of an ordered attempt to weld all these different things *into a branch of science.*

Now Newton's tract of 1669, the *De Analysi*, seeing that it was communicated to Barrow as from a former pupil to his tutor, would no doubt have contained all that he (Newton) knew at the time; and was thus communicated, as I suggest, to make good a previous omission, mentioned later in a letter to Collins, dated December 10, 1672: "I remember on one occasion that I told Barrow, when he was engaged in preparing his lectures, that I was in possession of such a method for drawing tangents; but I do not know by what digression I was prevented from describing the method to him." In this tract, Newton is not even using the Binomial Theorem to extract a square or other root, but does it "more arithmetico"; his differentiations and integrations are merely of powers of

the independent variable, *i.e.*, as we should now say, by first principles, using the *o*-method and Wallis' rule. Newton certainly has in a note: "If $\sqrt{aa+xx}=z$ (area), we get by calculation, $x/\sqrt{aa+xx}=y$." But the context makes it almost certain that the "calculation" was by the *o*-method, as follows:

If $\sqrt{aa+xx}=z$, then $aa+xx=zz$;
hence $aa+(x+o)(x+o)=(z+yo)(z+yo)$;
therefore, by subtraction and omission of the terms containing oo, and division by o, we have

$$2x=2zy=2\sqrt{aa+xx}\,.\,y;$$

and the result follows immediately.

There is nothing to suggest the following:

$$\text{If } y=\sqrt{z}, \text{ then } \frac{dy}{dx}=\frac{1}{2\sqrt{z}}\cdot\frac{dz}{dx},$$

or Newton's equivalent for it.

Thus, in 1669, Newton is only on the threshold of the calculus, in spite of all the wonderful results he has obtained in his retirement to Woolsthorpe, and since his return to Cambridge in 1667; yet these results enable him to advise Barrow to include five examples of the use of the *a*- and *e*-method, and the supplements.

As for Leibniz, beyond the desultory reading of certain mathematical texts, which he enumerates, he has practically no knowledge of mathematics whatever; perhaps, because he has been far too occupied in his legal, logical, historical, and political studies.

Thus, to prove that Barrow was the inventor of the calculus, in the form I have enunciated in type (ii) above, all that remains to show is that this form was in the possession of Barrow, *as a complete subject and recognised by him as such*, at any rate in 1669, if not several years earlier, as suggested by Prof. Love in the matter of the seven lectures.

First of all, Barrow is convinced of the extreme importance of the inverse nature of differentiation and integration; for he discusses this directly and conversely in two separate propositions with corollaries (Lect. X, Prop. 11, and Lect. XI, Prop. 19). It will be sufficient to quote the enunciation of the first:

"Let ZGE be any curve of which the axis is AD; and let ordinates applied to this axis, AZ, PG, DE, continually increase from the initial ordinate AZ: also let AIF be a line, such that, if any straight line EDF be drawn perpendicular to AD, cutting the curves in E, F, and AD in D, the rectangle contained by DF and a given line R is equal to the intercepted area ADEZ; also let DE : DF = R : DT. Then TF will touch the curve AIF."

Now, if we translate this into modern notation, we have :

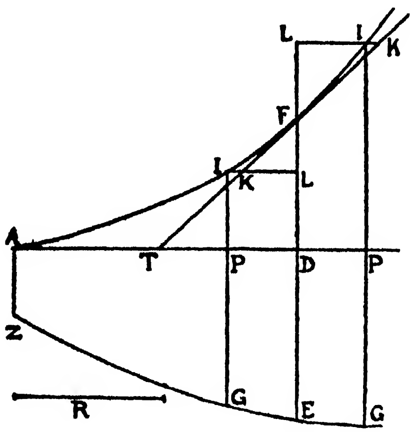
Let the equation of ZGE be $y = f(x)$, so that the area ADEZ $= \int f(x) . dx$; also let the equation of AIF be $y = \phi(x)$, and let $\int f(x) . dx = \phi(x) . R$. Then, if FT is the tangent to AIF, so that $DF : DT = d[\phi(x)]/dx$, we have

$$d[\phi(x)]/dx = DE/R = f(x)/R ;$$

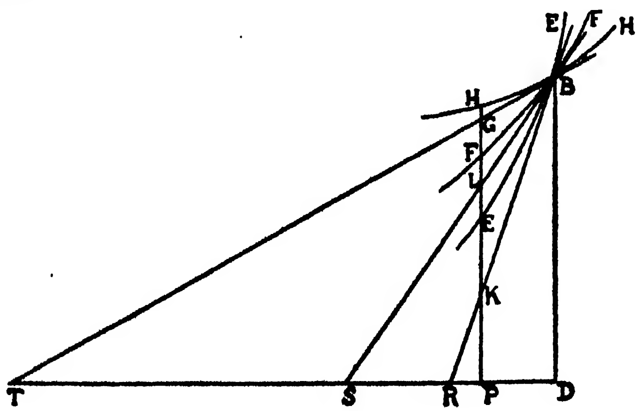
or rather, it is the converse of this that Barrow enunciates. Now, if we take R to be of unit length—for R is only introduced to keep the dimensions correct—the translation of Barrow's theorem is: If

$$\int f(x) . dx = \phi(x),$$

then $d[\phi(x)]/dx = f(x)$.



Secondly, Barrow has the differentiation of a product, a quotient, and any power, positive, negative, integral or fractional (Lect. IX, Prop. 12, Lect. VIII, Prop. 9, Lect. IX, Prop. 3, and combinations of these). For instance, the enunciation of Lect. IX, Prop. 12—which is the third of a group of theorems on arithmetical and geometrical means, for which Barrow, so to speak, uses oblique axes—may be stated for the particular case in which the axes are rectangular, and with Barrow's diagram rotated into what is the more usual modern position, as follows :



If EBE, FBF, HBH are three curves so connected with a straight line RD, that $PG^{M-N} \cdot PF^{N} = PE^{M}$, and if DR, DS, DT are the subtangents intercepted on the line RD, then assuming two of these subtangents to be known, the third is given by the proportion

$$N \,.\, TD + (M - N) \,.\, RD : M \,.\, TD = RD : SD.$$

This may be written in the form (which by Barrow would have been considered illegitimate): $M/SD = N/RD + (M - N)/TD$; and thus the theorem is equivalent to *a generalised rule* for dealing with products and quotients, stated in a form such as would be nowadays derived by logarithmic differentiation:

$$\text{If } u^{m+n} = y^{m} \,.\, z^{n}, \text{ then } \frac{m+n}{u} \cdot \frac{du}{dx} = \frac{m}{y} \cdot \frac{dy}{dx} + \frac{n}{z} \cdot \frac{dz}{dx}.$$

Thirdly, the appendices contain theorems equivalent to the integration of the missing functions, *i.e.* those that could not be written down as inverse differentiations. Thus, by discussing the curve in which equidistant ordinates to an axis are in geometrical progression, and showing that the subtangents are constant, Barrow obtains $\int dx/x$ and $\int a^{x}dx$ (Lect. XII, App. III, Prop. 3, 4); also $\int \tan\theta d\theta$ and $\int \sec\theta d\theta$ (Lect. XII, App. I, Prop. 2, 5), $\int dx/(a^{2} - x^{2})$ and $\int dx/(x^{2} + a^{2})^{\frac{1}{2}}$ (Lect. XII, App. I, Prop. 8, 9); these being all reduced to equivalent areas under a rectangular hyperbola, which were known to be logarithms.

Challenged by the late P. E. B. Jourdain, at whose instigation I had been led to make a thorough study of Barrow's *Lectiones Geometricæ*, I showed that, using nothing but geometrical theorems given by Barrow, it was possible to give a geometrical construction, that involved the drawing of not more than a dozen lines, for the subtangent of the curves

$$y = \sqrt{(\sin x \,.\, \log_{10} \cos x)} \pm \frac{1}{10} \sqrt[3]{(3 \tan x/x^{2})};$$

and, what is more, ***the steps of the construction could be immediately translated into the very analysis that would be used at the present day for such a differentiation.***

Want of space prevents me from doing more than mention that Barrow had the equivalent of the proof of the fundamental theorem for rectification of an arc, that theorems using Cartesian co-ordinates were usually followed by analogous theorems for polar co-ordinates, that he used an Euclidean definition of

tangent for theorems on differentiation, that he gives the condition $x/r = (c-x)/s$ for the maximum value of $x^r(c-x)^s$, and the minimum value of $x^r/(x-c)^s$, and that he even has a "practical" method for the area under a curve which cannot be "integrated" by any of his theorems. But for these and other things I must refer my readers to my little book on Barrow published by the *Open Court.* I think I have brought forward sufficient arguments to prove that Barrow invented the Calculus.

To clinch the matter, there are two observations to make. The first is that some of the constructions are so intricate, so forced and unnatural, that it is impossible to assume that they were not worked out analytically: the very steps in the constructions are nothing but *geometrical translations of the changes in the independent variable which would be the natural steps of an analytical solution.* When it is considered that in his lectures on Apollonius, Barrow endeavours to show that the constructions given are obtained by an analytical reduction of the problem, it is almost impossible to conclude anything else but that Barrow had a complete analytical calculus before the time, from 1663 to 1669, whenever it was in this period that he wrote it down in geometrical form, as a necessary preliminary to publication. The second remark to be made is that Barrow knew what he had done; he knew that he had created a new branch of science. In the *Dictionary of National Biography,* Canon Overton gives a quotation (source not stated) pointing out that Barrow's pique at the neglect accorded to his work was a great factor in making him give up for ever mathematical investigations and turn to Divinity.

If I may be allowed to conclude this part with a simile, I should say that although in the mathematical marathon Barrow had breasted the tape before Newton had arrived at the stadium, while Leibniz was a non-starter; nevertheless, some years later, when Barrow's running days were over, Newton and Leibniz had so profited by his training hints, that they made a dead-heat of it, beating Barrow's record time by minutes, not seconds.

Part II

NEWTON AND LEIBNIZ IN RELATION TO BARROW

Thus, in 1670, Barrow had published a treatise on the elements of the differential and integral calculus as a *complete subject of mathematical science;* and, from internal evidence, it is past a doubt that he had obtained his theorems by the previous use of some analytical process, such as his *a*- and

ϵ-method. The proofs, however, are in geometrical form, and of such a nature that only expert geometers could understand them; and of these latter, probably only such men as Newton, and later, Leibniz, the Bernouillis and Euler, were capable of appreciation of their inner meaning, or of finding the analytical equivalents from which they had been derived. There then arises the question as to whether these mathematicians derived any ideas in their work from this work of Barrow; and, if so, how great was their indebtedness, and why was there either no acknowledgment of this indebtedness, or a denial of it?

Considering the last question first, I find a justification for Newton's silence in the work of Barrow itself. In the preface to the first combined edition of the *Lectiones Opticæ et Geometricæ*, Barrow states that he committed the publication to the care of two friends, one of whom was Newton and the other was Collins: "As delicate mothers were wont, I committed to the foster care of friends, *not unwillingly, my discarded child*, to be exposed or reared, as it might seem good to them." In the preface to the *Lectiones Geometricæ*, a second or special preface, he wrote: "What these lectures bring forth, or to what they may lead, you may easily learn by tasting the beginnings of each." Further, in several places, we have practically an invitation offered to any mathematical reader to make use of his theorems; *e.g.* in Lect. XI, at the end of a set of theorems which are equivalent to those of Pascal, we find: "Should anyone explore and investigate this mine, he will find very many things of this kind. Let him do so who must or will." Again: "Perhaps at some time or other the following theorem, deduced from what has gone before, will be of service; it has been so to me repeatedly." In short, Barrow, through pique at non-recognition of the value of his work, or some other reason, had become "sick to death of" all things mathematical and threw open his work for the use of anyone who cared to follow it up. Personally, if I had been in the position of Newton, I should have considered that this was a special invitation to myself; and, afterwards, having dug in the mine, and by a process of my own converted the crude ore into high-grade steel, I should not have thought it necessary to mention the discoverer of the mine.

In my little book on the *Early Mathematical Manuscripts* of Leibniz, I have given what I consider to be the correct explanation of Leibniz' denial of indebtedness to Barrow—namely, confusion between Pascal and Barrow. This opinion, I acknowledge, was formed solely on a consideration of the few manuscripts that were discussed by Gerhardt; and in the main, this opinion has not been altered, but is corroborated by the recent further publications by Mahnke, which the

author states were investigated and published at the suggestion of Wieleitner, and may therefore have been primarily due to certain correspondence between myself and Wieleitner several years ago. In connection with my argument I deplored the paucity of my material ; but I considered that the perspicacity of Gerhardt had provided the cream of the manuscripts. I even suggested a way in which some multi-millionaire lover of letters might employ his surplus capital usefully ; namely, in financing the publication of every scrap of manuscript that Leibniz wrote on mathematics, carefully dated as far as was possible, and photographically rendered. Now, I suggest in addition, that, to make a perfect job of it, such a benefactor to the history of science should finance a similar publication of all the still unpublished manuscripts of Newton ; together with complete annotated editions of Wallis' *Arithmetica Infinitorum*, Barrow's *Lectiones Geometrica* (my little book is useless for the purpose), *Les Lettres de Amos Dettonville* of Pascal, Fermat's work on maxima and minima, and all other works germane to the issue, where this has not been done ; and these publications should at least be in the two languages, Latin and French, even if national jealousies prevented English and German translations as well. Until something like this has been done, no argument leading to an absolute decision is possible. Failing that, any argument must be founded on an integration of the several minute points of circumstantial evidence that may be found ; such an integration as is supposed to take place in the minds of detectives of fiction, such as "Sherlock Holmes," with whom not even the quality of a cigarette-ash is overlooked. It was such an integration that I attempted to employ in my study of the manuscripts of Leibniz, such as I had ; the book was reprinted from successive articles that appeared in the *Monist*, and in the preface I warned readers that I had not altered their form, so that the work should record my growing convictions as I proceeded, and not my final judgment. Indeed, I stated no final judgment, leaving others to judge ; so that I must vigorously protest against the ill-considered and inaccurate attacks that have been made against me. In one American critique, it was plain that the critic had never read more than the first eighty pages or so ; but, as an example, I will only mention a very mild case of misrepresentation, that of Prof. L. J. Russell, in the Newton memorial number, published in 1927. It must be observed that I distinctly stated that I did not agree with Rouse Ball's rather harsh alternatives : "Essentially it is Leibniz' word against a number of suspicious details pointing against him." Also that, in all cases, I have tried to reconcile statements with facts ; and where these were irreconcilable in my opinion,

I have endeavoured to give an explanation of the discrepancy, ascribing it to perfectly human motives and accidents. No one, in my opinion, can accuse Leibniz of absolute lying or of direct dishonesty ; just as much, on the other hand, no one can say he was perfectly straightforward. He was under the influence of the professional and national jealousies, of which Prof. Russell writes ; just those jealousies which made Newton in later editions of the *Principia* omit reference to Leibniz ; as who would say : " If you are going to make such a to-do about your share in the invention, well, I'm not going to advertise you ! " Further, it must be remembered that, while Newton's position was impregnable, Leibniz had his back to the wall, owing to the discredited, and more or less discreditable, attack of Fatio de Duillier, and what was a direct accusation of his European contemporaries, " you " (Jakob Bernoulli), " your brother, or any others," which was the origin of the " postscript," and the probable reason of the existence of a draft copy which differed in several details.

Prof. Russell's words are : " When Leibniz said that he found, not in Barrow but in Pascal, the theorem leading to his early thoughts on quadratures, and that after he had made his discoveries he found them later in Barrow, Child, not finding in Pascal the diagram corresponding to what Leibniz said, but on the contrary finding in Barrow both the theorem which Leibniz said he discovered and a diagram to which what Leibniz said could be referred, and knowing that Leibniz actually possessed a copy of Barrow, decided that Leibniz was at any rate not speaking accurately ; and from that he went on to ask why." I have never met with such a number of small inaccuracies in one sentence before ; tiny inaccuracies each one of them, but which taken together give a totally wrong presentation of my argument.

Leibniz said : " A proof of Dettonville's that was of a supremely easy nature, by which *he proved the mensuration of the sphere* as given by Archimedes," but does not in this passage state that it was not from Barrow ; the inclusion of the name Barrow in Prof. Russell's words, therefore, gives a wrong impression. The reference to Barrow comes at the very end of the " postscript " which is in question, and the words are " and as in Barrow, when his lectures appeared, in which latter I found the greater part of my theorems anticipated." Yet Leibniz had a copy of Barrow in his possession before he was advised by Huygens to study Pascal ; and he had bought this book a year before *to read*, because he had seen a letter of Gregory, dated September 1670, saying : " I think Barrow has gone infinitely further than all those who have written before him. From his method of drawing

tangents, etc." It was not the "diagram" that I omitted to find in Pascal, as stated by Prof. Russell; it was the whole context of the passage that failed to agree with facts. Leibniz stated that the proof in question was one by which Pascal proved the mensuration of the sphere. It was a full month after I first came across this statement of Leibniz before I wrote the note on p. 15 in my book; during the month I had done a considerable amount of work, namely, written out and typed the whole of "The Letters of Dettonville": I only mention this to show the thoroughness with which I endeavoured to carry out my study. I found that in the tract on sines of a quadrant there was no theorem for the mensuration of the sphere; and that not until I came to the tract on circular solids was there any mention of Archimedes; and then I was struck with amazement to find these words: "Moreover I have *assumed* that the solids of revolution of these spaces about the diameter MF are also given; for *that has been demonstrated by Archimedes.*" It was then, and not after I had found the figure in Barrow, that I sought for the reason. I remembered that I had seen something like the two figures of Leibniz given in Barrow; I searched for them and found them, as I then thought, in Barrow's Lect. XI, Prop. 1; and it was considerably later before I recognised them both in Lect. XII, Prop. 1, 2, 3; and here the figure was not only used to prove the proposition of Archimedes, but this was done by the use of a subsidiary arc, used in Leibniz' second figure, such as *occurs nowhere in Pascal at all.* Even then I did not pass judgment; I left it to others to judge, and merely stated "further comment is needless." Lastly, this final note was not written until, having finished with the "postscript," and gone on to the translation of the "Historia et Origo," I found therein that the idea that Leibniz, writing many years afterwards, was confusing the two works of Pascal and Barrow (which he was probably studying simultaneously) received more and more corroboration.

I must apologise for spending so much time over this small point; but anyone who, in such a grave matter, on such small grounds as Prof. Russell suggests, would make a decisive judgment, is either a knave or a fool; and I have no conscious intention or perception of being either. Why I apologise is because it is truly a small matter; and the further manuscripts published by Mahnke corroborate this view. They prove that Leibniz, whether he got his ideas from Pascal, or Barrow, or I suggest a good many of them *subconsciously* from Cavalieri (*e.g.* the use of moments), had advanced much further than the manuscripts published by Gerhardt showed; but this advance was chiefly in the direction of many geo-

metrical and algebraical theorems of a particular rather than a general bearing; the idea, however, of differentiation could not have come from Pascal's work, which is all integration. Still less had he any idea of the calculus as a subject of science; any more than Newton had, no matter how much he had done by the use of the infinitesimal unit *o*, the binomial series, and integration of other series term by term. What we want to find is when either of these were able to differentiate a product of two functions of an independent or a dependent variable, when they recognised that differentiation was the inverse of integration, when they could differentiate a logarithm or integrate an exponential, and thus deal with a function of a function. Lastly, what had they been studying, or (as we must be content to see) what had they had the opportunity of studying?

Taking Newton first, we find:

1664. Subsizar of Trinity: sits for Scholarship: comes under the notice of Barrow. Barrow becomes his Tutor, *i.e.* one practically in charge of his education.

1665 and 1666.—Retires to Woolsthorpe, having probably had some instruction from Barrow on the use of infinitesimals: develops fluxions to such a pitch that he uses them for tangents and radii of curvature; can differentiate an explicit function in the form of a polynomial; can also integrate the same, by the use of Wallis's method.

1667–1669.—Back at Cambridge, under the influence of and intimate with Barrow; but chiefly engaged on Optics. Revised Barrow's Optics.

1669.—*De Analysi*; shows that he is still using "first" principles for differentiation and integration.

1670.—*Helps Barrow with the publication of the Lectiones Geometricæ*; advises him to put in the *a*- and *e*-method and to complete his calculus with the propositions which form the Supplements. This shows he has come to understand what the *Lectiones* stand for.

Barrow gives up Mathematics, and takes to Divinity, leaving Newton to carry on his work; probably he has already recognised the master-mind. I consider that Newton did carry on this work, and that he had the analytical, the practically useful, calculus complete within the next two or three years.

For Leibniz, we find:

1672.—Arrives in Paris, having little, if any, knowledge of Geometry.

1673.—First visit to London, January to March 1673; Ms. makes a note under heading of Geometry: "Tangents to all curves" (? ref. to Barrow); also notes extract from Gregory's

letter mentioned above; purchases a copy of Barrow. After his return to Paris, Huygens advised him to read Pascal. I suggest he has already dipped into his copy of Barrow on his way home, for it was his custom to study on his journeys. In October 1673, starts to write Methodus nova investigandi tangentes, etc., which I suggest is due to a second attempt at Barrow.

1674 and 1675.—Has apparently done much work on integration, especially using the idea of "moments" (? obtained from Pascal and Cavalieri); from which he has derived the equivalent of integration by parts (Pascal has many examples of it). In October 1675, finds integral of x^2 and x^3 (Pascal or Cavalieri). October 29, and November 1, more theorems on general quadratures; November 11, he is using figures reminiscent of Barrow for the first time with some success; and from now on the figures are all Barrovian in appearance. This year is the date given by Leibniz for reading Barrow.

1676.—But, even after his second visit to London, and his visit to Hudde, he is only on the threshold of the calculus. In July, he thinks he has solved two problems, and has not enough knowledge to recognise his failure. However, in November, he has the differentiation and integration of powers of the independent variable, and progress is fairly rapid after this.

Here the great point to be noticed is that it is not till after he has really studied Barrow that he does anything except quadratures. It is not until July 1677 that we find the differentiation of a product and a quotient; even then these are only stated, and it is not until about 1680, in an undated manuscript, that we find the proofs. The reading of Barrow has borne its fruit, and, as it would be only a hindrance to Leibniz' pure analysis from this time forward, I suggest it is discarded; thus truly can Leibniz say that he was not indebted to Barrow "for his methods." But, if he had never seen a Barrow, I very much doubt if he (or even Newton) would have invented the analytical calculus, and completed it in their lifetimes.

In conclusion, if I live to see the day when all the manuscripts of Newton and Leibniz can be set in chronological order, so that the reading of both Leibniz and Newton during their finding of the calculus can be set down more or less definitely, I thoroughly believe that my reconstruction of the reading of Leibniz, given at the end of my book, will be found to be correct; and that Newton was indebted to Barrow by personal communication and by the fact of the editing of Barrow's work.

NOTES

A Link with Mendeleeff (G. D.)

On May 8 last, Prof. Bohuslav Brauner celebrated his seventy-fifth birthday, whilst on February 26 last he had been a Doctor of Science of the Charles University of Prague for half a century. To mark these occasions his friends and pupils have contributed to a special issue of the *Collection of Czechoslovak Chemical Communications* a series of interesting papers recording some of the research work in progress in the various laboratories and institutes in Czechoslovakia and elsewhere. In addition, Prof. Brauner himself has contributed a lengthy memoir on his late friend and former Slavonic colleague, D. I. Mendeleeff.

Mendeleeff's chief contributions to science were made in the 'sixties of last century, and as long ago as the 'seventies Brauner was co-operating with him in studying the implications of his Periodic Classification of the Elements and in producing more and more evidence in support of this Law. Much of Prof. Brauner's article on Mendeleeff appeared originally in Czech as an Obituary Notice in the *Pokrokova Revue*, at the time of Mendeleeff's death in 1907, and in view of its importance it has been adapted and rendered into English by Prof. J. Heyrovsky. From it English readers learn many new details concerning the life and many-sided scientific accomplishments of the Russian savant.

Dimitrij Ivanovic Mendeleeff was born on February 8, 1834, at Tobolsk, as the fourteenth child of the head master of a secondary school who died not long after his birth. The view of modern anthropologists is that the last of a large family are frequently distinguished by uncommon talents. This was the case with Mendeleeff. After passing through the Siberian secondary school, he entered the Pedagogic Institute at St. Petersburg at a time when the foundation was being laid for modern chemistry. His thesis for doctorate dealt with the fundamentals of atomic and molecular weights ; it was in Russian and so attracted no attention. Mendeleeff then spent some time abroad at Heidelberg, when Bunsen was at the height of his fame. Brauner, who knew both these scientists, is able to show how it was that they could not

possibly work together. Bunsen was a "classical" investigator, working slowly and exactly, whilst Mendeleeff was a "romantic Slavonic genius," full of ideas and hypotheses.

When Mendeleeff returned in 1860 to Russia he was known for his researches on the behaviour of liquids at high temperatures. He had found that at a definite temperature, liquids lose their cohesion and pass into the gaseous state independently of their volume or pressure. This so-called "absolute boiling-point" anticipated Andrews's identical "critical temperature" by six years. Mendeleeff also worked out lengthy tables for the density of alcohol-water mixtures at different temperatures with an exactness hitherto unknown, and these tables were adopted for excise purposes in all countries. From 1892 until his death Mendeleeff was the director of the St. Petersburg Bureau of Weights and Measures, which he had founded.

His claim to fame, however, rests upon his Periodic Classification. In the 'sixties of last century he was writing his *Principles of Chemistry*, and sought a plan for the book, and this led him to arrange the elements in order of ascending atomic weights. As soon as he observed the periodic repetition of properties by successive rows of elements the scheme revealed itself. Difficulties were, however, encountered. Among the rare earth elements he considered that the atomic weights of several ought to be raised. He himself corrected the atomic weights of some of these elements from specific heat determinations, whilst three of the unknown elements he predicted were subsequently discovered by Lecoq de Boisbaudran (gallium = eka-aluminium, 1875), Nilson (scandium = eka-boron, 1879), and Winckler (germanium = eka-silicon, 1886). His prediction that several other atomic weights should be corrected were not fulfilled, however, in the pairs of elements, Ar—K, Co—Ni, and Te—I. This anomaly was to be accounted for by Moseley some time after Mendeleeff's death.

Prof. Brauner goes on to relate the difficulties encountered by Mendeleeff in endeavouring to publish his paper in Liebig's *Annalen*, in which it eventually appeared in 1871, whereby it was brought to Brauner's notice. In the meanwhile Brauner himself had commenced a research in organic chemistry. He had received from Dr. Antonin Frič, Tutton's *Chemical Box*, in 1863, and in the 'seventies he commenced a series of researches into the chemistry of the rare earth metals which did much to direct attention to Mendeleeff's Periodic Law. He showed, among other things, that the atomic weight of Beryllium was 9, as Mendeleeff contended, whilst the value hitherto accepted was 13·5, a figure which could not be reconciled with the Periodic Table. After Brauner's period, under Sir Henry Roscoe, at Manchester, where he prepared fluorine in the

'eighties by heating lead tetrafluoride, he returned to Prague to continue the researches on the rare earth elements. In 1902 he again went to see Mendeleeff in Russia, two years after they had been together in Prague. Brauner's lecture at St. Petersburg on the Rare Earths was one of the last meetings that Mendeleeff attended.

Mendeleeff's other scientific activities include, according to Prof. Brauner, the metronomic computations recorded in his journal, *Vremennik* ; a book on Meteorology, and his aerostat, in which he flew above the clouds to observe undisturbed an eclipse ; a monograph on "Petroleum"—the theory he advanced was that the carbides of heavy metals are the source of the world's supplies. Navigation and agricultural problems also interested him.

Further Notes on Adder Poison (N. Morrison, D.Sc., F.Z.S.)

Nature has armed the snake with a hypodermic fang and connecting poison gland for the purpose of self-defence and killing its prey. A vast period of evolution involving millions of years has perfected these sinister weapons, and increased and intensified the lethal power of the poison. But are all animals vulnerable to the action of this deadly poison ? The question may seem superfluous, because it is a common belief in scientific circles that all creatures are subject to the destructive qualities of snake venom. Nevertheless, I am in a position to prove that two or three species, at any rate, in the animal kingdom are immune to adder poison.

Some time ago I carried out a series of experiments to ascertain and determine the effect of adder bites on frogs. I experimented on three different frogs with three different adders, and the result in each case was, that the venom had no apparent physical effect on these creatures. There could be no doubt whatever of the fangs penetrating the skin. The reptiles bit the frogs so viciously that the fangs were driven home with such force that the adders had some difficulty in withdrawing them. Therefore, the inoculation was an absolute certainty. I was so astounded at the negative result of my experiment that I began to suspect that the mechanism of the poison apparatus must have been defective or faulty, although this would have been most unlikely to happen in three different specimens.

I decided to submit my experiment to a final test. Accordingly I experimented with one of the adders above referred to on a large brown rat. The adder bit the rat on the upper part of the left leg, and practically simultaneously with the bite the hind-quarters of the rat became paralysed, rendering

the creature quite helpless. It lived for an hour and a quarter after being bitten; the respiration became faulty and spasmodic, gradually slowing down until the poor rodent died of suffocation. The venom of the adder contains a high degree of blood-destroying element, involving the disorganisation of the nerve centres which govern respiration.

I have also carried out a similar experiment with toads, and the result in each case was exactly the same as in the case of the frogs. Moreover, I made microscopic slides of toad's blood mixed with adder venom. A microscopic examination of the slides shows the corpuscles intact—quite normal (see Figs. 1 and 2). I have already mentioned that the poison possesses a blood-destroying or hæmolytic element. Therefore, the corpuscles in the toad's blood, after being subjected to an injection of adder poison, should have been disintegrated and destroyed.

My next experiment was with slow-worms (*Anguis fragilis*). I allowed two slow-worms to be bitten repeatedly by three different adders, which bit these creatures on the back with such force that blood oozed from the punctures made by the fangs. Yet the poison had no physical effect whatever on the health of these reptiles. They lived with me in captivity for weeks afterwards, and they became quite tame. Microscopic examination of the infected blood, stained with hæmatoxylin, showed the nuclei much enlarged—otherwise the corpuscles were intact.

I have also submitted a lizard to the fangs of an adder, or rather made an adder bite a lizard, and the creature was dead twelve minutes after. It is interesting to note that the slow-worm and the lizard (*Lacerta vivipara*) are, zoologically speaking, first cousins; that is, they belong to the same family (*Sub-Order Lacertilia*). Still, the former is immune to the lethal action of adder venom, while the latter succumbs to its deadly potency. What, then, is the explanation of this strange immunity? To give an off-hand explanation, one can only say that the blood of the slow-worm, frog, and toad contains a powerful antitoxin element, which immediately neutralises the action of the adder poison, or it may be partly due to a dermal restraint. I shall refer to this point later on.

A few years ago I read in the press an article by a well-known zoologist, who stated that the pig and hedgehog are immune to adder poison. Of course this is a common belief in Britain. However, I have always been sceptical on this point; consequently, I mixed pig's and hedgehog's blood with adder venom, and microscopical examination of the slides prepared from the mixture revealed in each case that the blood underwent a comparatively rapid hæmolytic and aggluti-

native change. This is, of course, an *in vitro* demonstration of a typical action of the venom. It is possible that *in vivo* results produced by the injection of venom would be of a closely similar nature. Further research work on this point is eminently desirable.

I have also carried out an experiment to determine the effect of adder bite on freshwater eels. I had two adders in captivity at the time—a male and female—the latter a bad-tempered reptile. I submitted an eel (14 inches long) to the fangs of the female adder, which bit it on the back, about the middle of the body.

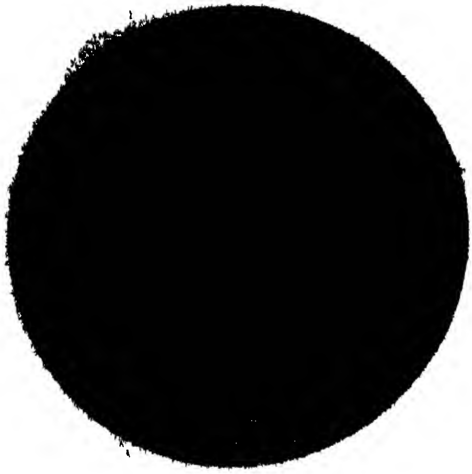
The effect of the poison was apparent shortly after the bite. The eel became less active, and showed general signs of discomfort and weakness. About ten minutes after the attack the body below where it was bitten became paralysed. Then the paralysis slowly crept up the body, till at last the poor eel could only move its head and neck. It lived for five and a half hours after being bitten, which goes to show the extraordinary tenacity of this species to life. As far as I know, this is the first time that the effect of adder poison has been observed on fish. Later on, I shall make further observation on this subject.

The Chemistry of Adder Poison

The poisonous snakes are the *Proteroglypha,* which comprise three families: (1) *Elapinæ, e.g.* the cobra; (2) *Hydrophidæ,* the poisonous sea-snake; and (3) *Viperidæ,* inclusive of all snakes with a fang mechanism for the injection of poison. I am here concerned with a member of the last family—the adder.

Adder venom is ascribed a hæmatogenous origin by some investigators. This description is quite justifiable, but there seems to exist a certain confusion as to how the venom is elaborated. For example, it is said that the poison is simply filtered by the poison-gland from the blood serum. I agree with Arthus in our denial of a simple mechanism of filtration. It is probable that the blood serum contributes its reasonable quota to the toxin-forming elements, but this quota is worked on and changed to a marked degree in the laboratory of the gland. If, as suggested, adder poison reached the gland by a simple process of filtration from the blood serum, then one would expect snake serum to be endowed with highly toxic properties. Such is not the case. I have often worked with adders, withdrawing blood from the creatures, and sometimes accidentally, sometimes purposely, I allowed the blood to come into contact with open wounds on the back of my hand. No symptoms of intoxication supervened, such as would obtain

Fig. 1.



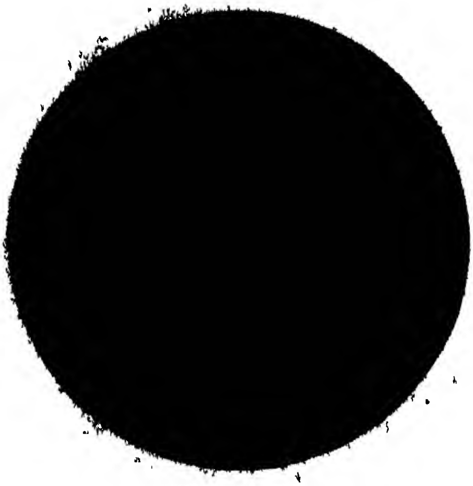


Fig. 2.

1.—Toad's Blood + Venom, showing corpuscles intact after being infected with adder venom.

2.—Frog's Blood (normal).

with a subcutaneous injection or with ordinary contact of pure adder venom. I am therefore forced to the conclusion that adder serum is alone quite harmless, although it may contain compounds which, on reaching the poison glands, are converted by simple chemical changes into highly neurotoxic or hæmotoxic substances.

Adder venom is a slightly viscid, clear, mucinoid fluid with a specific gravity varying from 1·030 to 1·080. It gives an impression of volatility, as it easily evaporates—or rather coagulates—on exposure to air. When injected, it is either weakly acid or neutral in reaction, and contains no bacteria. It contains a high percentage of easily coagulable solids, mostly of a protein nature. The percentage weight of solids is on the average in the neighbourhood of 30. These solids demonstrate their protein nature by their easy precipitability by such chemical substances as ethyl alcohol, ether, and tannin. Two protein fractions have been found. These have been differentiated by their varying thermolability, *i.e.* their susceptibility to coagulation at different temperatures. Prince Lucien Bonaparte in 1843 was among the first to show that there were proteins present in the venom. Later, in 1883, Mitchell and Reichert discovered two different proteins, the first a heat-coagulable substance of the nature of a globulin, and the second a proteose substance. To the former they ascribed the local, irritating effect of the venom, and to the latter its profound systemic intoxication.

Other investigators have discovered other compounds in the transparent fluid. Faust, for example, claims to have isolated nitrogen-free glucosides, *i.e.* compounds containing in their molecule a sugar structure. These glucosides resembled quillajic acid ; in other words, they seemed to belong to the saponins, which are strong hæmolytic substances of vegetable origin. Faust, in describing the hæmolytic action of venom exclusively to his ophiotoxin and allied substances, gives but a partial explanation of the mode of venom action, as he concentrates on the hæmolytic phenomenon and ignores the neurotoxic effects produced.

It is probable that Faust's ophiotoxins of a glucoside nature collaborate with the protein globulin and proteose to produce the effects so markedly evident in the symptomatology of adder poisoning.

The poison gland of the adder is a wonderful director of chemical activity. In addition to the elaboration of proteins and glucosides, it manufactures locally such enzymes as proteases and fibrin ferments, which play an important rôle in blood clotting, and lipases, which aid hæmolysis and fatty degeneration in the wounded areas.

Of inorganic substances Delezenne has found zinc in venom. This element is probably ingested in compound form in the food of the creature, and finds its way to the poison gland by the blood-stream. At its destination, it is in all probability profoundly changed and assimilated into the poison complex, so as to perform certain functions in the activation or otherwise of its associated enzymes.

The various snake venoms, although acting along parallel lines, seem to be quite distinct from each other, although they fall into a well-marked group of zootoxins, including scorpion, spider, centipede, bee, and wasp poisons. Thus, cobra venom contains chiefly neurotoxin, and cobra bite is therefore followed by symptoms largely of a nervous character. In addition, there is little agglutination and great hæmolysis. There is little change at site of puncture.

Rattle-snake poison, on the other hand, owes its effect to the endotheliotoxin called haemorrhagin by Flexner and Noguchi. After injection, there are extensive hæmorrhages, local necrosis, and extravasations of the blood. Rattle-snake and water moccasin venoms show marked differences in agglutinative and hæmolytic powers. I have already referred to the haemolytic action of adder venom. The most poisonous venom of all—that of the sea-snake—acts by vagus stimulation. It also brings about paralysis of respiratory centres and motor-nerve endings. Another dangerous viper, the Russell viper (*Daboia Russellii*), contains no neurotoxin, and achieves its end by intravascular clotting.

Scorpion venom, which is an excretory rather than a secretory product, has much in common with snake venom. Thus, it can withstand heating to 100° C., but is inactivated after 15 minutes' heating. Its active principle is destroyed by such enzymes as pepsin and trypsin. It is harmless when taken into the stomach.

Spider poison, though rarely killing man, is highly dangerous to some animals. Thus, it is interesting to note in passing that the Malmignatte (*Latrodectes tredecimguttatas*) of Southern Russia is responsible for the death of about 70,000 cattle in the year. The percentage fatality from its bite is 12, therefore it must be an aggressive and vicious creature. Its venom, which is distributed through the whole body, resembles that of the snake.

Bee poison, in addition to containing formic acid, has at its disposal a store of protein which is digested by such enzymes as pepsin and trypsin.

I have already mentioned that the eel is not immune to adder venom. This seems strange, as the eel possesses a serum which is itself poisonous, being hæmolytic and neurotoxic.

Eel serum is, however, different from snake serum, which is non-poisonous. Adders are themselves immune from adder bite, and the explanation of the susceptibility of the eel to adder poisoning seems to lie in the fact that, as De Lisle has shown, eel serum does not act like an amboceptor. On heating, and on the addition to it of fresh mammalian serum, it is found that its reactivity has not been regenerated.

The dermal secretion of the toad is poisonous, and some investigators have discovered in the blood a poison with hæmolytic properties. The immunity of the toad to adder venom may be explained on the principle of what I would term a dermal arrest. The snake venom is arrested as soon as a puncture has been made in the dermal region. The dermal poison in all probability makes an attempt to counteract the effect of adder venom, until the blood-stream is reached, when a union of its power with that of the blood poison completes the defence. Frogs have also poisonous dermal secretion. I do not know if their blood is toxic. I rather think that it is not ; and, since frogs and toads are commonly immune to adder poison, it would seem that the poison is arrested and neutralised in the skin of the animal almost immediately after the adder puncture has been made.

Radio Research (S. K. Lewer)

The Report of the Radio Research Board for the period ended March 31, 1929 (price 3*s.* 6*d.* net., H.M. Stationery Office, Adastral House, Kingsway, W.C.2), consists of a general review of the chief investigations undertaken by the Board since its establishment in 1920. This is the first of a series of Annual Reports, the work in past years having been summarised in a section of the Annual Reports of the Department of Scientific and Industrial Research. Increasing attention to the published reports both at home and abroad has called for this separate publication.

The first twenty pages of the Report contain a description of the Board's buildings and sites, and of the general development of activities. On many occasions, Government departments have co-operated in the provision of facilities, buildings, and sites, and the British Broadcasting Corporation has assisted by providing special transmissions required in several investigations. Much of the work has been entrusted to the National Physical Laboratory, with which the closest co-operation has always been maintained. A number of Special Reports have been published through the Stationery Office. An extensive list of abstracts of articles dealing with all aspects of radio communication and kindred subjects is prepared monthly by the Board and published in *Experimental Wireless*.

The section of the Report dealing with the Propagation of Waves is a lengthy account of the study of signal fading on long and short waves, the influence of the Heaviside layer, the determination of the conductivity of the ground, and the measurement of attenuation over towns. In 1925, Prof. E. V. Appleton, a member of the Board, suggested and carried into practice a method for determining the height of the Heaviside layer. This provided the first direct experimental proof of the existence of the layer, and has since been developed sufficiently to permit a complete study of the characteristics of down-coming waves and their effects in causing signal fading. The essential feature of this method is the artificial production of "fringes" at the receiver by continuously changing the wave-length of the transmitter over a small range. Signal variations are produced by interference between the ground ray and the down-coming ray. An improvement has been effected by the use of photographic recording of the variations, permitting the use of more rapid wave-length changes. The variation in the height of the layer for broadcasting waves has been followed through the hours of darkness, the values obtained at various stations showing remarkably close agreement. This study has been extended to include changes in the angle of incidence, intensity, phase, and polarisation of the down-coming waves. Experiments show that the down-coming wave may be described as approximately circularly polarised with a left-handed rotation, possibly due to the influence of the earth's magnetic field. It is suggested that a critical test would be to repeat these experiments in the southern hemisphere. If the effect is due to the earth's magnetism, the polarisation would be right-handed. The correlation between directional errors and fading is also indicated.

The second section deals with Directional Transmission and Reception. This study is of the greatest importance on account of its applicability to marine and aerial navigation. The errors observed in direction-finding may be divided into two types, permanent and variable. The permanent errors have been shown to arise either from causes local to the direction-finder or from geographical conditions between the transmitter and receiver. In addition to these errors, which can be compensated for by calibration, the apparent bearings show erratic variations dependent on the time of day and on the season. In general, it appears that in daylight during the summer the errors are small and fairly constant. The errors increase as sunset approaches, and decrease again at sunrise. During the winter, the day-errors are somewhat greater, and the influence of darkness is more marked. It has

now been satisfactorily established that these variations in apparent bearings are due to the arrival at the receiver of waves from the upper atmosphere, polarised with their electric force in a horizontal plane. Although no marked change in the character of the errors occurs over the whole band of wave-lengths from 300 to 20,000 metres, it was found that an instantaneous change of wave-length of about 1 per cent., such as is sometimes used for signalling, gave rise to a large difference in the errors in bearing for the two wave-lengths. It was also noted that during a solar eclipse, the night variations were temporarily restored. The night variations are not observed within about thirty from the transmitter over land, or about eighty miles over sea. In marine navigation, therefore, there is no serious difficulty in obtaining sufficiently accurate bearings.

The rotating vertical-loop transmitter, or radio-beacon, has been developed to a high degree by the Royal Air Force. To investigate its performances, a beacon was set up by the Board at Gosport in 1926. The beacon consists of a loop aerial made to rotate once per minute, a characteristic signal being emitted when the plane of the loop coincides with the geographical meridian (and another corresponding to the east point to provide for the case of an observer due north or south). It is then a simple matter for an observer equipped with an ordinary receiver and a stop-watch to obtain a bearing by noting the interval between the signals and the minimum or zero intensity. The principle of reversibility may be applied to test the accuracy and reliability of the beacon by using it as a direction-finding receiver and examining the errors. This was carried out at the Gosport station with the co-operation of ships in the Channel. An analysis of the results showed very fair agreement between the bearings obtained by the two methods.

Work is now in progress on the development of short-wave (15–60 metres) direction-finding apparatus, the primary object being to assist in the study of wave propagation. Experiments have indicated that surface irregularities, such as hills, trees, and buildings, may give rise to very large errors in apparent bearings.

One of the greatest advances in the study of atmospherics, which constitutes the third section, is the development of the direct-reading instantaneous direction-finder. Co-operation between the Board's station at Slough and the Post Office radio station at Cupar, where these instruments have been set up, has provided information on the origin of individual atmospherics, and has yielded results of value in designing receiving assemblies in connection with the Post Office transatlantic telephone services. For several years the Board

has included in its programme a study of the wave-form of atmospherics, and the results recently obtained have cleared up the confusion which had previously existed as to the form and interferent properties of the individual atmospheric, and gave a sufficient explanation of the failure of devices designed for the elimination of atmospheric disturbances from a receiving apparatus. It is interesting to note that a very close agreement is obtained between the electrical characteristics computed from the wave-form of a typical atmospheric radiated from visible lightning flashes and the electrical characteristics of flashes measured by Prof. C. T. R. Wilson.

The development of radio-frequency standards and precision measurements of fundamental quantities was entrusted to the National Physical Laboratory. Here, a primary standard of frequency has been developed from the Abraham-Bloch multivibrator. The apparatus enables any frequency within the range of 10 to 1,200 kilocycles to be measured or produced to an accuracy of about 2 parts in 100,000. The frequency is controlled by an electrically driven tuning-fork, and if this is measured against a standard of time, the accuracy can be still further improved to less than 1 part in 100,000. This dependence on the tuning-fork has called for an examination of its capabilities, and a considerable amount of work in this direction has been done independently at the National Physical Laboratory. A number of these standard forks are under construction for various bodies requiring standards in the telephone and radio-frequency ranges, in particular the organisation at Brussels, which controls the wave-lengths of European broadcasting stations, and the fortnightly transmissions of standard calibration frequencies from the National Physical Laboratory, based on the multi-vibrator principle, are regarded as of considerable value both to Government technical establishments and to the radio industry in general. Further work on the standardisation of frequency has involved a study of quartz resonators and oscillators, the importance attached to these being their applicability to stabilising the wave-lengths of transmitters and thereby reducing interference. From time to time various national laboratories have compared frequency standards. The results are given in the Report of the most recent comparisons of the frequencies of three piezo-electric oscillators constructed by the United States Bureau of Standards. They represent probably the most accurate international frequency comparisons yet made, and the agreement obtained shows that a very high standard has been attained in the various national laboratories. Other quantities, such as capacity and inductance, effective resistance, dielectric constants, power factors, have been measured with

great accuracy and convenience by the development of the Schering bridge for general use at radio frequencies.

With a view to placing the theory of antennæ and the design of short-wave beam reflector systems on more solid foundations, the Board carried out theoretical and experimental studies with satisfactory results. The performance of amplifiers was another of the subjects studied at the National Physical Laboratory under the auspices of the Board. A method for accurately determining the amplification factor of a complete amplifier has been developed. The method is applicable to either high or low frequencies, or a combination of both, with a rectifier. Experimental support has been obtained for the theories of amplification generally accepted. The study of interference caused by transmitting stations has been temporarily discontinued, the problem having become less acute. In view of the importance of this subject, however, it is likely that the study will be taken up again in the near future. Work of a preliminary nature has been carried out during the past two years on short waves (less than 10 metres). Investigations of the apparatus and radiating systems suitable for these wave-lengths have been made, in the course of which a valve fatigue effect was discovered. The remainder of the Board's research programme consisted of a number of subsidiary problems associated mainly with thermionics.

The Report cannot fail to impress the reader with the usefulness and importance of this work, which is so largely fundamental in character. Many of the problems, which were at first purely academic, have led to results of great practical service. Future Reports of the Radio Research Board will be welcomed with interest.

Barrow, Newton, and Leibniz (R. R.)

We are very glad to publish in this number an essay by J. M. Child, of the Manchester University, on the subject of the comparative shares of Barrow, Newton, and Leibniz in the immortal discovery of the Calculus. It appears from the careful analysis of Barrow's work which Mr. Child gave us in his book of 1916, entitled, *The Geometrical Lectures of Isaac Barrow* (Open Court Publishing Co.), that the Leucasian Professor, Barrow, at Oxford had really discovered the principles of the Calculus in geometric form before his pupil, Isaac Newton, put those principles into algebraic form some years before Leibniz invented the special notation which is now given to the Calculus throughout the civilised world. Perhaps the discovery of the Calculus may be looked upon as the greatest scientific discovery ever made ; and it is therefore right

that readers of scientific work should know exactly to whom they are indebted. Little doubt regarding this matter can remain after Mr. Child's analysis; and it is surprising that work like that of Barrow has received so little attention. Readers might like to refer also to the two following works by Mr. Child: *The Early Mathematical Manuscripts of Leibniz* (Open Court Publishing Co., 1920), and "Newton and the Art of Discovery," an article which appeared in the *Newton Memorial Volume* (Bell & Sons, 1927).

Notes and News

The honours list, published on the occasion of the King's birthday, included the following awards to workers in science: *Knights:* Prof. A. S. Eddington; Prof. Leonard E. Hill; Dr. G. A. K. Marshall, director of the Imperial Bureau of Entomology; Prof. J. Arthur Thomson, of Aberdeen; and Mr. H. W. A. Watson, lately Chief Conservator of Forests, Burma. *C.B.E.:* Dr. E. W. Smith, honorary technical adviser to the Area Gas Supply Committee, Board of Trade. *O.B.E.:* Prof. W. M. Roberts, professor of mathematics, Royal Military Academy, Woolwich; Dr. F. B. Young, Principal Scientific Officer, Admiralty Research Laboratory.

The President and Council of the Royal Society have recommended Mr. Ramsay MacDonald and General J. C. Smuts for election into the Society under the statute which permits the admission into the Society of persons who have rendered conspicuous service to the cause of science, or whose election is likely to be of signal benefit to the Society.

We have noted with regret the announcement of the death of the following well-known men of science during the past quarter: Dr. F. R. Blaxall, bacteriologist; Mr. G. H. Curtiss, of flying-boat fame; Mr. Hugh Elliot, philosopher; Mr. W. J. Greenstreet, editor of the *Mathematical Gazette;* Prof. A. Gullstrand, of Uppsala; Mr. A. S. Hirst, zoologist; Dr. E. B. Knobel, astronomer; Dr. C. Ladd-Franklin, originator of the theory of colour vision bearing her name; J. A. Le Bel, chemist; Dr. F. Nansen; Prof. K. J. P. Orton, chemist; Mr. E. A. Sperry, inventor of the Sperry gyro-compass; Prof. G. N. Stewart, physiologist of Cleveland, U.S.A.; Mr. A. F. R. Wollaston, naturalist.

Sir Arthur S. Eddington, Sir William B. Hardy, Sir Arthur Keith, Prof. J. E. Marr, Prof. R. Robinson, and Dr. D. H. Scott have been elected Honorary Fellows of the Royal Society of Edinburgh.

Prof. Henry E. Armstrong has been awarded the Albert medal of the Royal Society of Arts for his discoveries in chemistry and his services to education.

Baron G. J. de Geer,of Stockholm, and Prof. T. Levi-Civitia, of Rome, have been elected foreign members of the Royal Society.

Mr. C. C. Paterson, director of the Research Laboratories of the General Electric Co., has been nominated President of the Institution of Electrical Engineers.

Dr. W. H. Eccles has been re-elected President, and Dr. R. S. Clay Vice-President, of the Institute of Physics.

Dr. Ludwig Prandtl, of Göttingen, is the second recipient of the Daniel Guggenheim medal, awarded by the American Society of Mechanical Engineers for work in aerodynamics.

The Bruce prize of the Royal Society of Edinburgh for the period 1928–30 has been awarded to Mr. N. A. Mackintosh for researches into the biology of whales.

Sir Henry G. Lyons has been reappointed Director of the Science Museum until October 1933.

Brigadier H. St. J. L. Winterbotham has been appointed to be Director-General of the Ordnance Survey in succession to Brigadier E. M. Jack.

The first award of the Frederick G. Donnan Fellowship in chemistry, tenable for three years at Johns Hopkins University, has been made to Mr. A. Lewis, of King's College, London.

A Senior Beit Memorial Fellowship has been awarded to Mr. R. J. Lythgoe, and Fourth Year Fellowships to Mr. P. Eggleton and Dr. F. R. Winton, all working in the Medical Faculty at University College, London.

The Ramsay Memorial Fellowships Trustees have awarded new Fellowships for the session 1930–31 to Mr. W. R. Angus, M.A., B.Sc., Mr. K. Krishnamurti, M.A., D.Sc., and Mr. J. Bell, B.Sc., Ph.D., who will work at University College, London, and to Dr. A. Girardet, who will work at the University of Edinburgh.

The Council of the Institute of Marine Engineers desires to draw the attention of apprentices and students intending to become Marine Engineers to the educational advantages afforded by association with the Institute. The annual examination held by the Institute for student graduateship and the accompanying diploma will take place on April 13–16, 1931, at centres arranged to suit the candidates. The superintendent engineers of the leading shipping companies recognise that possession of this diploma is proof of all-round ability in the technical subjects specially required as the foundation of a successful career as a marine engineer. The Lloyds Register Scholarship, value £100 per annum, and tenable for three years at an approved university, will be awarded next year on the results of an examination to be held on May 11–12. Full

particulars may be obtained from the Secretary, Institute of Marine Engineers, 85 The Minories, London, E.C.3.

The Association of Special Libraries and Information Bureaux is forming a panel of persons able and willing to translate technical articles from foreign languages into English. The Association (26 Bedford Square, London, W.C.1) invites communications from those desirous of inclusion on the panel or wishing to use the specialised service it provides. Translators must be thoroughly acquainted with the languages they offer and with the subjects with which they are prepared to deal.

The Department of Overseas Trade announces that the British Industries Fair, 1931, will open in London and Birmingham as usual on the third Monday in February, and will close on Friday, February 27. By the addition of the two floors of the Empire Hall, Olympia, which were not ready for occupation last February, the exhibiting area is increased by 50,000 sq. ft. to a total of 300,000 sq. ft. The hours of opening will be 9.30 a.m. to 7.30 p.m., and the public will be admitted only from 4.30 p.m. to 7.30 p.m. except on Saturday, February 21, when the hours will be 1 p.m. to 7.30 p.m.

Popular Research Narratives (Vol. II, Baillière, Tindall & Cox, London, price 4*s.* 6*d.* net) is a collection of fifty interesting but rather trivial stories of research, invention, or discovery, written by the discoverers, and collected by the Engineering Foundation, New York. Each story occupies two or three pages of print, and the ground covered traverses a very wide range of physics and engineering. J. A. Greig describes the remarkable efficiency of an aerial composed of copper braid (10 strands bare copper wire, 18 S.W.G., $\frac{1}{2}$ in. × $\frac{1}{8}$ in.), originally made for connecting lightning-rods to ground. He states: "With an antenna 30 feet long of this type, strung in a basement at a level of about one foot below the surface of the earth, better results were obtained than with an antenna of the usual type, of 100-foot length, strung between two poles out-of-doors at an elevation of about 40 feet." These experiments were first described in 1922, and it is rather surprising that antenna of this type have not come into more general use.

J. Breuchaud gives an account of the Beccari zymothermic cell for the conversion of sewage, kitchen waste, leaves, grass clippings, etc., into manure. Since each cell consists merely of a cubical masonry box of from 1 to 25 cubic yards capacity, with certain subdivisions and air channels, and costs nothing for upkeep, it is again surprising that the process is so little known. W. G. Houskeeper, of the American Telephone and Telegraph Co., describes the materials and methods used in sealing base metals to glass, but without any mention of the

Guillaume alloy platinite used in the ordinary electric lamp. H. B. Smith describes the attempts now being made to devise satisfactory insulators for high-voltage overhead power transmission (110,000–330,000 volts). The type at present preferred consists of a spindle of impregnated wood about 20 inches long, topped with a hood of sheet metal resembling an inverted hand basin with a wide brim, about 45 inches diameter, and terminated by a torus (or "doughnut-shaped ring") of metal tube about 17 inches diameter. The desiderata are of course the prevention of arcing and coronal discharges, and the reduction of cost, weight, and length. Narrative No. 98 describes "How Food got into Tin Cans and Glass Jars." Success in this mode of food preservation was, we are told, first achieved by Nicolas Appert, who competed for a prize offered by Napoleon in 1795, "for a process that would keep perishable foods fresh, chiefly for use at sea." We are also told, however, that *c.* 1850 another member of the Appert family invented the "autoclave" for boiling water under pressure, and that later an American, Isaac Solomon, "found that the temperature of boiling water could be raised by adding calcium chloride." These statements seem to require qualification!

The first University of Oklahoma Study, entitled *The Terminology of Physical Science* (University of Oklahoma Press, price $1), and written by Duane Roller, assistant professor of Physics in that University, is a useful and provocative monograph which deserves a wide circulation among teachers and students of Physics. It is divided into six chapters, of which the most important is that devoted to a critical account of the definitions of the quantities commonly dealt with in Physics. Here and there his statements are not above reproach. He states, for example, that *therm* is used as a synonym for *calory* without any reference to the modern use of the former unit in this country, and that "centre of mass, centre of inertia and centroid are synonymous," a remark hardly to be expected in a critical survey. He defines the ice point as the temperature at which water freezes instead of that at which ice melts, and refers to Boyle as an "Irish scientist," which is absurd. He allows the terms anode and cathode to be used for the poles of primary and secondary cells, as well as for electrolytic cells (voltameters), whereas they should be used only as names for the electrodes in electrolytic cells. The use of the term *latent* in connection with the heat of fusion and heat of vaporisation is doubtless unnecessary and even undesirable, but it is certainly not obsolete. Concerning the term *specific*, the author merely relates without comment the two senses in which the term is used. This double usage leads to much confusion, and in the opinion of the writer of this note

it might well be used only "to indicate a comparison of the same properties of two substances, one of which is chosen as a standard," as in specific gravity and in the old-fashioned definition of specific heat. In writing of specific resistance (resistivity) no warning is given concerning the common but pernicious statement that this quantity is measured in ohms per cm. cube, from which it is an easy descent to ohms/cm.^{3} or ohm—cm.^{3}, and all the numerical errors which follow in their wake. We have referred to only a few points in Mr. Roller's essay, with most of which we are in complete agreement. The University of Oklahoma has performed a distinct service to Physics in publishing the monograph.

Circular No. 382 of the Bureau of Standards, Washington, deals with Bismuth in all its aspects and in particular with *all* its alloys. The following table, extracted from the *Circular*, which gives the composition of a graduated series of alloys suitable for constant temperature baths, may be of service to some of our readers.

Melting-point ° C.	Parts by Weight.				Melting-point ° C.	Parts by Weight.			
	Bi.	Pb.	Sn.	Cd.		Bi	Pb	Sn.	Cd.
70	5	4	2	2	122	8	8	8	—
75	8	8	3	10	130	8	10	8	—
77	8	6	1 5	1	143	8	16	14	—
82	7	4	4	1	154	8	24	24	—
95	2	—	3	1	160	8	26	24	—
100	8	6	3	—	165	8	28	24	—
108	8	8	3	—	171	8	30	24	—
118	8	8	6	—	177	8	32	24	—

Alloys of still lower melting-point may be obtained by adding mercury, *e.g.* by mixing 53·5 per cent. Bi with 17 per cent. Pb, 19 per cent. Sn, and 10·5 per cent. Hg, a fusible metal melting at 60° C. is obtained. Strangely enough, mixed with the alkali metals bismuth yields alloys with melting-points far above those of either constituent. The compound Na_3Bi melts at 790° C., while Bi_3Ce_4 melts at 1630° C., *i.e.* considerably above the normal boiling-point of pure bismuth (1,470±30° C.). The thermoelectric properties of the metal are very marked, and by coupling pure bismuth with a bismuth alloy containing 5–6 per cent. tin an e.m.f. of 120 microvolts per degree C. can be obtained. Unfortunately, bismuth wire is brittle and very hard to prepare. Information concerning its preparation will be found in the *Jour. Opt. Soc. Am.*, **14**, p. 445, 1927 (extrusion), and *Phys. Rev.*, **23**, p. 665, 1924 (drawing).

In passing it may be noted that the Bureau of Standards is introducing new and strange units. Thus in the paper on Bismuth just discussed thermal conductivity is given as calories per second per *cubic* centimetre per degree C., and viscosity as "poises (*dynes per cm.*^{2})"! In *Miscellaneous Publication No.* 108, the unit of specific resistance is given as ohms/cm.^{3} on p. 1, and ohm—cm.^{3} on p. 3, neither of which can be accepted as a substitute for the orthodox form.

Miscellaneous Publication No. 107 of the Bureau of Standards deals with the performance of the brakes of self-propelled vehicles. For all vehicles weighing 6,000 lb. gross or less, and all vehicles of whatever weight used for passenger transport, the following recommendation is made: on a dry, hard, level road free from loose material, and with the clutch disengaged, the foot brake shall be capable of stopping the vehicle from a speed of 20 m.p.h. within a distance of 50 ft., while under the same conditions the hand brake shall stop the vehicle within a distance of 75 ft. Stopping from 20 m.p.h. in 50 ft. implies an acceleration = − 8·6 ft. per sec. per sec. It may be added that four-wheel brakes of average performance will stop a car travelling at 20 m.p.h. within 24 ft., and two-wheel brakes within 50 ft., the theoretical maxima being 16·5 ft. and 30 ft. respectively.

In the *Indian Journal of Physics* (Vol. XIII, Part VII, June 1930), Messrs. Teegan and Rendall give a brief account of the results of some experiments to determine the average intensity of sunlight in Rangoon. They employed a photoelectric cell coupled to a one-valve amplifier, arranged so that the anode current from the valve passed through a copper voltameter provided with platinum electrodes. The average intensity of illumination of the photoelectric cell was estimated from the mass of copper removed from the copper sulphate solution in the voltameter in a known time. They found that the average illumination between ten o'clock and four o'clock on a sunny day in November was 325,000 metre candles, while on a dull day it fell to 60,000 metre candles. For a bright summer day in England the average illumination over a period of twelve hours is about 50,000 metre candles, with a maximum of 150,000 metre candles.

In the April number of the same journal, L. D. Mahajan describes his experimental investigation of the vibration of a pianoforte sound-board. The method devised by Sir C. V. Ramon in 1919 for obtaining vibration curves of the sound-board was used. Mr. Mahajan describes it in some detail, and gives photographs of twenty-one curves illustrating the mode of vibration of the board when strings of frequencies ranging from 27 to 646 vibrations per second are struck. The curves

have been analysed, and the relative amplitudes of all the harmonics up to the twelfth determined. When the lower notes are struck the vibrations of the sound-board are complex and the analysis shows weak fundamentals and strong overtones (*e.g.* $n = 30$, amplitude first harmonic 0·29, fourth 2·2, twelfth 0·43). The vibrations become simpler as the frequency of the strings rises, until for $n = 646$ the curve becomes nearly simple harmonic and the amplitude insignificant. The results are explained by the fact that the natural frequency of the sound-board is *c.* 100 vibrations per second.

CORRESPONDENCE

To the Editor of SCIENCE PROGRESS

PARTHENOGENESIS

From Prof. ROBERT K. NABOURS, Ph.D.

DEAR SIR,—In the July number of SCIENCE PROGRESS, p. 36, it is stated that parthenogenesis had been noted, hitherto, only in the Hymenoptera, Coleoptera, Hemiptera, and Diptera.

I am writing to call attention to parthenogenesis in a group of the Orthoptera which was first, I believe, reported by me in 1919. I have, up-to-date, had several thousand individuals of the species *Paratettix texanus*, *Apotettix eurycephalus*, *Telmatettix aztecus*, and *Tettigidea parvapennis pennata*, of the sub-family Tettigidae, hatch from unfertilised eggs.

The bibliography is as follows :

NABOURS, ROBERT K. :

1919. Parthenogenesis and Crossing Over in the Grouse Locust *Apotettix*. *Am. Nat.*, **53**, pp. 131–42.

NABOURS, ROBERT K. :

1925. Studies of Inheritance and Evolution in Orthoptera. *Kansas Agric. Sta. Bull.* 17, pp. 1–231.

NABOURS, ROBERT K., and BERTHA SNYDER :

1928. Parthenogenesis and Inheritance of Color Patterns in the Grouse Locust *Telmatettix aztecus*. *Genetics*, **13**, pp. 126–32.

NABOURS, ROBERT K., and MARTHA FOSTER :

1929. Parthenogenesis and the Inheritance of Color Patterns in the Grouse Locust *P. texanus*. *Biol. Bull.*, vol. **56**.

NABOURS, ROBERT K. :

1929. The Genetics of the *Tettigidae* (Grouse Locusts). *Bibliographia Genetica*, vol. **5**, pp. 27–104.

Since these publications appeared in Genetical Journals, I can see very well how your editor overlooked these facts.

Very truly yours,
ROBERT K. NABOURS.

KANSAS AGRICULTURAL COLLEGE,
MANHATTAN, KANSAS.
July 17, 1930.

(I thank Prof. Nabours for pointing out my slip. Naturally his work is well known, and the omission of Orthoptera was of course an oversight.—H. F. B.)

ESSAY-REVIEWS

ON THE SURFACE OF THINGS. By ALFRED W. PORTER, D.Sc., F.R.S., F.Inst.P., Hon. F.Inst.Rad., Emeritus Professor of Physics in the University of London. Being a review of **The Physics and Chemistry of Surfaces**, by NEIL KENSINGTON ADAM, M.A., Sc.D. [Pp. x + 332.] (Oxford: at the Clarendon Press, 1930. Price 17*s.* 6*d.*)

ALTHOUGH on Etruscan vases boys are depicted blowing bubbles, Leonardi da Vinci is the first to have a claim to the discovery of capillary phenomena, and it is not till Hauksbee (1709) that the first accurate measurements were made upon them. Newton (who was acquainted with Hauksbee's work) had clear ideas as to the existence of strong forces of short range holding the parts of bodies together, and Laplace showed how these forces must give rise to special effects at the surface of bodies, and thus a physical explanation was given of surface tension, the conception of which had been introduced by Segner. All this work was done without introducing the conception of molecules, the particles of Laplace being simply minute volumes or elements in a continuous material—elements, in fact, to which the theorems of the infinitesimal calculus would apply.

This surface tension is exhibited as a force, parallel to the surface, acting so as to reduce the surface and actually reducing it unless opposed by external tangential forces of other origin applied to the boundary. When thermodynamics came to be developed, it was easily seen that work would be done during any contraction of surface against such external forces and that the surface must be a seat of energy. The value of the surface tension is found to be independent of the extent of surface, and hence it can be regarded either as the force per unit length in equilibrium with external forces or as the free energy per unit area. The two are simply equivalent expressions, each implying the other and not mutually destructive.

Dr. Adam, however, has an obsession against the idea of surface tension, considering it at best as being only a *mathematical* equivalent of the free surface energy. This idea is especially curious emanating from Dr. Adam, who has probably done more than any other man in actually measuring the external tangential force necessary to expand a liquid surface.

In the equilibrium state there must, of course, be an equal opposite force in the film to maintain the equilibrium.

If his attack were based upon the conceptual nature of all forces there would be nothing more to be said; excepting that it would be a needless introduction of purely ontological questions into a problem of physical measurement. This, however, is not the case. Dr. Adam is quite willing to admit the existence of forces *normal* to the surface (the inward attractions); hence his antipathy must be taken as being directed specially against forces of a tangential character. It is better, however, to adhere to ordinary mechanics wherever we can. The free energy can never diminish at constant temperature, except by work being done at the moving boundary against an external force having a component opposed to the direction of motion. [The equation for its change dF is $dF = -\phi dT + Xdx$, where ϕ = entropy, T = absolute temperature, and X is measured positive in the direction of x. The second term is zero when the temperature is constant.] It is surely not possible that he can think of this boundary force, X, without a force in the film to balance it when equilibrium exists.

To prevent misconception let it be said that the free energy is equally real and plays a very important part in general theory. Physical writers have been very slow to adopt some of the general principles of thermodynamics. Surface tension has often been considered as the counterpart of surface energy—no distinction having been made between energy and free energy, in spite of the fact that one of the first problems which Kelvin worked out, after he had finally adopted *both* laws of thermodynamics, was to show that for a film to extend at constant temperature heat must be allowed to flow in and the energy increases in a twofold way, and not alone by work done against surface tension.

It is, of course, not necessary to think of a palpable skin on the surface, like the skin of boiled milk, any more than we need think of actual strings—holding the molecules together—to account for internal pressure. If we could magnify the neighbourhood of the surface a hundred-million-fold and view it from outside, we would see molecules moving to and fro, stopping and reversing, and molecules moving sideways or obliquely; and all the time, if we could make a sufficiently detailed examination, molecules approaching one another would, on the average, tend to accelerate relatively, and those receding would tend to slow down relatively, indicating attractions between them. We would thus find that there were not only attractions normal to the surface, but also tangential ones for molecules lying parallel to the surface, and the totality of the tangential components for the surface region is the force

whose value per unit length is the surface tension. One-thousandth of a centimetre or more down beneath the surface the force parallel to the surface corresponds to the Laplace pressure K, *i.e.* for water, about 10,000 atmospheres. For a layer of the thickness 10^{-8} cms. [one molecular diameter] this corresponds to a force of 100 dynes/cm. The actual value of the surface tension for water is 75 dynes cm. This calculation is extremely coarse, but it is sufficient to prove that there is no difficulty whatever in Segner's assumption that the surface "film" is the seat of a tangential force—*viz.* the surface tension required to account for the "surface free-energy."

We must pass on to other questions. The volume under review is not confined to the consideration of the notions of elementary mechanics; it brings together for the first time a host of recently discovered experimental facts for the explanation of which the molecular theory of matter, in both its physical and chemical aspects, is essential.

It is with the properties of thin films on liquids that the new phenomena are concerned. "Surface films were known to the ancients mainly because of their power of protecting ships in a rough sea by calming surface waves. A little oil will calm a large area of rough water." But few facts were known concerning them. It was known, for example, that a very little oil is needed to stop the erratic motion of floating camphor on water: a finger (in its ordinary state) dipped into the water is sufficient to produce a great effect. The contamination can be removed by "scraping" the surface of the water with a strip of clean paper. The late Lord Rayleigh was the first to make definite measurements of the amount of oil required to check the motion. He found that it only requires 0·81 milligramme of olive oil spread over an area of 555 sq. cms.. the thickness of the film being about 16×10^{-8} cms. Oils in thick layers have much smaller surface tension than water—much less than a half; this thickness of film lowers the tension by about 16 dynes/cm. It was also known that contamination of a surface alters the reflection of light from it; this is detectable by the ellipticity of the light reflected near the polarising angle. This is practically all that was known in 1891, when Miss Pockels proved that films can be pushed about by moving barriers; they can be compressed, the surface tension falling as compression proceeds, slowly at first, but very rapidly after a certain critical compression is reached. In 1899 Rayleigh made the important suggestion that at the critical compression the layer is one molecule thick, the molecules of oil touching each other over the whole surface. Much depends upon what we mean by "touching." Rayleigh himself was very guarded in his statement—"if they behave

like the smooth spheres of gaseous theory no forces will be called into play until they are closely packed . . . if we accept this view the first drop in tension corresponds to a complete layer one molecule thick and the diameter of a molecule [deduced from his observations] is about 1 $\mu\mu$. An essentially different result would seem to require a repulsive force between the molecules resisting concentration long before the first layer is complete."

These were only the first beginnings and necessarily present an imperfect picture of the phenomena. The development of the subject is due to a very large extent to two observers and their co-workers—to I. Langmuir in America, and to N. K. Adam himself in England. They are developments with which any man must be proud to be associated. Briefly, " by pushing the film about " by means of a floating barrier, the force acting on which could be delicately measured, the alteration of the surface tension on the film side is determined. Observations are thereby made in two dimensions analogous to pressure-volume-temperature observations during the compression of a gas ; and perhaps we should not be surprised that they turn out, in some respects, to be even more complicated than those of a simple condensing gas. Rayleigh, in his pioneer work, does not appear to have contemplated the important part which thermal motions would play : even now the surface picture is probably incomplete and some parts of it may need to be brushed out and repainted. Starting with a very sparse layer of molecules and diminishing its area, a diagram analogous to Andrews's diagram for carbon dioxide can be obtained. In the first period the sparse molecules behave as a gas ; ultimately they become a compact mass which (at any rate as a rule) is one molecule thick. Measurements, when compared with measurements of similar molecules made by means of X-rays, indicate that in this close-packed state the molecules are all standing up on end as though attached at their feet by the strong attraction of the sub-lying liquid. In an intermediate range the behaviour is to some extent analogous to that of the heterogeneous region—mixed gas and liquid. The resemblance is not complete ; the isothermal pressure in this region is not constant as for ordinary vapours. The reviewer has compared this region to that for the condensation of mixed gases ; and it may be that we have here an indication that the molecules of liquid on which the film is lying play an important part. It is difficult to conceive these molecules as absolutely inactive, but the mechanics of the problem have not yet been worked out. We can picture molecules of the film swaying about (with thermal motion) like a field of corn in the wind on a gusty day. The measurements actually made will there-

fore only give average values and not the individual values for any part of the film.

For the details in regard to these measurements recourse must be made to the book itself. The subject is a fascinating one, and it all looks very easy when described by Dr. Adam; but probably no one knows better than he the difficulties that have had to be overcome in developing it.

Films such as those of olive oil are films of insoluble substances. A different class of film is found when we have a mixture of mutually soluble substances. These were first described thermodynamically by J. Willard Gibbs, of Yale University, at New Haven in America.

A third kind consists of those for which there is nothing but film, as in a soap bubble. These present many exceedingly interesting problems to which inquiring Etruscan boys must have received very unsatisfying answers. They have been studied chiefly by Plateau (with the help of his co-worker van der Mensbrugghe, for he himself had become blind), by C. V. Boys, and for some years before his death by James Dewar at the Royal Institution. Chapter V deals with these.

In connection with these problems the question of the thickness of the film is very important. There is much evidence that even in the closely packed film it is in the main only one molecule thick, although further compression may cause it to buckle up, in which case molecules in it will lie one above another but perhaps not so as to form a consistent film. This unimolecular character is quite in accordance with what we know about molecular forces. The evidence is that these forces vary with distance according to a much higher power than those of gravity—even according to a seventh or eighth power. The pull of water on a second layer of oil is probably less than 5 per cent. of its pull on a single layer: any such second layer, if it exists, is therefore held mainly by cohesion with the first.

The process of the *formation* of a film is not always simple, and the mechanism still requires elucidation. A drop of oil "placed on water spreads first as a film of visible thickness, sometimes showing interference colours, but often too thick for this. As soon as the whole surface is covered, however, this thick film breaks up, and after passing through a variety of stages, settles down as a mono-molecular film in equilibrium with one drop [compare the lenses of grease floating on soup] if sufficient time be allowed. Volatile substances spread in a similar manner, but equilibrium is never reached, since the drops, and particularly the film, are evaporating rapidly, and there is constant motion from any unspread

liquid to make up losses from the film." Here are many problems to be solved. Osborne Reynolds described an elucidatory experiment. If dust is first sprinkled over the water surface, the dust does not move until the advancing edge of the oil reaches it, and then the dust is heaped up as a rib being swept up by the advancing oil. He wrote (as quoted by Dr. Adam): "The result is to give the impression that the dust is being driven back by the oil, as if the oil were spreading by some inherent force . . . but as a matter of fact the oil is being drawn forward by the contraction of the dust-covered surface of the pure water."

Of course Dr. Adam will have none of this explanation, "since this contractile skin is now known to be mythical," and substitutes instead: "The motions of the molecules of water cause the expanding movement of the oil drop. The molecules of water are in constant motion parallel to the surface, diffusing long distances. The oil molecules adhere to them and are carried outwards along the surface by reason of these surface-diffusing motions."

We observe only that we would have expected Dr. Adam to have recourse to the expansive motions of the oil (as Reynolds suggested to be plausible) rather than to depredations by water molecules.

The problem is a difficult one and the actual process in particular is complicated. In the process itself we are, of course, not dealing with equilibrium conditions. The problem of rushing jets of steam presents similar difficulties—it is even a moot point whether or not it is possible to give a precise meaning to such a common quantity as "pressure" during the motion. Nevertheless, equilibrium notions in regard to surface tension are quite applicable here, if we deal with instants at which change has temporarily ceased. At the edge of the drop three surfaces meet: oil-water, oil-air, water-air. The surface tension of the water-air surface is greater than the sum of the two others, hence equilibrium cannot exist and outward motion begins. (This is on the assumption that we are dealing with a spreading liquid.) The first action is surprisingly quick and apparently overshoots the mark, since thick temporary films thus form in many cases which subsequently rupture and fresh lenses are formed. In order that these may be in equilibrium, the surface tension of the film must be less than that of pure water to such an extent that the three tensions can now be in equilibrium. So much is clear; but much variety is possible in the details of the actual process, and in the particular shares for which thermal motions and condensation of vapour are responsible.

The consideration of spreading leads on to lubrication.

In "incomplete" lubrication the film is very thin—even a single layer of molecules produces an effect; in "complete" lubrication (usually known as "film lubrication") the solid parts do not come into contact at all. The problem of solid friction still presents difficulties—even the fundamental law of simple proportionality to the normal force (for moderate loads) is as yet unexplained. Probably a number of the particles of the two bodies approach so near that the Laplace attraction holds them together—the total normal force is the sum of these attractions. Friction measures the tangential forces required to tear them apart. Both forces are therefore proportional to the number of contacts. Notions based on local depressions (such as the spherical depressions made in Brinell tests) do not give Coulomb's law.

The wide range of surface actions is illustrated by the chapter on adsorption and catalysis. These subjects are much too wide to be treated satisfactorily in a single chapter. It would have been better to have given them a separate volume, especially as they lie rather outside the work for which Dr. Adam is himself largely responsible.

The last chapter gives a very brief summary of methods of measuring surface tension. So little room has been left for them that it has not been possible to give more than a scanty outline without the practical details that are so necessary for successful measurement. Workers will welcome the reproduction of some of Sugden's tables for reducing observations, even though these tables have been compiled in a very inconvenient form. It is not convenient to treat the unknown quantity which is being measured as the independent variable. This results in a rough value of it being needed in the first place, and improved values must then be obtained by successive approximations by means of the tables. The tables give the right results, but a very much better way of tabulating could have been chosen. The quantities known from experiment are usually a height (or pressure) and a radius, and these should be treated as independent variables. The reviewer has given a short table for the case of capillary ascension in the June number of the *Journal of Scientific Instruments.*

In conclusion, the volume is a mine of experimental results, a very large number of which have been discovered in very recent times. In particular, in the field in which Dr. Adam has himself worked so successfully an entirely new domain has been opened up. It is not an exploration of ancient ruins that has been undertaken, but the unveiling of a marvellous architecture which is continually taking form in some of the phenomena of everyday life.

BEYOND THE RED. By F. I. G. Rawlins, M.Sc., F.Inst.P. Being a review of **Das ultrarote Spektrum**, by Prof. Dr. Cl. Schaefer, and Dr. F. Matossi. [Pp. vi+400.] (Berlin: Verlag von Julius Springer. Price 28 marks.)

The gradual unfolding of portions of the electromagnetic spectrum has at all times been one of the most interesting features of man's advancing knowledge in physics. Nearly always the process has been fitful: decades will pass without a new region of frequencies being laid bare, although slowly but surely information about the characteristics of the missing portion is being accumulated until finally it is brought to light. There are, however, two distinct cases to be considered, one in which the unrecognised wave-lengths are certainly being emitted under some set of conditions, perhaps not very obvious, and which only await the inspired day of the discoverer, and another in which (as has occurred recently) the problem is to find the conditions of excitation and to realise them experimentally. In other words, generation of the particular frequency or frequencies desired does not take place under normal circumstances at all, so that a very abnormal state of affairs has to be brought about in order to attain the end in view. The distinction is more a matter of degree than of kind, for whereas intermittent sources may present themselves comparatively readily, continuous emitters are all too rare. Constancy of flux is generally essential.

In the year 1800 Herschel was experimenting upon the visible spectrum, and was led to enquire what happened beyond the red end, that is, towards longer wave-lengths. Trial with a thermometer showed the existence of invisible heat radiations, and to these was given the name infra-red (German, *ultrarot*), a term upon which it would be difficult to improve. Naturally, early efforts did not establish the complete identity between these new rays and visible light as contiguous portions of the electromagnetic spectrum, but it was not long before this was demonstrated beyond all manner of doubt. Later work showed that all the basic "optical" effects (using the word "optical" in a broad sense) such as reflection, refraction, dispertion and polarisation, were to be found in the infra-red.

Experimental difficulties have always been severe, in fact they have been at the root of the neglect from which work in this field has suffered from Herschel's day to this. Objective proof of the reality of these heat radiations was greatly helped by Abney's remarkable success in long wave-length photography. His range was very limited, but his achievement in producing an image of a kettle boiling in a dark room has become a classic. It is only in very recent times that photography in the infra-red has been taken up again and a small extension of range captured

at enormous labour. There will always be an incentive to efforts of this kind because a method of recording which does not involve direct human agency—such as enters with galvanometers—is most desirable both for consistency's sake and for economy of time. When the Raman effect was first announced (1928) it was natural to infer that much that had hitherto been gained by experiment in the infra-red could be learnt far more quickly and easily from work in the visible and subsequent translation into the appropriate infra-red frequencies. Additional experience has gone far to refute this surmise.

Meanwhile, as practical difficulties were gradually being overcome, theoretical physicists were at work upon the nature of the partition of energy in the spectrum as a function of temperature and frequency. It is enough to say that this has always been one of the majestic problems of physics to which master minds of several generations have given of their best. Very simple indeed it looks. What is the most general form of the function (λT), where λ is wave-length and T is temperature ? Nothing but a breach with classical radiation theory would suffice to solve this grand riddle. It is common knowledge that the joy of " getting there " fell to one of the greatest of theoretical physicists on the Continent, but satisfaction comes from the recollection that this year has seen him accorded the highest honour which that British University can bestow in which his fellow-seeker after the form of (λT) was once Chancellor. It is a charming incident, for which this function must be held responsible. Spectroscopists of all breeds are perhaps a little too eager to claim the quantum theory as their own particular property (though indeed they have made that land exceedingly fertile), and to overlook the penetrating insight of a quarter-century ago which gave us that constant " h," wrung by genius from the deepest recesses of radiation phenomena.

Side by side with progress in the study of radiation has gone a general forward movement towards establishing the constants of the Maxwellian field equations for these infra-red rays. This has of necessity followed along classical lines, but the work of Rubens and his School is the bed-rock of most of what is known about the indices of refraction of substances transparent to this portion of the spectrum. The behaviour of quartz, fluorite, rocksalt and sylvine is of great practical importance, for it is these substances that serve as material for prisms between 1 and 23 μ (1 $\mu = 10^{-4}$ cms.), while quartz becomes valuable again out in the far infra-red at about 70 μ. In general, gratings are essential beyond 23μ, and may easily become so in the near infra-red (*circa* 5 μ) when high resolution

is demanded. In fact, a tandem arrangement consisting of a rocksalt prism of small angle to act as a filter and a suitable grating to effect the dispersion has been found to be the only way of obtaining the fine structure in the rotation-vibration spectra of gases of large moment of inertia.

Some twenty years ago infra-red spectroscopy met with a crisis in its history. Looked at as an electromagnetic flux, these waves offered little more attraction to the investigator. On the other hand evidence was accumulating from thermodynamic quarters that gaseous molecules possessed rotational frequencies of the order 10^{11} and the particles of crystal lattices vibrations of roughly corresponding magnitude.

To consider the case of gases first. The rate at which a dipole radiates energy is a function of the frequency and of the electric moment. If this latter quantity is equal to zero then no positions of selective absorption (sharp bands) corresponding to resonance at the correct frequency can be expected, but if a permanent electric moment exists, then absorption may be anticipated in the infra-red region (since a frequency of 10^{11} corresponds to a wave-length of 30 μ). Much may be learnt in this way about molecular structure and the forces at work within such complexes. As a matter of fact, it is not these pure rotations which provide most of the information desired, but the bands of shorter wave-length associated with vibrations with rotations superposed. This rotation-vibration spectrum is more convenient experimentally: actually the technique involved for pure rotations is exceedingly difficult, while the theoretical "yield" is not correspondingly great.

It is this possibility of deducing from absorption spectra of gaseous molecules in the infra-red a considerable amount of data relating to molecular structure that gives this study its great significance. The moment of inertia may be calculated as well as the geometrical form and dimensions. More important still is the light which is thrown on the types of binding, for in this way may be laid the foundations of a new and profound science of chemistry. It is true that the infra-red is not capable of achieving this by itself: comparison with other spectral regions is required, but the agreement on common ground is usually very encouraging. The chief drawback to the contributions from the infra-red is the uncertain nature of intensity measurements. This is one of the hardest experimental problems to be faced: it is just becoming possible to ascribe quantitative value to the best results obtained for gases, though probably not yet for solids and certainly not for liquids.

It has become increasingly clear as time has gone on that infra-red spectroscopy has much to give to other branches of

physics, but it is also obvious that in questions relating to molecular form X-rays, electric moments and specific heats will all be required to contribute their quota. An excellent example of this is the molecule of carbon dioxide. Infra-red evidence alone is not conclusive in deciding whether the arrangement is linear or triangular, but when combined with the outcome of investigations relating to the specific heat and the temperature gradient of the dielectric constant, the linear form is established. This is an important result, since the X-ray structure of solid CO_2 is known : it is a molecular lattice of the unit O—C—O.

Polyatomic molecules provide very interesting material for research of this character. Until recently there was no very great incentive to undertake such work on account of the complexity of the necessary mathematical machinery, but great advances have been made during the last couple of years in methods of tackling the dynamics. Excessive experimental zeal in the past has often led to the accumulation of much empirical data utterly beyond the powers of a sound and manageable theory to unravel. The mistake will probably not be repeated ; in fact infra-red observations of the requisite degree of precision are so laborious and difficult that nobody should embark upon them unwarily.

Before discussing the contributions from this region to our knowledge of the solid state, it may be well to review the position which has been reached for liquids. Frankly, it is more or less unsatisfactory, and that for a reason as simple as it is profound. In the gaseous state, interaction between molecule and molecule is negligible except at collision, while in the solid state interaction has gone to such lengths as virtually to settle the type of crystal lattice which can exist under the given energy conditions. The latter result in the perfect geometrical arrangement of the true crystalline state which literally invites investigation. The liquid state has neither of these advantages : it has nothing to commend it as a medium for enquiry except its inherent intractability. Naturally enough it has fallen an easy prey to the piling up of data of more than doubtful accuracy, and for which the use at any future time is decidedly improbable. Before any real progress in our grasp of the character of the liquid state is possible much labour of a statistical kind seems unavoidable : should this ever be done there are other ways more amenable to exegesis in preference to absorption methods in the infra-red. The liquid molecule is apparently nearly always highly associated, while the force fields appear to be such as to bring it under the influence of its neighbours at wholly erratic intervals at present beyond our comprehension.

These inherent drawbacks do not apply in full force to

solutions, aqueous or otherwise. Much of a valuable character has been learnt from a study of the infra-red bands of dissolved salts. Without anticipating results, it may be stated quite definitely that along these lines have emerged a number of examples of the conservation of molecular units between the solid and dissolved phases. Much the same is true of present-day notions about the act of sublimation. In a word, change of state is losing for us its cataclysmic nature so much the more as evidence accumulates of molecular complexes persisting under divergent thermodynamic conditions.

Through such transitions as these one is brought to a consideration of the solid state, and of the sort of information which may be extracted about it from infra-red spectra. Here, essentials are distinctly satisfactory. The labours of the X-ray worker have provided a fundamentally static conception of the crystal lattice imagined as a three-dimensional network arranged according to a definite geometrical law: the crystal units—whatever they may be—possess unique co-ordinates with respect to an arbitrarily chosen origin. In contradistinction to this, the infra-red spectroscopist is able to furnish a real crystal dynamics in the sense that it is the (vibrational) movements of the crystal units which he can investigate, and thus he is the connecting link between the worker in crystal optics and those whose interests are thermodynamically inclined. Clearly, such studies have frontiers co-extensive with other branches of physics for considerable distances. Perhaps this has not always been kept in the foregound in framing research programmes to the degree in which it might have been: recent developments in structural questions point quite naturally to common ground. The crystallography of the future must tend inevitably to a comprehensive science of crystal spectra, X-rays, the visible, ultra-violet and infra-red. A wide vista it is.

The molecule makes its presence felt in a variety of ways in crystal spectra: it is the ionic vibrations, however, that may here be discussed most conveniently. They may be registered (for the present purpose) by reflection or by absorption. Much of the older work is confined to the former, but the latter is of predominating importance to-day, and that for a reason which is not far to seek. Positions of resonance give rise to high intensity in either case, but absorption spectra offer the observer a valuable factor of choice which reflection does not, that is, the thickness of the crystal section under investigation. By working with thick specimens the positions of feeblest absorption can be found, while by using thinner and thinner slices it is possible—in theory at least—to reach the bottom of the deepest bands, and thus to ascribe to them their characteristic

frequencies. A complete study should not neglect reflection measurements entirely, for theoretical reasons: it is the absorption bands, however, that correspond very closely to the vibrational periods which it is usually the aim of such work to determine.

Generally speaking, infra-red spectra of crystals fall into two distinct classes often called external and internal. In the former one is dealing with oscillations in which the lattice of A ions is swinging against that of B ions (*e.g.* in NaCl), whereas in the latter it is the movements of the ions within groups or radicles which is responsible for the bands observed. A good example is the intense series of bands associated with various modes of vibration of the carbonate ion CO_3^{--}. The positions of these two systems (external and internal) on a frequency scale show the relative strength of binding: the former occur at wave-lengths some ten times greater than the latter. Moreover, in solution the internal oscillations are found to persist, whereas the process of dissociation obliterates the external variety.

Effects such as those just described depend essentially upon the generation of an electric moment. When modes of vibration are possible but which nevertheless fail to meet this requirement, then no positions of selective resonance can be expected, and the particular frequency is called inactive. The Raman effect enables these inactive oscillations to be brought to light in a number of instances: the method is not direct, but the agreement with calculated values is satisfactory.

From the foregoing, the reader may be able to get a glimpse of a vast subject, one that is as inexhaustible as it is fascinating. Professor Schaefer and Dr. Matossi, whose book, *Das Ultrarote Spektrum*, forms the "text" of the present essay, have placed workers in all subjects which share boundaries with the infra-red under a debt of gratitude for writing this handsome volume in the series *Struktur der Materie*. Only too often pioneers are content to scatter the results of their labours among all the journals of the scientific world without waiting to gather them together, that nothing be lost. It is not only the investigations for which the authors are themselves responsible (and they are legion) which find places in this book, but also the contributions from countries outside Germany which receive attention at once critical and appreciative. In this connection, several names are to be found in the index which in all probability would never have been there (or anywhere else) except for the generosity of Professor Schaefer and the Institutes over which he now presides, or has done so in past years. At least one school of infra-red research having at one time no power of itself to help itself can look on these pages not only with

gratitude, but with the conviction that it owes much of what it may have produced to Professor Schaefer and Dr. Matossi, who had much to give, and gave it in good measure, pressed down and running over.

Nobody could contend that the book was easy reading: obviously that was not to be expected, but the authors have managed to tell a fairly long and intricate story with remarkable clarity, and with a " readableness " that is sometimes absent in works dealing in detail with highly specialised branches of study. Investigators in numerous fields should welcome it, physicists, chemists and perhaps mineralogists too. Should anyone be at the threshold of his career in some such branch of research as this, happy is he if this volume comes his way (although the price is very high), for it will do a great deal. It will not give competence to grapple with such difficult, and at times contrary, things as if by magic—nothing but many nights spent in actual observation can do that—but it will point out the way and keep the path marked out with a sufficient number of poles to guide for the first few miles. There are not very many laboratories where investigations of this kind are undertaken, but in those that there are this book will assuredly be found, consulted daily (and more likely nightly) when nothing but masses of more or less inconsistent data have been gleaned. The authors have been there and they know.

REVIEWS

PHYSICS

An Introduction to the Study of Wave Mechanics. By LOUIS DE BROGLIE. Translated by H. T. FLINT, D.Sc., Ph.D. [Pp. xix + 246, with 14 diagrams.] (London: Methuen & Co., 1930. Price 12*s*. 6*d*. net.)

MODERN quantum mechanics has developed out of the older or "classical" quantum theory along two different lines whose relationship was not at first clear. One of these (matrix mechanics) was initiated by Heisenberg, the other (wave mechanics) by Louis de Broglie, the author of this work. Their fundamental mathematical identity was demonstrated by Schroedinger. They say the same thing in different languages, and most physicists will probably prefer the language of the undulatory form.

The present work quite comes up to our expectations. M. de Broglie is endowed in a special degree with those qualities of lucidity and logical precision which we have long been accustomed to associate with French mathematical physicists. The general introduction to the book reproduces a communication which the author made to the British Association at the Glasgow meeting in 1928, and in it he explains how he was led to his undulatory mechanics. He points out that for a century we have been neglecting the corpuscular aspect in the theory of light, and he asks whether we have not erred in the opposite direction in the theory of matter. "These are the questions the author of this book set himself some years ago in reflecting upon the analogy between the principle of least action and the principle of Fermat, and upon the meaning of the mysterious quantum conditions, introduced into intra-atomic dynamics by Planck, Bohr, Wilson, and Sommerfeld." The introduction contains an outline of the elements of the subject, together with some account of the experimental basis provided by G. P. Thomson, Davisson, and Germer, and a clear and instructive discussion (continued in more detail in later parts of the book) of the meaning of the duality of waves and particles and the Bohr-Heisenberg principle of indeterminacy.

The introduction is followed by a short and sound description of classical dynamics as developed by Hamilton and Jacobi, including its relativistic extensions. Equation (2) in chap. ii is wrongly called Jacobi's equation. It is well known as Hamilton's partial differential equation, and Jacobi himself describes it in this way (*Vorlesungen ueber Dynamik*, No. 19). The important theorem expressed by equation (5) in the same chapter is correctly ascribed to Jacobi.

The ground is further prepared for the description of wave mechanics by a careful account of the conceptions underlying it and of wave propagation in general. The chapter on the motion of the probability wave calls for special praise, and the description which it contains of the nature of the new mechanics, and of what is really *new* in it, is probably the best and most illuminating that has so far appeared.

Special chapters are devoted to light quanta and to the theory of Bohr and Heisenberg, and it is a valuable feature of the book that it exhibits clearly how the new mechanics emerges from classical mechanics, and how it is related

to what is now called "classical" quantum theory, of which a short and useful description is given.

The translation is excellent, and the book can be strongly recommended.

W. W.

Electron Physics. By J. Barton Hoag, Ph.D., Instructor in Physics, University of Chicago. [Pp. ix + 208.] (London: Chapman & Hall, 1930. Price 15s.)

Electron Physics is a book based upon a laboratory course in radio-activity and conduction of electricity through gases which has been held at Chicago for some years. The book is one in which a theoretical outline of each of the main features of that branch of physics which is closely associated with the electron and its properties is followed by a description of more or less simple laboratory adaptations of some of the more striking experiments. The theoretical treatment is very simple, perhaps too simple for most of the students with whom English university teachers have to deal, but the laboratory experiments are clearly and concisely described, and in such a stimulating manner that they may be read with profit by all our students. For example, a laboratory apparatus based on Millikan's method for the measurement of e is not without appeal, and the simple laboratory modification of Busch's method for the measurement of e/m is likely to be very helpful, even if we do not appreciate the statement that the magnetic field is measured in "gausses." About one-third of the book is devoted to appendices which deal thoroughly with the properties and uses of electrometers, the production, measurement, and technique of vacua, and with the various electroscopes in common use. References to works for more advanced study are everywhere given, and a large number of interesting problems is provided at the end of the book.

L. F. B.

The Size of the Universe: Attempts at a Determination of the Curvature Radius of Space Time. By Ludwik Silberstein, Ph.D. [Pp. viii + 215, with 15 figures.] (London: Humphrey Milford, Oxford University Press, 1930. Price 10s. net.)

This book gives, in the first place, a general account of the cosmological theories of Einstein and de Sitter, and, in the second place, a particular account of Dr. Silberstein's own researches in this subject. The first part is muddled and scrappy; the second part savours of special pleading.

To justify these strictures on the first part it is necessary to consider the two lines of approach to cosmological theories. These two methods are respectively metrical and dynamical. The metrical theory arises from the introduction of "distance" into a manifold provided with projective co-ordinates. The dynamical theory is a deduction from the general cosmological equations, $G_{\mu\nu}=\lambda g_{\mu\nu}$. In Part I Dr. Silberstein gives an account of the projective basis of the metrical theory, but stops short of the crucial process of introducing the distance between two points. He then goes on to give, without proof or definition, the expression for the "curvature" of the dynamical manifold, and to apply this expression (in Part II) to the theories of Einstein and de Sitter.

In developing the metrical theory the author agrees that the natural, undisturbed, or free motion of a particle is along a rectilinear route in the 4-dimensional, metrical space-time manifold; but in developing the dynamical theory he introduces, with Einstein and de Sitter, a special system of co-ordinates (page 61), in which this fundamental property is completely obscured. All the curious properties of stellar orbits described by Silberstein are not inherent in the nature of free stellar motion, but are dependent on the peculiarities of his co-ordinate system.

Of course, it may be true that he is right in identifying the particular

co-ordinate system of page 61 (40) with the ordinary system of polar and temporal co-ordinates. It remains for us to consider the experimental evidence. Now, it is admitted that the evidence is indirect and scanty, and the reader is tempted to conclude that Dr. Silberstein's own arguments could be made to appear ridiculous by the same lively methods as he himself has applied to the arguments of other investigators.

Taken altogether, this book is unsatisfactory and misleading, and contains little more true information than the single relevant chapter in Eddington's *Mathematical Theory of Relativity*. G. TEMPLE.

An Introduction to Biophysics. By DAVID BURNS, M.A., D.Sc. [Pp. xix + 580, with 116 illustrations.] (London: J. & A. Churchill, 1929. Price 25*s*. net.)

THIS is a second edition of Prof. Burns' textbook in which the human body is viewed, as far as is at present possible, from the standpoint of the physicist. Slight changes have been made in this edition in order to suit the Syllabus of Biophysics suggested by the General Medical Council: a new chapter on Emulsions and Soaps has been added, and several chapters have been largely rewritten. The book consists of two parts, the second consisting of a useful series of instructions for the production of experiments to illustrate the phenomena discussed in Part I.

Part I is divided into five sections, the first dealing with the laws of energy transformation in all its forms, the second with cellular mechanics, the third with the phenomena exhibited by cells in communities, the fourth with "transport" (circulatory processes), and the fifth with the animal regarded as a whole.

The method of treatment of the subjects is very clear and precise, and the author has succeeded in adopting a style which is interesting and stimulating, which is no mean feat in a textbook which necessarily contains such a mass of facts and theory taken from several other sciences. References are given at the end of each chapter for the use of those who wish to deal more fully with subjects discussed, and mathematical treatment has been reduced to a minimum.

Prof. Burns has succeedod in presenting the essentials of biophysics in a clear and readable form, and his book should prove of increased value to all students of the subject. G. B. BROWN.

CHEMISTRY

Perfumes, Cosmetics, and Soaps, with Especial Reference to Synthetics. By W. A. POUCHER. Volume I, Third Edition. [Pp. x + 394, with 25 plates.] (London: Chapman & Hall. Price 21*s*. net.)

MR. POUCHER'S work on *Perfumes, Cosmetics, and Soaps* is well known to every perfume-chemist, and is much esteemed as a very useful work of reference. First published in 1923, it was divided into two volumes in the second edition, the first volume dealing with raw materials, the second with the manufacture of the various classes of finished products.

This, the second edition of Volume I as a separate book, marks, in several ways, a big improvement on its predecessor. For one thing, it is a considerably larger book, an increase in its size being necessitated by the inclusion of details concerning some thirty more natural products and about 250 new synthetics. Amongst the new substances dealt with, the present reviewer is glad to note titanium dioxide, to the cosmetic utility of which he drew attention some little time ago (*Perf. & Ess. Oil Record*, Oct. 1929; SCI. PRO., Jan. 1930). The large increase in the number of synthetics which it has been necessary to include provides clear evidence of the growing importance of this class of product in the domain of perfumery and of the magnitude

of the research undertaken with a view to the discovery of new carbon-compounds of odour value.

Another improvement consists of the inclusion of the chemical formulæ and physical constants of the various synthetic products. An Appendix gives details of the nomenclature of carbon-compounds, information which should prove of considerable utility to users of the book who are not completely versed in the intricacies of "organic" chemistry.

Another useful addition to the book, which may be noted, is the inclusion of specific gravity and dilution tables for alcohol.

The book is essentially, to quote from its sub-title, "A Dictionary of Raw Materials," the arrangement being an alphabetical one, each product being briefly or fully described according to its importance. Amongst those receiving detailed treatment are the aromatic resins, gum-resins, and oleo-resins. These constitute a very important group of natural products, the study of which presents special difficulties, to the overcoming of which it is understood that the author has directed special attention. The materials in question are of great utility, not only on account of their particular odours, but also because of the good fixative properties which they exhibit. Not only is the chemical composition of these materials exceedingly complex; but, in some cases, their exact botanical source is obscure. Moreover, their identification is rendered the more difficult by the fact that certain names seem to have been transferred from one member to another in a most confusing manner. The little monographs on these products which the volume contains are, therefore, particularly welcome. Indeed, as a work of reference, this is a book which no one concerned in any way with the confection of perfumes and cosmetics can well afford to be without.

H. S. REDGROVE.

Sulphuric Acid and its Manufacture. By H. A. AUDEN, M.Sc., Ph.D. [Pp. viii + 231, illustrated.] (London: Longmans, Green & Co., 1930. Price 16*s.* net.)

THIS book strikes one as a little out of the ordinary, in that it is a definite attempt to help the average student of applied chemistry to grasp the fundamental problems associated with the manufacture of sulphuric acid, and by means of the copious references to enable him to consult the original articles where required. Liebig's statement that a nation's consumption of sulphuric acid is a mark of its degree of civilisation still holds good, and a study of the subject as one of the oldest yet newest branches of applied chemistry is essential for all serious students. Most of the standard works on the subject, however, are of considerable bulk, and of a price corresponding, so that one feels little inclined to buy or read them, but Dr. Auden's excellent résumé of the subject encourages one to do both. The author deals fully with the latest developments, but he also devotes a fair amount of space to the historical development of the various types of plant, so as to show the lines upon which evolution has proceeded.

Whilst the work is in no way intended as a textbook or work of reference on sulphuric acid, a study of its pages will certainly repay most students and not a few teachers.

F. A. MASON.

Oxidation-Reduction Potentials. By L. MICHAELIS, M.D. Translated by LOUIS B. FLEXNER. [Pp. xii + 199, with 16 text-figures.] (London: J. B. Lippincott Co. Price 12*s.* 6*d.*)

THIS monograph may be considered to be the second part of the author's earlier work, *Hydrogen Ion Concentration.* The sequence is a natural one, as an understanding of hydrogen ion concentration is, in practice, a necessary preliminary to a study of oxidation-reduction potentials. Incidentally the introduction of the quinhydrone electrode makes an understanding of the

principles underlying oxidation-reduction potentials desirable even to those mainly concerned in the determination of hydrogen ion.

The book is divided into two parts, the first dealing with physico-chemical considerations, the second with physiological applications. In the first, the author succeeds in giving a good general perspective without introducing many technicalities; so that this section will be found helpful to any reader who comes to the subject for the first time, whether he be a physiologist or not.

The same general interest attaches also to the second part, and those who have read the first part are sure to proceed to the second, whether or no it touches their immediate work. Here we are brought face to face with a fundamental property of living matter, namely, the ability to maintain a highly reducing condition while the surrounding atmosphere contains oxygen. Protoplasm is always respiring. The degree varies considerably, but a total cessation means death. A measurement of the oxidation-reduction condition of living tissues is likely to be a fruitful method of studying in closer detail the characteristics which distinguish living from dead matter, and may have far-reaching consequences in pathology.

The book performs admirably the purpose for which it was written, but we cannot refrain from one note of criticism. As the change in colour of dyes constitutes the most usual method of estimating oxidation-reduction potential, the author is drawn into a discussion of the origin of coloration in dyes. He endeavours to do this without introducing quantum conceptions. This is a pity. The general lines of the modern explanation of the origin of coloration can be put quite shortly. If, however, space did not permit this extension, it would have been better to leave the matter undiscussed than treat it inadequately.

R. K. Schofield.

Latex, its Occurrence, Collection, Properties, and Technical Applications. By Ernst A. Hauser, Ph.D., with a Patent Review compiled by Carl B. von Boernegg, Ph.D. Translated by W. J. Kelly, Ph.D. [Pp. 202, illustrated.] (New York: Chemical Catalog Co., Inc., 1930. Price $4.00 net.)

It is a somewhat curious fact that after having carefully avoided the use of crude rubber latex for about a century and instead coagulating the latex at once on the plantations, rubber chemists are now coming back again to processes in which the latex itself is made use of instead of going through the prolonged operations of coagulation, solution in a solvent, and finally removal of the solvent. In fact, Prof. Hauser tells us that in 1759 the Government of Para sent the King of Portugal as a present a coat made from fabric impregnated with latex, anticipating our old friend Mr. Macintosh by many years!

The chief difficulty in making use of crude latex is on the one hand that unless suitable preservatives are used the liquid ferments and the rubber is coagulated, and on the other hand the bulk of the liquid causes heavy transport charges, so that rubber chemists are at present investigating the possibilities of stabilising and concentrating latex for transport so that it may be diluted up again and utilised for various purposes on arrival in Europe or America. For this reason the work under review provides a valuable summary of what is known on the subject. The scope of the book is indicated by the titles of some of the chapters, such as the Historical Introduction, Collection of Latex, Physical and Chemical Properties of Latex, Coagulation of Latex, Preservation, Shipping, and Concentration of Latex, Industrial Applications, Internal Structure of Rubber, and an Appendix on the Patent Literature of the subject.

It will be seen, therefore, that the subject is very adequately treated for so relatively small a volume, and the only serious criticism that can be

brought against it is that Prof. Hauser does not bring into it sufficient of the latest work, including his own contributions to the subject.

The illustrations and diagrams add much to the value of the work, and the book will, without doubt, be welcomed by many workers in the field of colloid and rubber chemistry. F. A. MASON.

Solvents. By THOS. H. DURRANS, D.Sc., F.I.C. [Pp. xvi + 144.] (London: Chapman & Hall, 1930. Price 10s. 6d. net.)

THIS volume is the fourth of a series of monographs on various branches of applied chemistry issued by the publishers under the editorship of Dr. E. H. Tripp. The greatly increased use of solvents of all kinds during recent years is in part due to the demand for liquids possessing special properties of volatility, solvent power, stability, and so on, for use in the preparation of paints, varnishes, and lacquers, especially those which involve the use of cellulose derivatives. There is, of course, a vast amount of miscellaneous information scattered throughout the literature, but it is mostly unco-ordinated and diffuse, and Dr. Durrans has performed a very useful task in bringing together such published facts as he thinks are reliable and likely to be of practical value. In the first part the author discusses various general considerations which govern the value of solvents such as solvent action, plasticising solvents, viscosity, vapour pressure, inflammability, and toxicity; whilst in the second part the various types of solvents, hydrocarbons, alcohols, esters, ketones, glycols, etc., are summarised. The book should be of very considerable value both to technical and academic workers.

F. A. MASON.

The Beginnings of Chemistry. By HARRIETT BEALE. [Pp. x + 243.] (London: George Allen & Unwin, 1930. Price 4s. 6d.)

FROM the title of this book the reader would infer that it was concerned with the early history of chemical science. This, however, is not so; the object of the work is stated in the sub-title, "A Story Book of Science for Young People," and the author writes in her Preface: "Every book that I have seen on the subject of chemistry presupposes a knowledge on the child's part that he does not possess, and the study which should be delightful to him is wrapped from its inception in a fog of bewilderment."

Certainly Mrs. Beale's book is written in a clear and simple manner, calculated to appeal to young pupils, and those who read it through carefully should acquire a fair conception of the underlying ideas of chemistry. Some sections of the work are good, but others are open to serious criticism, and unless the pupils who use it are given careful guidance and have the misleading passages corrected for them they will acquire some distinctly wrong impressions. Thus, it is stated on page 61, that "there are ten gases among the elements, and mercury and bromine are the only liquids. *The rest are metals.*" Later, the major portion of the chapter entitled "The Metals" is concerned with the *non-metallic elements*, carbon (including several pages on organic compounds and vitamines), silicon, phosphorus, and sulphur! Indeed, apart from two introductory pages on metals in general, the only ones described in the chapter, "The Metals," are antimony and mercury.

Again, in describing the burning of hydrogen in chlorine the beginner is shown how to prepare hydrogen from zinc and *any acid* (page 94), and a little later we read: "So you would have changed phosphoric acid into silver phosphate [by replacement with silver], and you would write your equation like this:

$$H_3PO_4 + 3Ag = Ag_3PO_4 + 3H."$$

That the author has also dished up much theory in childish language is gathered on reading the chapters headed, "The Periodicity of the Elements,"

"Radium," and "The Electron." It is doubtful whether any educational authority in this country would approve of a course of instruction for beginners on these lines. A much sounder plan is to commence by introducing the pupil to simple facts and phenomena, and then to correlate the data to build up the guiding principles, generalisations, and theories. By starting with an historical sketch of the atomic theory, the laws of chemical combination, and valency the author has not, in the reviewer's opinion, made the pupil's path easier.

J. G. F. DRUCE.

The Chemistry of the Colloidal State. By JOHN C. WARE. [Pp. xiv + 313.] (London: Chapman & Hall, Ltd., 1930. Price 18*s.* 6*d.* net.)

THE author, in a somewhat lengthy preface, finds the existing literature unsuitable for the beginner; the larger hand-books are too advanced and the small introductions are becoming obsolete, the presentation not being "in keeping with modern tendencies." Thus they begin with "the classical experiment of Graham on dialysis, which, unfortunately, involves some of the more advanced principles of the subject." The author does not adopt the historical method, but plunges boldly into attempts at definitions, of which more will be said presently. Like every other teacher of the subject, he is confronted with the difficulty of deciding what knowledge he can assume his readers to possess, his experience of large classes at New York University having taught him that "many of the students have shown a very limited knowledge of physical terms, especially those that pertain to topics of an electrical nature." He has, therefore, apparently decided not to presuppose any knowledge at all, but to supply information on difficult matters like the resolving power of the microscope, the polarity of molecules, osmotic pressure, etc., by short excursus in the text or by footnotes. The success achieved by this method may be illustrated by a single instance: On page 78 occurs the term "electrostatic moment," to which the following footnote is appended: "A moment of a force is a single force that will produce the same effect as a combination of two or more forces. It is the effective value of the combination."

The first six chapters deal, roughly speaking, with the physical properties, other than electrical, of disperse systems and are headed: The units of a colloidal solution; sedimentation; interfacial phenomena (non-electrical); turbidity and colloidal suspensions; colloidal suspensions and colour; motion in colloidal suspensions. At the beginning of the first chapter the author writes: "The colloidal state is not determined by size only or by any other single factor, but it can result with the majority of substances under proper treatment and conditions. It is evident that a certain procedure will put one substance in the colloidal state, whereas the same treatment will be ineffective with another. *In other words, an inanimate 'personal equation' must be largely considered.*" (Italics by the reviewer.) Notwithstanding this disclaimer, the author is, like other people, forced to use particle size as a first criterion, though he is alone in introducing the ultramicroscope almost at once. In mentioning the cardioid condenser the author states surprisingly: "If uranium glass is used, fluorescence is developed and this makes the reflections by the suspended particles still more pronounced." The evidence to be gained from filtration and sedimentation, which on all pedagogic grounds should come first, then follows, and the author gives size distribution curves, which are hardly material for beginners. The chapter on interfacial phenomena deals with adsorption among other topics, and the author makes an attempt (on which hardly any other textbook has ventured) to give a deduction of the well-known parabolic formula: "In order to unravel some of the mystery that frequently surrounds the derivation and use of this isotherm by beginners, suppose we revert to the equation previously given. If C_b/C_s does not equal K [*i.e.* if Henry's law fails, Reviewer's note], then

C_b, C_s, or the fraction must be raised to some power to make the identity possible." It is impossible to think of a large class so abject that not one of the students would ask the obvious question, why none of the other possible algebraic operations may be performed on either or both variables.

A somewhat confused account of the Tyndall phenomenon culminates in the statement: " It is evident, then, that lateral scattering of light will not take place when any source of light falls at any angle upon any particle in any solution. This last sentence attempts to emphasise certain features that are of very great importance in a study of the colloidal state." One can only wonder what the student will make of this.

To illustrate the Brownian movement the author gives a reproduction of what is obviously a photograph taken on a falling plate, in which the particles appear as wave-lines. The author seems to think that these are the actual paths of the particles, as he says below the illustration, " It is interesting and refreshing to find that in an actual photograph the vibratory motion of the particles is clearly shown. This is quite in contrast to the traditional diagram."

The author—quite rightly—devotes a chapter to emulsions, although the particle size is generally much in excess of the colloidal range. In addition to much that can be found elsewhere, he gives a method for determining the size of the globules: " In this method, which was developed by Langmuir, a drop of the emulsion, for example oil-in-water, is placed on a clean water surface where it immediately spreads out with great rapidity until a monomolecular layer has been formed. The dispersion must be sufficiently great so that *the suspended particles can be assumed to be spherical* [Reviewer's italics]. By referring to Fig. 82, the relationships can be seen that are necessary for the computation." In this figure, which is described as " The Langmuir method for calculating the size of the particles in an emulsion from monomolecular film relationships," the thickness of the film is, however, marked *d*, which is stated to be the diameter of " each particle." It is, impossible to imagine a worse confusion of ideas than that displayed here, or to understand why the diameters of oil globules which are rarely less than 3 μ should not be measured in commonplace fashion by the aid of the microscope.

Misstatements and misunderstandings similar to the few examples given, and expressed in the same style, could be drawn from almost every chapter of this book if considerations of space did not place a limit on quotations. It is not easy to imagine what class of readers could benefit from a study of the work.

E. Hatschek.

Colloid Symposium Annual. Papers presented at the Seventh Symposium on Colloid Chemistry, Johns Hopkins University, June 1929. Edited by Harry B. Weiser. [Pp. viii + 300.] (London: Chapman & Hall, Ltd., 1930. Price 22*s*. 6*d*. net.)

This volume, which continues the series published under the title " Colloid Symposium Monograph," contains twenty-three papers covering, as usual, a very wide range of subjects, since the Symposium, unlike the annual meeting of the Kolloid-Gesellschaft, does not choose a special aspect of the science of colloids as the main theme of its deliberations.

The paper most likely to create some stir is one by Wilder D. Bancroft (who is obviously the author in the narrower sense of the term), C. E. Barnett, and B. C. Belden on " Compound Formation with a Volatile Base or Acid," which attacks the old problem of the nature of combination between proteins and acids or bases in a new way. The authors begin by pointing out that it is impossible to settle the question by studying solutions " since one may postulate ammonium gelatinate or gelatin hydrochloride with any desired degree of hydrolysis and dissociation, and any desired properties"—provided, of course, that one may postulate such compounds with a body like gelatin at

all. An answer can, however, be obtained by allowing the powdered protein to react with a gaseous acid or base; if chemical combination takes place, the gas pressure remains constant until all the protein has combined to form ammonium proteinate of protein hydrochloride. The authors first show that the diagrams typical of chemical combination are obtained when a number of organic acids, including amino-acids, react with gaseous ammonia, and also when gaseous HCl reacts with organic bases and with amino-acetic acid. The smooth curve characteristic of adsorption, on the other hand, is found when gelatin or casein reacts with ammonia. The authors anticipate a possible attempt to reconcile this result with the theory of stoichiometric combination by saying: "There is no reason to believe that either the gelatin or the casein—much less both—is made up of innumerable acids of such nature as to give a step-wise curve which is indistinguishable from a smooth curve."

A number of papers deal with various aspects of adsorption. Wesley G. France describes the adsorption of dyes by growing crystals of alums and lead nitrate, and the effect on the crystal habitus: "there appears to be no simple rule whereby one can predict what foreign materials will be adsorbed by any given crystalline substance." A. Frumkin, in a paper on "Significance of the Electrocapillary Curve," finds interesting differences between the adsorption at the air-solution and the mercury-solution interfaces: saccharose, *e.g.*, which is negatively adsorbed at the liquid-air surface is strongly adsorbed at the mercury-liquid interface. W. A. Patrick describes a new series of measurements of the adsorption of vapours by silica gel. F. E. Bartell and Ying Fu determine the surface of adsorbents from the heat of wetting and obtain consistent results with a number of liquids. O. Reinmuth and N. E. Gordon, under the somewhat comprehensive title, "Nature of Interaction between Hydrous Oxides and Mordant Dyes," investigate one case in detail and conclude that no "broad general conclusions on the subject of interaction between dyes and gels" may be drawn from it, but that the possibility of complications "introduced by chemical reaction into dye-gel adsorption studies has been clearly indicated."

James W. McBain and R. C. Williams attack the problem of the distribution of electric charges on emulsion particles by a new method. They apply a field which just stops the particles from rising to emulsions of air or oil globules in water containing a little cetyl-sulphonic acid as stabiliser and calculate the number of free charges from the electrical "pull" caused by a known field which equals the buoyancy of the globules. The result, which confirms previous work by McBain and his school, is that only a minute fraction of the total surface, less than 1,100,000, is covered and that "the classical treatment of electrokinetics on the basis of a double layer is invalid."

The "guest of honour" at the symposium was Prof. F. G. Donnan, who describes investigations on the scattering of light in sols and gels carried out by K. Krishnamurti and published some time ago. By measuring the intensity of the scattered light and its degree of "depolarisation," *i.e.* the ratio unpolarised-polarised light, information is obtained regarding the growth and the deviation from sphericity of the particles. The figures indicate that in agar sols from 0·2 to 2·0 per cent. concentration the particles are substantially of the same size, whereas in the corresponding gels they differ considerably, the size increasing with increasing concentration. The interpretation of the Tyndall measurements is, of course, not unambiguous, as arbitrary assumptions regarding the factors in Rayleigh's formula, more especially the difference of the refractive indices, have to be made.

A paper dealing with another aspect of the gel problem is that by S. E. Sheppard and J. G. McNally, who describe a very interesting investigation on the re-swelling and the optical behaviour of gelatin films dried while deformed

and thus permanently strained. They find that "strained gelatin sheet swells practically entirely in the direction at right angles to the direction of deformation," an observation which agrees with the behaviour of deformed bodies of gel during drying. The authors describe a specially designed polariscope by means of which they show that the birefringence of strained and dried gelatin sheet may, according to the strain distribution, be either uniaxial or biaxial. They sketch the arrangements of the micellæ which would produce either isotropy or one or the other type of anisotropy.

The whole volume bears witness to considerable activity which workers in this country can hardly fail to contemplate with a certain amount of envy, since it cannot be ascribed merely to the greater wealth possessed by American institutions. E. HATSCHEK.

The Proteases of Plants. A Record and a Reply. By S. H. VINES, F.R.S. [Pp. 32.] (London: Macmillan & Co., Ltd., 1930. Price 1*s*. net.)

THIS little pamphlet contains a summary of the papers on the proteolytic enzymes of plants published by the author during the years 1897–1910. The main conclusion drawn from these was that there are two kinds of proteolytic enzymes found in plants: one a protein splitting peptase which acting upon such substances as fibrin or albumin breaks them down to albumoses and peptones; the other a peptone splitting enzyme, analogous to Cohnheim's erepsin, which produces amino-acids and notably tryptophane from proteoses and peptones. The immediate cause for the restatement by Prof. Vines of his case is the fact that he has recently become aware that these views have been contested by Willstätter, who holds the view that the two digestive processes are effected by one and the same enzyme—protease or proteinase. Willstätter claims that Cohnheim's erepsin is not a peptone splitting enzyme and that its apparent action upon peptones is due to an impurity of some trypsin.

Prof. Vines' argument is that it does not necessarily follow that if a peptone splitting erepsin does not occur in animals such an enzyme does also not exist in plants. In a postscript the author states that Prof. N. C. Nag, working at the Bose Research Institute in Calcutta, is about to publish results of his work on the proteolytic enzymes of Papaw-juice which confirm Prof. Vines' conclusion that there are in this juice two distinct enzymes—one a peptase which digests fibrin but not proteoses, and the other digesting proteoses but not fibrin and therefore an ereptase. P. H.

The Conductivity of Solutions and the Modern Dissociation Theory. By CECIL W. DAVIES, M.Sc. [Pp. viii + 204, with 22 text figures.] (London: Chapman & Hall, 1930. Price 15*s*. net.)

THE Delye-Hückel-Onsager equation for the equivalent electrical conductivity of very dilute solutions of a completely dissociated electrolyte as a function of its concentration is a new weapon of considerable power. It is now possible to calculate the reduction in equivalent conductivity to be anticipated from interianic attraction, and by comparing this with the observed values to discover any further reduction due to incomplete dissociation, and thus make a more rigorous test of the Arrhenius-Ostwald theory than was formerly possible. The Onsager equation directly applies only to very dilute solutions the conductivities of which can only be determined with sufficient accuracy when due precautions are taken. Nevertheless, the conductivity method when carefully applied is very searching, and as the author shows, when the existing evidence is surveyed the concordance is very encouraging.

This book is stimulating to read because it gives an indication of the future field of usefulness of conductivity measurements as well as a survey

of the more outstanding results of the last decade. At the same time the author deals faithfully with the difficulties and uncertainties of the subject such as the influence of the viscosity of the solvent, the temperature coefficient and the solvation of ions.

The later chapters treat of the bearing of conductivity measurements upon Brönsted's theory of acids and bases, upon complex ion formation, and upon amphoteric electrolytes. Throughout the book the data for non-aqueous solvents are considered wherever they exist. In one chapter the author makes an interesting attempt to use the interior-attraction theory in a general form to elucidate in some measure the behaviour of moderately concentrated solutions ($<0{\cdot}5$ N) such as are usually handled by the chemist in volumetric analysis and other operations.

R. K. SCHOFIELD.

GEOLOGY

The Platinum Deposits and Mines of South Africa. By P. A. WAGNER, D.Sc., D.Eng., F.G.S. [Pp. xv + 326, with 38 plates, 37 text-figures.] (Edinburgh: Oliver & Boyd, 1929. Price 21s. net.)

THIS book helps us to realise the loss the geological world has suffered in the untimely death of Dr. Wagner so soon after its publication. It is the crowning achievement of a career full of achievement. Dr. Wagner was the chief geological expositor of South Africa's astounding mineral wealth. His last book is an account of the vast platinum fields of the Transvaal which were only discovered in 1924. The volume is dedicated to Dr. Hans Merensky, "mining geologist, super-prospector, and best of friends," who played the leading part in this "epic of mineral exploration."

The book opens with valuable chapters on the platinum group metals and their mineralogy. The third chapter deals with the distribution of the platinum metals in South Africa. Dr. Wagner shows that, including the Rhodesian occurrences, a great platinum and gold belt traverses South African geological formations indiscriminately from north to south. He concludes that the *sima* zone beneath is abnormally rich in the precious metals, and recalls Spurr's idea of great "ore canals" in subcrustal regions. Thereafter, Chapters IV and V deal respectively with occurrences of platinum in ultrabasic and basic igneous rocks of the Swaziland (Archæan) formations, and with the detrital platinum metals in the auriferous conglomerates of the Rand. With Chapter VI begins the description of the newly-discovered platinum deposits of the Bushveld igneous complex, certainly the most extensive in the world. This occupies eleven chapters. Chapter XVII contains a valuable discussion of the mineragraphy (study of polished surfaces of ores by metallographic methods), spectrography, and genesis of the platinum-bearing nickel-pyrrhotite ores of the Bushveld complex, by Prof. H. Schneiderhöhn. Then come successively chapters on the platinum deposits of the Great Dyke of Rhodesia, other occurrences of platinum in South Africa, eluvial and alluvial deposits, platinum mining and metallurgy, the present position and prospects of the South African platinum mining industry, and a final chapter on the principal South African platinum mining companies. The book concludes with statistics of production and a full bibliography.

In the series of chapters on the main platinum deposits of the Transvaal petrographers will be gratified by a good short description of the Bushveld igneous complex and its differentiation, illustrated by numerous excellent chemical analyses. The platinum deposits are confined to those parts of the norite zone in the complex in which there is evidence of considerable magmatic splitting. The platinum deposits are highly variable both in nature and origin, and require quite an elaborate table for demonstration of their relationships. A pneumatolytic origin is ascribed to certain types.

Each kind of deposit is described very fully and clearly with the aid of excellent diagrams and plates. In fact, those interested in the South African platinum industry are to be congratulated on having this book at their disposal.

The book has been well produced, printed, and illustrated, and possesses a full index. The word "pneumatolytic" has been twice misspelt on page 222, and in the diagram, Fig. 8 (page 81), the main platinum pipe has been wrongly labelled.

G. W. T.

A Textbook of Geology. Part I. By (the late) Prof. L. V. PIRSSON. Part II. **Historical Geology.** By Prof. C. SCHUCHERT. Part I, Third Edition. Revised by W. M. AGAR, A. M. BATEMAN, C. O. DUNBAR, R. F. FLINT, A. KNOPF, and C. R. LONGWELL. Revision edited by C. R. LONGWELL. [Pp. vii + 488, with 322 figures.] (New York: J. Wiley & Sons; London: Chapman & Hall, Ltd., 1929. Price 18*s*. 6*d*. net.)

IN this book, Part I (Physical Geology) of the great *Textbook of Geology* by Pirsson and Schuchert has been thoroughly revised and practically rewritten by the co-operation of six authors, all members of the Department of Geology in Yale University. The edition has had the advantage of passing through the hands of a single editor, Prof. C. R. Longwell. It may be said at once that the book represents a great advance on Pirsson's original text. The term "Physical Geology," as used in this book, covers all geological science except the stratigraphical side. The order of presentation of the subject has been completely changed. The former subdivision into Dynamic Geology and Structural Geology is abandoned, on the ground that the unity of the subject can be better emphasised, and a clearer picture given, by treating the two phases as indissolubly connected as they are in Nature. Numerous other changes have been made. The introductory chapter has been completely rewritten. The chapter on the work of the atmosphere has been rearranged, soil formation having been emphasised instead of wind erosion, and a short discussion of weather and climate having been added. The treatment of stream erosion has been amplified, and new sections added explaining erosion under semi-arid and arid conditions. More logical and consecutive arrangements have been made in regard to material dealing with glaciers and glaciation, ground water, lakes, and swamps. Sedimentary rocks are dealt with after all the agencies responsible for their production have been considered. Igneous rocks are likewise treated after volcanoes. Folding, faulting, and warping are now considered in one chapter, and a discussion of earthquakes follows. The chapters on the ocean and its work, land forms, and metamorphism have been expanded and brought into conformity with modern views.

The introductory chapter and the chapters on Warping, Folding, and Fracturing in the Earth's Crust, Earthquakes, Nature of the Earth's Interior, and Origin and History of Mountains, have been written by C. R. Longwell; A. Knopf has written the chapters entitled General View of the Earth, Volcanoes, Igneous Rocks, and Metamorphism; A. M. Bateman chapters on the Atmosphere, Weathering and Soils, Glaciers and Glaciation, and Ore Deposits; R. F. Flint, chapters on Rain and Running Waters, Lakes and Swamps, and Land Forms; C. O. Dunbar chapters on Oceans and Seas, and Sedimentary Rocks; while W. M. Agar has contributed the chapter on Subsurface Water, and appendices on Minerals, and Chronology of Earth History. The team work appears to have been excellent, and the authors have so co-ordinated their contributions under the able supervision of Prof. Longwell that they have produced a connected and balanced whole. The work strikes us as an American analogue to the German *Grundzüge der*

Geologie, although it has been conceived on a somewhat smaller scale. Many new illustrations and block diagrams have been added to the well-selected set provided by the late Prof. Pirsson. The book may be commended as a most readable text for first-year students of geology.

G. W. T.

The Physiographical Evolution of Britain. By L. J. Wills, Sc.D., F.G.S. [Pp. viii + 376, with 2 plates, 154 figures.] (London: E. Arnold & Co., 1929. Price 21*s*. net.)

Dr. Wills's arresting and original book is at once a stratigraphic-tectonic history of the British area, and a treatise on palæogeography. It recalls Jukes-Browne's *Building of the British Isles*, but has a much stronger infusion of fundamental tectonic theory than that celebrated work.

The book begins with a summary general discussion of the principles of physiographical and stratigraphical geology. Part II deals with Post-Carboniferous systems, including therein studies of semi-arid continental environments (New Red Sandstone), epeiric marine environments (Jurassic and Cretaceous), and alpine orogeny in relation to present-day conditions in Western Europe (Kainozoic). This section, by the way, contains one of the best short summaries of Alpine structure that the reviewer has seen in English.

Part III deals with the Pre-Cambrian and Palæozoic systems, and is divided into sections treating of Pre-Cambrian environments, the Lower Palæozoic Geosyncline and the Caledonian Orogeny, epeiric environments, mainly in lakes, estuaries, and deltas (Devonian and Carboniferous), and volcanic processes. In the last-named section a number of igneous episodes have been gathered together, Kainozoic as well as Palæozoic. In particular, the newer conceptions of ring eruptions, etc., as illustrated by the Tertiary Mull volcano and others, have been assembled in this chapter, together with some original serial sections. We are not sure, however, that Dr. Wills would not have done better to have included the igneous episodes as integral parts of his stratigraphical outlines.

The unusual non-chronological arrangement of the material is justified on the ground of the relative simplicity and completeness of the Post-Carboniferous geological record. Each physiographic process concerned is discussed when a formation is reached that appears to illustrate it particularly well. The illustrations and sections are clearly drawn, excellently chosen, and of refreshing originality. Dr. Wills has obviously read wisely and well in the most recent British, German, and American literature, and has given in his footnotes a wide range of unhackneyed references. The name "Twenhofel" is consistently misspelt, and there are a number of other misprints.

The modest claim made in the preface of this work to have filled a gap in student literature is better founded than many such claims are. Dr. Wills has gathered an enormous mass of data, and has co-ordinated into a reasoned whole many ideas that have been floating for some time in the literature. He has done the same service for the tectonic and palæogeographical side of stratigraphy as Dr. Neaverson has done for the palæontological side, and all teachers of geology will be grateful to him.

G. W. T.

Handbook of the Geology of Great Britain. A Compilative Work. Edited by J. W. Evans, C.B.E., D.Sc., F.R.S., and C. J. Stubblefield, Ph.D. [Pp. vii + 556, with 67 figures, 24 tables.] (London: T. Murby & Co., 1929. Price 24*s*. net.)

This book is a rewritten and amplified edition of the volume on *The British Isles* which was published twelve years ago as part of the *Handbuch der Regionalen Geologie*. The general plan of the work is the same in essentials

as that of the earlier edition. The original band of contributors, save one, has been retained, but they have been reinforced by Prof. P. G. H. Boswell, Mr. H. Dewey, Prof. E. J. Garwood, Dr. G. H. Plymen, Dr. G. Slater, Dr. H. C. Versey, and Dr. W. B. Wright. The book has been issued under the able editorship of Dr. J. W. Evans, assisted by Dr. C. J. Stubblefield. The allocation of the material is as follows: Pre-Cambrian of England, Cambrian, and Ordovician, Prof. W. W. Watts; Pre-Cambrian of Scotland, Prof. J. W. Gregory, who has also done the Morphology of Scotland; Silurian, Prof. O. T. Jones; Devonian, Dr. J. W. Evans; Introduction to Carboniferous, and Coal Measures, Prof. P. F. Kendall; Lower Carboniferous, Prof. E. J. Garwood; Millstone Grit, Dr. W. B. Wright; Permian, Dr. H. C. Versey; Trias and Rhætic, Mr. L. Richardson; Jurassic, Morphology of England, and the Tectonic Map, Dr. Morley Davies; Cretaceous and Tertiary, Prof. P. G. H. Boswell; Introduction to Quaternary and Glacial Geology, Dr. G. Slater; Channel Islands, Dr. J. Parkinson and Dr. G. H. Plymen. The igneous rocks throughout have been dealt with by Dr. A. Harker, and there is also a section on British Earthquakes by Dr. C. Davison.

The work is purely stratigraphical with a strong bias towards the palæontological side. The tectonic, palæogeographical, and petrographic sides of the subject are hardly mentioned in some contributions. The treatment of igneous rocks in separate sections (except in the Ordovician and Silurian) lends an air of unreality to the descriptions, as though the igneous activity was not really integral to the stratigraphical story.

It is undoubtedly difficult in a compilation of this sort to ensure uniformity amongst the various contributions. There is, however, a fairly uniform and indeed high level of achievement in this work, although the reader occasionally has the feeling that some of the sections are more up-to-date than others, owing, no doubt, to their completion at different times. It will be the source-book of facts concerning pure British stratigraphy for a long period.

The book is notable for a number of excellent correlation tables, geological sections, and bibliographies. It is well printed and produced, and has a very full index. The substitution of commas for periods in the figures for oil-shale production on page 227 has resulted in a ludicrous overstatement of the yields of oil and sulphate of ammonia from that material.

G. W. T.

BOTANY

The Plant Rusts (Uredinales). By J. C. Arthur, in collaboration with F. D. Kern, C. R. Orton, F. D. Fromme, H. S. Jackson, E. B. Moins, and G. R. Risby. [Pp. v + 446, with 186 figures.] (London: Chapman & Hall, Ltd., 1929. Price 32*s*. 6*d*. net.)

The group of obligate fungal parasites with which this work deals is one of great biological interest and economic importance. The diversities of the life-history presenting autœcious and heterœcious types, microcyclic and macrocyclic species, and strains which though morphologically alike are biologically restricted, combine to render the group both intricate and interesting. The authors have therefore wisely prefaced the more detailed consideration by a general account and an historical review. These are followed by chapters treating of the Development and Classification, and the Cytology and Morphology. The succeeding chapter dealing with Dissemination and Geographical Distribution contains a useful summary of available information concerning an aspect that has a much wider appeal than to the special student of this group. The minute size of the diverse types of spore, the profusion of their formation and the fact that both the æcidiospores and basidiospores are forcibly ejected from 0·2 to 15 mm. in the vertical direction, all combine to render their dispersal by air currents remarkably

efficient. But though Stakman obtained spores of wheat rust at an altitude of 16,500 feet, and Dietz obtained evidence of spores of oat rust being carried 50 miles, yet the effectiveness of natural barriers is surprising.

The chapter on Physiology contains a résumé of investigations on such important matters as the temperature range for germination of spores, their humidity requirements, and the effect of external conditions generally. The optimum conditions for infection would appear to be a high humidity (over 95 per cent.), a temperature between 12° C. and 23° C., and a low illumination. The subject of the next chapter, Specialisation, deals with what is perhaps the most interesting aspect of these fungi. But though the facts are of the greatest interest their explanation remains as yet obscure. Why, for example, are the two phases of the heterœcious rusts so commonly found on totally unrelated hosts? Or, again, we may ask why the sporophytic phase of *Puccinia graminia* has such a diversity of hosts among the Gramineæ whereas the gametophytic (æcidial) phase is confined to a few species of *Berberis*. On the other hand, the autœcious *Puccinia suaveoleus* is confined to *Cirsium arvense*, and we now know that many "species" of rust comprise physiological races which are distinguished by the host plants that they can infect. The general restriction of rusts, however, to nearly related hosts has made possible their use in a manner similar to that of serum reaction.

There is a bibliography of 47 pages of literature citation that adds considerably to the value of this work, which is well produced and should prove a useful addition to the bookshelf of the specialist and general reader alike.

E. J. S.

Our Catkin-bearing Plants: An Introduction. By H. Gilbert-Carter, M.A., M.B. [Pp. xii + 61, with frontispiece and 16 plates.] (Oxford: at the Clarendon Press, 1930. Price 4*s*. 6*d*. net.)

Many people who are not botanists take an interest in trees, the study of which adds enjoyment to a country walk in winter, when the more flamboyant attractions of summer no longer distract our attention. This little handbook gives us, in a form convenient for the pocket, a concise account of the botanical characteristics of the commoner native or introduced catkin-bearing trees and shrubs of Britain.

The descriptions are concise, and the author has added useful and interesting notes which reflect that personal acquaintance with the living plant which is so often absent from guides to identification.

The book can be thoroughly commended as a field companion, the value of which is enhanced by illustrations that are not only charming tree studies but really do interpret the text.

E. J. S.

ZOOLOGY

Animal Life on the Seashore. By Prof. L. Renouf. [Pp. 78, with 7 plates.] (London: George Routledge & Sons, 1930. Price 6*d*. net.)

Prof. Renouf deals with a few of the commonest seashore animals in a simple way, giving much information in a small space. Winkles, Sea Anemones, Mussels, Starfish, Prawns, Shrimps and Crabs, Barnacles, Ragworms—all have chapters to themselves, and there is a final general chapter on the seashore, a bibliography, glossary, and seven text-figures. The author, who has had much experience as an out-of-door naturalist, gives us first-hand information on those common animals which are always to be seen and about which so many questions are always asked. What is the difference between a Shrimp and a Prawn? Is a Sea Anemone an animal? What is a Periwinkle? These and many other questions are answered in this attractive

little book, which is quite wonderful for the price, and merits much better reproduction of the illustrations. These figures, which are outline drawings and evidently originally clear and good, showing certain essential structures, are so much reduced and printed on such cruelly rough paper that they are quite spoiled for any detailed study. One cannot help wishing that the book were twice the size and that the figures were printed on smooth paper.

It might be mentioned that the Pea Crab is common in the Edible Mussel, and not only in the Horse Mussel, as might be inferred from the author's words.

M. V. L.

Salmon and Sea Trout, with Chapters on Hydro-Electric Schemes, Fish Passes, etc. By W. L. Calderwood, I.S.O., F.R.S.E. [Pp. xi + 242, with 12 illustrations, mostly on plates.] (London: Edwin Arnold & Co., 1930. Price 12*s.* 6*d.* net.)

For thirty-one years Mr. Calderwood has served as Inspector of Salmon Fisheries of Scotland, and his previous writings have shown him to be an authority on the subject with which he was concerned. The reader will naturally expect a good deal from the present work, and in this he will not be disappointed. It shows the author as one who, while immediately concerned with Scottish problems, has sought far and wide for likely information to help in their solution. So in his company we visit the Pacific Coast of North America, Norway, Eastern Canada, Germany, and New Zealand to consider some experiment or other which throws light upon a debated question. What a pity that the Canadian investigations on the Pacific salmon (*Oncorhynchus nerka*) were not further advanced when the book was written!

The first six chapters are concerned with the natural history of the salmon and the sea trout, and provide a well-digested review of the present state of our knowledge. They are of particular interest, since they are written by one who has an extraordinary acquaintance with the facts at first hand. The remaining five chapters deal with the problems that arise as the result of man's interference with the fish themselves or the waters in which they live: Pollution, Artificial Hatching, Power Dams, Fish Passes, Fisheries Management, etc. Here the author has shown discrimination and restraint, for it would have been easy for one so enthusiastic as himself to take a one-sided view of some of the issues raised. However, he manages to see both sides of the question, and puts forward a strong plea for better understanding between conflicting parties that should command respect. On p. 149, when dealing with the meaning of *p*H, the author seems to have slipped into rather deep water, but this is a minor point that does not affect the soundness of a book which is assured of a welcome from all interested in the salmon and its fishery.

C. H. O'D.

Man's Place among the Mammals. By B. F. Wood Jones, F.R.S. [Pp. viii + 372, with 160 diagrams and 12 plates.] (London: Edward Arnold & Co., 1929. Price 21*s.* net.)

The question of the origin of man and his relationship to other mammals is of perennial interest to layman and scientist alike and one upon which there has been and still is a considerable divergence of opinion. The latest contribution of importance is the book now under consideration, and whether one agrees with its conclusions or not it undoubtedly provides interesting reading. In its forty chapters the characters and interrelations of the Primates are considered from various points of view. Naturally the treatment is uneven, for our knowledge of different forms and groups varies widely. It is well illustrated by twelve plates and 160 figures by the author, and many

of the latter are very useful since they indicate structures or relationships not usually clearly shown.

The author's views on the subject are well known and were first definitely put before the public in a lecture delivered at King's College, London, in 1918, and published as *The Problem of Man's Ancestry.* Briefly they may be stated that man in his evolution has not passed through an ape or a monkey stage but that he has developed independently from a Tarsioid ancestry. The classification of the Primates adoped by Professor Wood Jones, while not actually given, may be deduced from the book. The Primates fall into two divisions: I. Strepsirhini (Lemurs, Lorises, etc.) and II. Haplorhini. The second group contains three sub-orders: (1) Tarsioidea, (2) Pithecoidea (including Old and New World monkeys and the great apes), and (3) Bimana (man). This classification does not appear to be entirely satisfactory even to the author himself, for we find on p. 145, "Following Pocock (1918), the Haplorhini are here divided into two sub-orders: (1) Tarsioidea and (2) Pithecoidea containing all the Monkeys, Apes and Man . . . . " so that here Man is considered as a Pithecoid and this of course implies a pithecoid ancestry as distinct from and in advance of a tarsioid one. Perhaps this is in part covered by the statement on p. 174, "It seems a pity not to accord to the living *Tarsius* what would have been accorded to it had it not survived the Eocene—its right to be included among the Pithecoidea." This, however, leaves the muddle, for it suggests that Tarsioids and Pithecoids are not sufficiently differentiated to be placed in separate divisions. Again on p. 175 after the Tarsioids have been considered we find "The Monkeys and Apes—the remaining members of the Haplorhini." This implies one of two things, either man is not even in the Haplorhini or that man is a monkey or an ape. However, the former is contradicted by the statement on p. 145, and the latter by the statement on p. 307, that man belongs to a separate sub-order, the Bimana. From the point of view of classification, therefore, we are left in a most unsatisfactory position.

His general attitude is a protest against a view that has been widely accepted and assumes that the living forms of monkey-ape-man represent a consecutive series of evolutionary stages. Indeed in its extreme form, when searching for a "missing link," whatever that may be, there has been a tendency to postulate an ancestor for man that is a composite of certain human features and those of some type of ape, usually a gorilla or a chimpanzee. The author protests that "There is no justification for the picture of the slouching, semi-erect Ape Man . . ." He adds some partly deserved but perhaps too severe strictures (p 362) on certain of the well-known reconstructions: "fancy portraits with which pseudoscience has been ready to arrest uninstructed attention." To the extreme, older view his chapters on the anatomical distinctions of man and human characters should serve as a timely corrective.

In spite of its brilliance, which is undoubted, one cannot avoid the feeling that this book is not a full statement of all the available data and facts, that it is one side of the story, that it is a special plea rather than an impartial summary of the evidence. It will undoubtedly provoke discussion and provide an incentive to the further investigations and consideration of its main problem. Its general point of view has been given above, but in conclusion we will take two quotations: "It must be realised at the outset that of all animals the Giant Apes show the nearest structural affinities with Man. This fact has always been realised, and it remains an uncontrovertible truth, for *no other Mammals show in the whole of their make-up so many features that ally them systematically with ourselves*" (the italics are ours); and "But we are still left with a residue of likenesses that exist between Man and the Anthropoids and which are not shared so fully by the Old World

Monkeys." These very considerations are those that need a great deal of explaining away when we try to estimate man's place among the mammals.

C. H. O'D.

The Oligochæta. By J. STEPHENSON, M.B., D.Sc. [Pp. xiv + 978, with 242 text figures.] (London: Humphrey Milford, Oxford University Press, 1930. Price 60s. net.)

This large and important Order of the Chætopoda has been somewhat neglected in general reviews, Beddard's "Monograph of the Order Oligochæta," published in 1895, being the only one available previous to the present work. It is true that eleven years before this Vejdovsky produced his "System und Morphologie der Oligochaeten," but this hardly lives up to its title since it is not a balanced and complete review. In the thirty-five years that have elapsed since the publication of Beddard's volume a great deal of work has been done on the group from many different points of view, and the greater part of that memoir was devoted to systematics. Dr. Stephenson's book fills a need that has long been felt and has been growing more urgent as time went by. The major portion of it is concerned with a general treatment of the group and the systematics occupy but two-ninths of it. This is made possible by Michaelson's systematic review in *Das Tierreich* in 1900.

The first nineteen chapters are concerned with the morphology, physiology, and ecology of the Oligochaetes from all angles and constitutes a veritable mine of information that will appeal to a wide circle of readers and give command of an extensive and widely scattered literature. Many of the illustrations are original, all are well chosen and reproduced, and they form a very useful addition to the text. The proof of the pudding is in the eating, and we have appealed successfully to the pages of this book for answers to a number of questions that are slurred over or judiciously avoided in the ordinary textbooks.

In the preface the author expressly calls attention to the requirements of teachers and general zoologists, and his qualifications for meeting these needs are undoubted, for not only is he one of the foremost authorities on the Order but he is also a teacher of wide experience. When a command of detail is accompanied by the power of clear elucidation and a gift of straightforward, forceful writing, the result is bound to be noteworthy. The book is provided with two useful indices and a bibliography. The latter contains over 1,000 items, and as only a very few of these are included in Beddard's literature list, we have here some measure of the amount of investigation that has taken place in the interval between the two works. The author also makes a plea, that will be heartily supported by all who have to teach general zoology, for workers in other groups to write similar surveys. If they did so and took the present volume as a model they would similarly earn the gratitude of their colleagues, for Dr. Stephenson's *Oligochæta* is a book that no zoological library can afford to be without and one that will for many years be the standard work upon the subject.

C. H. O'D.

New Views of Evolution. By G. P. CONGER. Philosophy for the Layman Series. Edited by R. W. Sellars. [Pp. xii + 235, with 3 illustrations.] (New York: The Macmillan Company, 1929. Price 10s. 6d. net.)

THE recent legislation of certain American states prohibiting the teaching of evolution in schools, universities, and even medical schools has produced a novel situation which educated people in Britain are scarcely able to comprehend. This legislation has revivified the old controversies between dogmatic theology and evolution which have been virtually dead in most

countries for a quarter of a century, and which were abandoned in Britain, together with many other useless things, towards the close of the Victorian era. This book is a contribution to these controversies. It appears to aim at instilling into the layman the broad principles of evolution, and showing him the futility of attacking a purely scientific theory on supposed theological grounds. The book deals clearly and simply with such widely different subjects as the evolution of matter, the evolution of living organisms, and the evolution of human society and culture. It is rather anomalous to call the book "New Views" of evolution, but doubtless much that it contains will be new to the laymen of those unfortunate states that have mistaken law for truth.

F. W. R. B.

Embryology and Evolution. By G. R. de Beer. [Pp. viii + 116, with 7 illustrations.] (Oxford University Press, 1929. Price 5*s.* net.)

Mr. de Beer in this book has set out to attack the theory of recapitulation in its generally accepted form. The vigour of his attack and the manner in which he dismisses the views of those with whom he does not agree will probably raise a storm among the more orthodox embryologists. He has put together in a connected essay thoughts which have been revolving in the minds of the younger generation of zoologists for some time, and which are, in a large measure, the outcome of modern genetical conceptions of the organism. Goldschmidt's theory of intersexuality in the gipsy moth, Lymantria, has played a fundamental part in the formulation of these conceptions. All zoologists to-day are acquainted with this theory, but few have realised, to the same extent as Mr. de Beer has done, its implications and the bearing which they have on embryology and evolution. Goldschmidt has shown that genes vary quantitatively, as well as qualitatively. Moreover, they exert their effects in ontogeny at certain definite rates depending on their quantitative values. It follows from this that mutations may result not only in qualitative variation of characters but in variations in the time of their appearance in ontogeny. The modern conception of constant differential growth rates in different parts of the body, largely due to the work of Huxley, implies that the rate at which any character develops, relative to the rest of the body, is constant in ontogeny, but exhibits variation in different races and species. Mr. de Beer has woven these fundamental concepts together and concludes that evolution is brought about by acquisition of qualitative novelties, and by the production of novel situations by alterations in the time of appearance and the rate of development of characters. New characters may appear at all stages of ontogeny and may be retarded or accelerated, so as to appear earlier or later in subsequent ontogenies.

These arguments and their implications are presented in a lucid manner and are frequently supported by examples. The book is provocative and stimulating, but some parts are less convincing than others. Many will disagree with the views of the author, but few will fail to benefit greatly by reading his book.

F. W. R. B.

A General Textbook of Entomology. By A. D. Imms, D.Sc., F.R.S. Second Edition. [Pp. x + 703, with 607 illustrations.] London: Methuen & Co., 1930. Price 36*s.* net.)

That there should be barely more than five years separating the publication of the first and second editions of this textbook is sufficient testimony of its popularity among entomologists. Its general acceptance by readers and students, especially advanced and research ones, must be a source of great gratification to the author. To Dr. Imms is due much credit for the able manner in which he has presented the materials of his subject and for the

keen discrimination which he has displayed in selecting, in such a vast field as entomology now covers, the essentials of the subject in its anatomical, physiological, and systematic bearings. For the economic entomologist the book has proved of inestimable value, and whether his interests are agricultural or medical, he will continue to find in this new edition the thorough treatment of the principles on which much of his more specialised researches must be based.

The essential plan of the first edition, which was reviewed in this journal (No. 77, July 1925), has been maintained, and any changes which have been made have reference to the chief advances that have occurred in the five years separating the two editions. In the case of the more important orders there has been an extension of the list of chapter references, but this does not seriously affect the pagination, which has only been increased from 698 to 703. The extra five pages at the end are given over to important additions to our knowledge on such questions as polyembryony, pædogenesis, and the physiology of digestion, which could not be very well incorporated in the text without disturbing its arrangement. Concerning the question of muscle structure the author has given a fuller explanation of the several types of muscle fibres found among different insects, and in this regard has called attention to the more modern work of Jordan (1919–20) and Morison (1927). Fig. 46, which represents two sarcostyles from the leg muscle of the bee and the sarcostyles with sarcosomes from the wing muscle of *Hydrophilus*, replaces that of the leg-muscle of an insect borrowed from Schäfer in the first edition. The mechanism of the indirect muscles of the wing is illustrated in Fig. 49 by a new diagram taken from Snodgrass, and since Fig. 47 of the original text has been eliminated, there is thus no change in the total number of 607 excellent illustrations, the reproductions of which leave little to be desired. If any criticism would be offered, it would be that the full names of the particular parts in an anatomical diagram would with advantage replace inscriptive numbers and letters, as has been done in many modern medical textbooks. The interpretation of symbolic letters and numbers involves a heavy expenditure of time by the less experienced reader and is apt to blunt the edge of his appetite for the subject-matter of the text, be it ever so ably presented.

The paragraph on muscular power (pp. 52, 53), which is only of slight interest, has been entirely omitted in the new edition. In the discussion of the respiratory system attention has been drawn to the important researches of Keilin (1925) on cytochrome in two additional paragraphs on p. 120.

Changes in the classification affect only the three orders, Dermaptera, Isoptera, and Thysanoptera. In the Dermaptera three sub-orders, the Forficulina, Arixenina, and Hemimerina, are now recognised, and the first of these is further subdivided into three families. Holmgren's subdivision of the Isoptera into four families is replaced by the classification of Banks and Snyder, who recognise but three. For the Thysanoptera the classification of Karny and Priesner has been adopted in favour of that of Bagnall, whose sub-order Polystigmata, including but the single family Urothripidæ, the former authors have incorporated in the Tubulifera.

Whilst the author does not make serious incursions into the problems of medical entomology, it is of interest to note in passing that on p. 300 it is stated that "there is evidence, however, that *Cimex rotundatus* may carry Kala-azar in India." With this opinion few will be found to agree. At the same time, in view of the important work on the probable transmission of this disease in India by *Phlebotomus argentipes*, which has been actively prosecuted by Christophers, Shortt, Barraud, and Craighead during the past three years, one would have imagined that a brief reference to this species would have been made, despite the question of its incrimination as the actual vector being still *sub judice*. This, however, is a small matter, and Dr. Imms

may be assured that entomologists in both English-speaking and foreign countries amply appreciate the courage with which he has faced the task of producing this comprehensive textbook and the myriad difficulties which he has so successfully surmounted. One and all are immensely indebted to him.

A. E. CAMERON.

The Larval Stages of Caridion, with a Description of a New Species, C. steveni. By M. V. LEBOUR, D.Sc., F.R.S. [Pp. 181–94, with plates 1–8.] (London: Proc. Zool. Soc., 1930.)

THE title conceals something of a romance. Thirty years ago Sars described two larvæ which he attributed to the Prawns *Pandalus borealis* and *P. bonnieri*, and these identifications were accepted on his great authority, to the undoing of those who hoped that Decapod larvæ would throw light on the systematic position of the adults. Now Miss Lebour has found both these larvæ at Plymouth, and has kept them alive until they both moulted into—Caridion, a prawn of quite a different family! But only one species of Caridion is known from northern seas—*C. gordoni*, a species of rather deep water. It would be absurd to suppose that one species has two quite different larvæ; consequently there must be a second, hitherto undiscovered, species of Caridion. Miss Lebour set herself to gather material of the adults and to compare them in all details, with the result that she finds there are two species, *C. gordoni* being the deep-water form, and the new species, *C. steveni*, a species of the shore line. It is rather startling to find a wholly new species of prawn on a coast which has been under intensive observation for a century or so, and it is most satisfactory that the discovery should be made as a result of the study of the larvæ.

The paper includes a detailed account of the adult and the larval stages of both species, illustrated by eight plates, of which three are in colour. Miss Lebour is to be congratulated, and the Zoological Society thanked, for publishing these beautiful pictures.

R. G.

MEDICINE

Guide to the Study of Animal Parasites. By WILLIAM A. RILEY, Ph.D., Sc.D., and REED O. CHRISTENSON, M.A. [Pp. xv + 131, with 33 illustrations.] (London: McGraw-Hill Publishing Co., Ltd., 1930. Price 7*s.* 6*d.* net.)

THE publication of Leuckart's classical work, *The Parasites of Man and the Diseases which Proceed from them*, set a standard in parasitological literature which many later authors have sought to emulate. The last twenty years have seen many important additions made to our knowledge of the parasitic protozoa, helminthes, and arthropods, much of which is now readily accessible to the student in the many excellent textbooks dealing with parasitic species of medical and veterinary importance. In many departments of zoology both in this country and abroad, the practical teaching of parasitology has been very much neglected, probably because the necessary class material was not always readily available, and also because the technique employed in preparing the material for study might have been considered as somewhat too intricate for the average student. Braun and Lühe's *Handbook of Practical Parasitology*, published in 1915, has proved its usefulness to the specialist, but in many respects it is now out of date, and the time was ripe for the appearance of such a book as is now under review. It is the outcome of fifteen years of experience in the teaching of parasitology by the senior author to a class of general students. The parasitic anthropoda have been omitted in that most universities now possess facilities for teaching entomology in special departments, where parasitic species are fully dealt with.

For the teacher the book will prove of the utmost value in advising him

where specimens can be most readily obtained for class purposes, and the student will find all the information required for the manipulation and preparation of specimens for macroscopic and microscopic study. Faithful adherence to the principles laid down in the course of work indicated should render it easy for the student to undertake later some simple piece of investigation. One admirable feature of the authors' methods is the idea of acquainting the student with the literature, so that the latter may discover for himself how information may be acquired concerning some particular problem; and when this has been accomplished successfully, the student is asked to prepare, by way of practice, a brief résumé of the contents of the original papers consulted.

It is to be hoped that the authors will reap the reward of their efforts by seeing the book generally adopted as an aid to the teaching of parasitology, and that, thereby, more biologists will be attracted to a field of investigation in which many rich prizes are still to be won. Careful editing of the text is reflected in the marked absence of typographical errors, but one notes on pages 69 and 71 that "blepharoplast" is spelt "blepheroplast."

Under the list of formulas for reagents and mounting media, credit is erroneously given to Buxton for a medium, which he used in the preparation of specimens of *Sarcoptes* (*Parasitology*, vol. 13, p. 144). This is merely a slight modification of the fluid of De Faure for killing, fixing, and mounting small and delicate objects in one and the same operation and should be termed De Faure's fluid.

The book ends with a very useful list of the more common endo-parasitic species of laboratory animals, arranged, according to the hosts, in their various phyla, classes, and orders. In addition to the most specific references at the end of each chapter, the student will find the more general ones at the end of the book extremely valuable. Altogether we welcome this carefully prepared outline, which at the moderate price of seven shillings and sixpence should find a place on the book-shelves of all parasitologists.

A. E. CAMERON.

Introduction to Human Parasitology. By A. C. CHANDLER, M.S., Ph.D. Fourth Edition. [Pp. i + 655, with 318 illustrations.] (London: Chapman & Hall, Ltd., 1930. Price 25*s*.)

THE three previous editions of this book were published under the title *Animal Parasites and Human Disease*. In the preface the author confesses that these editions failed to woo the fickle general public as judged by the paucity of sales among non-professional readers. One cannot but admire his pertinacity in expending the energy of three separate efforts in trying to gain the favour of this proposed section of his clientele, but now that it has been shown that the public does not desire to have its mind educated parasitologically, the author has changed the title of the fourth edition and left the public to its own resources. So far as the professional reader is concerned, this is, perhaps, the most important change that will be observed. The same general plan has been maintained, and the Protozoa, Helminthes, and Insecta compose the three sections, which are treated in this order. The number of pages has been increased by 82 to 655, and there are 54 additional illustrations.

In his discussion of the pathogenic Protozoa the author includes an account of the border-line Spirochætacea, Rickettsia organisms, and filterable viruses. It is noted that much space is devoted to the depiction of harrowing details consequent on syphilis infection, and one is doubtful as to whether the author has entirely forgotten the abandoned general public and has not donned the cloak of the social reformer. In a book that aims to be scientific valuable space should not be wasted on the untoward effects of promiscuous osculation at a church social in Philadelphia. It is to be noted that the

author is apparently undecided as to whether the credit for the discovery of salvarsan should be ascribed to Schaudinn or Ehrlich (*vide* pp. 7 and 56). The course of development of trypanosomes in tse-tse flies is illustrated by a diagram (Fig. 43) borrowed from Lloyd and Johnson. Unfortunately, however, the aperture by which the organisms are supposed to pass from the labial groove to the salivary glands via the hypopharynx has never been actually demonstrated.

The second section of the book on helminthes is perhaps the best, and is strengthened by the author's personal experience of hookworm investigation in India. The epidemiology, pathology, diagnosis, treatment, and prevention of hookworms are clearly stated. Reference is made to the recent experiments with tetrachlorethylene, which may finally replace the rather uncertain carbon tetrachloride in hookworm treatment. The remarkable life-history of the important human tapeworm, *Diphyllobothrium latum*, which requires three distinct hosts, cyclops, fish, and carnivore or human being, to complete its life-cycle, is given as an example of a pseudophyllidean cestode. In the light of the recent researches of Vergeer (1929) one notes that infection with this species introduced into North America by immigrants from the Baltic is rapidly increasing in the territory adjoining the Great Lakes and many of the smaller Canadian lakes.

The Arthropoda are dealt with in the final eight chapters. The introduction of an adequate scheme of classification of the Acarina and Insecta would have added considerably to the value of this section. As it is, the student must have recourse to some more scientific book for keys which will enable him to identify the genera and species of the arthropoda discussed. We would advise the author that no student really grapples with parasitology until he has learned sufficient morphology to enable him to use an identification key with facility. Contrary to the statement on page 505, the Aschiza do possess a frontal lunule.

Additions in this section concern the rôle of *Simulium* and *Culicoides* in the transmission of filariasis, and *Dermacentor andersoni* as a vector of tularæmia. The rat fleas of the genus *Xenopsylla* are differentiated, but the figures illustrating this differentiation leave room for improvement. We had expected to find that, with the appearance of the fourth edition, the author would have learned that the unpaired piercing stylet of the flea is the labrum-epipharynx and not hypopharynx (*vide* Fig. 224). The borax or hellebore treatment of manure to control housefly maggots has its limitations. Close packing of manure is at once more economical and effective (p. 548). On p. 599 one encounters the statement that *Gastrophilus intestinalis* "would not be expected as a cutaneous parasite in man." The fact is that several cases of its occurrence in the subcutaneous tissues of human beings have been recorded. The life-history and habits of several species require more careful consideration, and this may be done before a fifth edition appears.

A useful list of periodicals and books on parasitology will be found at the end of the book, together with an index of authors and an index of subjects.

We close the book uncertain as to whether to congratulate the author on having turned his back on the public or to sympathise with him for having fallen short of the standard that is expected by the student and professional man.

A. E. CAMERON.

Thyroxine. By EDWARD C. KENDALL, M.S., Ph.D., D.Sc. [Pp. 265, with charts and photographs.] (New York: The Chemical Catalog Company, Inc., 1929. Price $5.50 net.)

THIS book forms the forty-seventh volume of a series of Monographs published by the American Chemical Society. The Committee of this Society had in mind two purposes in recommending the publication of these Monographs. The first "is to present the knowledge available upon the chosen topic in a

readable form, intelligible to those whose activities may be along a wholly different line." The second is "to promote research in the branch of science covered by the monograph, by furnishing a well-digested survey of the progress already made in that field, and by pointing out directions in which investigation needs to be extended."

The present volume, whose value is enhanced by a comprehensive bibliography, undoubtedly satisfies both these requirements. It gives an interesting account of the chemical investigations of the thyroid from the early work of Roos and Baumann in 1895 up to the present day. It describes the author's successful isolation from the gland, after alkaline hydrolysis, of an acid-insoluble fraction, which could be crystallised from an alcoholic solution. It contained a high percentage of iodine, and its administration to a cretin or myxœdematous patient was promptly followed by relief of all symptoms of thyroid deficiency. To this substance he gave the name of thyroxine. It was an important discovery, but made as it was in December 1914, when the thoughts of so many were occupied with other happenings, it did not attract the attention it would have done in normal times. The cost of production was high (it worked out at about $350.00 a gram), it was difficult to obtain in any quantity, and the investigations as to its chemical structure were not at once successful. But in 1926 the young English chemist Harrington, having obtained a sufficient quantity of thyroxine to make a satisfactory study of it, was able by a brilliant piece of investigation to show its structure, and finally to synthesise a compound, tetra-iodo-oxyphenyl-tyrosine, whose physiological activity was identical with that of the natural product. A full account of this work is given, of the methods of separation of thyroxine, and of its chemical and physical properties.

The chemical story having been told, the author then turns to the physiological and clinical aspects of the thyroid gland. A comparison of its activities with the effects produced by administration of thyroxine leads him to conclude that thyroxine produces qualitatively every physiological effect of desiccated thyroid, though quantitatively it is less active.

An interesting point emerges in the chapter on the relation between the iodine value of the gland and its activity. Evidence is given of a seasonal variation in iodine content of the thyroids of animals investigated in the United States, ranging, for example, in the case of the hog from 0·42 per cent. in August to 0·16 per cent. in March, which contrasts with a fairly constant figure throughout the year for sheep all raised within fifty miles of Newcastle. The proximity to the sea of most parts of this country may well account for the lesser amount of thyroid trouble found here.

Reference is made to the interesting suggestion of a distinction between hyperthyroidism and exophthalmic goitre, the latter being described as a case of dysfunction of the gland. This would account for the known fact that certain cases of this form of thyroid trouble can be very definitely improved by administration of the gland (or of thyroxine), which could hardly be the case if the trouble were solely due to excessive activity. Another "internal" secretion is suggested.

The author makes a useful survey of the different methods used for "assay" of thyroid preparations, and shows the necessity for biological methods of assay. He considers that the most reliable test for thyroxine is its protective action against acetonitrile poisoning.

Altogether a very interesting volume, giving the chemical side of the question in sufficient detail to be of value to the chemist, with a condensed but readable and up-to-date account of the influence of the thyroid on the general processes of the body, which will give it an appeal to the wider public always interested in the wonders of the endocrine secretions.

W. C. Cullis.

MISCELLANEOUS

The Living Past. By JOHN C. MERRIAM. [Pp. xi + 144.] (New York and London: Charles Scribner's Sons, 1930. Price 7s. 6d. net.)

IN *The Living Past* Dr. Merriam, President of the Carnegie Institution of Washington, has struck what is, at least on this side of the Atlantic, an unusual note in writing about palæontological and geological subjects. He has taken certain data and described them in the conditions of their discovery, but with an emotional and imaginative colouring in order to impart not their ordered relation in a scientific system but an impression which will emphasise the elements of human interest they embody. Thus " The Story of a Leaf " tells how Dr. Merriam himself discovered a fossil fragment of an actual gingko leaf in the Columbia River gorge, and the results of its comparison under the microscope with a modern leaf of the same plant. This is accompanied with a running commentary on the emotions aroused in himself by the process of discovery and examination. " Pools that reflect the Past " is an account of a remarkable collection of extinct mammalian remains found in an asphalt pool at Rancho La Brea, near Los Angeles, in which the description of a modern squirrel caught in the asphalt is made to illustrate and explain the occurrence of the fossil remains in the deposit. As a whole, the book gives a fascinating view of certain phases of the geology and palæontology of the United States of America in a form perhaps more suited to an American than a British public, which is not accustomed to having its emotional " i's " dotted.

E. N. FALLAIZE.

Orokaiva Society. By F. E. WILLIAMS, M.A. With a Preface by Sir HUBERT MURRAY. [Pp. xxiii + 355.] (Oxford University Press, 1930. Price 25s. net.)

PAPUA, under the mandate of Australia, has been fortunate in an administration which seeks, so far as is consistent with certain ideals of European civilisation, to govern the native with some regard for his customs and mentality. Not only are the officials encouraged to understand the native, but there is an official anthropologist whose business it is to collect information about him. From time to time the Government publishes reports based upon this information. Of such reports this is the tenth. It embodies information collected in the course of two visits to the Northern Division, which covered a total period of fourteen months between 1923 and 1925. The Aigā and Binandele and Wasida tribes were given the closest attention, but all the other tribes were visited. The whole of the data was, however, included in the one account. Mr. Williams has made his report almost entirely descriptive, but here and there, as occasion calls, he has entered upon interpretation of native customs, for which his readers will be grateful. Within this limitation, Mr. Williams has made a very thorough study, in which will be found much of great value to the student of social anthropology in general as well as to the specialist in the study of Papuan culture. Attention may be directed particularly to the chapters on Initiation, Mourning Ceremonies, the Dance and the Drama, and the " Spiritual Substitute."

E. N. FALLAIZE.

Races of Africa. By C. G. SELIGMAN, F.R.C.S., F.R.S. Home University Library of Modern Knowledge. [Pp. 256.] (London: Thornton Butterworth, 1930. Price 2s. 6d.)

WITH the rapid opening up of Africa during the last thirty or forty years, but especially since the advent of the motor-car as a means of transport in that continent, our knowledge of its peoples has indeed greatly increased;

but there are still many lamentable gaps. Until a comprehensive anthropological survey of the physical characters of the people has been undertaken, any account of African ethnology must be far from complete. As Prof. Seligman points out in his Preface, in the matter of ethnographical and linguistic evidence we are in somewhat better case, and for certain areas excellent monographs are available. In the classification of the various races, however, it is still necessary to employ such terms as "Bantu," "Hamitic," and the like, which in strict interpretation are purely linguistic. Another difficulty with which any attempt at a comprehensive survey is faced, if it is to be kept within reasonable bounds, is the enormous variety of African culture; and notwithstanding certain generic resemblances between, for instance, the component people of, say, the Bantu, it is more than hazardous to attempt generalisations from one tribe to another. In view of these considerations Prof. Seligman was hardly to be envied in the task of giving an account of African peoples within the compass of a book as small as this. He is to be congratulated on the success with which he has accomplished it. His work should certainly stimulate interest in the African peoples, to whose abilities and character less than justice is done in public estimation.

E. N. FALLAIZE.

Ethnologische Studien. HERAUSGEGEBEN VON DR. FRITZ KRAUSE, Direktor des Museums für Völkerkunde zu Leipzig. [Pp. 133.] (Leipzig: Verlag der Asia Major, 1929. Price M.10.)

ALTHOUGH this is a separate publication, it resembles an issue of a journal devoted to cultural anthropology, as the papers by different authors deal with a variety of unconnected subjects. There is no introduction to explain why they should have been bound together. The first paper, by S. R. Steinmetz, of Amsterdam, discusses the emergence of the idea of individuality among primitive peoples. As an attempt to systematise the study of this phenomenon, it is suggested that twenty-four tests may be applied to estimate the degree to which a particular society has evolved in this direction. They include such criteria as the incidence of robbery and suicide, beliefs in survival after death, and so on, and exactness in definition is, of course, not to be expected.

The second study, by E. Nordenskiöld, of Göteborg, was written with the idea of emphasising the cultural significance of the slight differences found between the same type of object fabricated in different localities. The wooden signal-drum found among many tribes in Central America is taken as an example. These vary considerably in form in this district, but they can all be supposed to be related to the same migrating culture, while the instruments found in Oceania must have had an independent origin.

K. Birket-Smith, of Copenhagen, contributes the only article in English in this book, all others being in German. It deals with the drinking-tube and tobacco pipe in North America. The drinking-tube is an ordinary article of use among the Eskimo, where it serves the purpose of conveying water to the mouth without the lips coming in contact with it. This article has an extremely wide distribution, however, though it assumes a ritual character among other peoples, such as the Bushmen and Australians. Having been connected in modern times with tobacco-smoking, it has acquired a "renewed vital power, and together with that it was able to conquer the world."

The longest, and apparently the most important of these studies, which is written by E. F. von Eickstedt, of Breslau, deals with the earliest historical records relating to the Veddas of Ceylon and the evidence of an earlier colonisation of the island. It is concluded that Nagas from the north, who

had previously colonised Malabar, were the earliest known settlers and that the present-day Sinhalese are descended from them. It is very commonly stated that the Veddas are the oldest inhabitants and, if this is proved incorrect, the generally accepted theories relating to the cultural and physical histories of the peoples have need to be modified.

A series of children's drawings from Liberia, many of which are reproduced in ten plates, is described by P. Germann, of Leipzig, and they are considered as psychological data by E. Franke. The collection was made in 1928-29, and representation of a watch, a football match, and a robed priest kneeling are the most obvious examples of European influence which is evidenced in most of the drawings to some extent. The influences of both age and race are said to be discernible.

Histories are given of "Das staatliche Sächsische Forschungs-Institut für Völkerkunde" of Leipzig University, by O. Reche, and of "Das Museum für Völkerkunde zu Leipzig," by the editor of this book. Museums and institutions devoted to ethnology are more numerous in Germany than in England, and they appear to be far more alive to the needs of the present-day student there than here.

The reader may find every one of these carefully-prepared studies interesting and instructive, but he may still wonder why they are placed here cheek by jowl; and the specialist who only has need to consult one of them will complain, perhaps. There are maps, plates, and bibliographies.

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Science and the New Civilisation. By ROBERT A. MILLIKAN. [Pp. 194.] (New York and London: Charles Scribner's Sons. 1930. Price 7*s*. 6*d*. net.)

THIS is a book which should be read by all those who doubt whether the pursuit of science is of real benefit to humanity. It consists of a series of addresses by one of the foremost American physicists, dealing with various aspects of the relation of science to human affairs, and in each one the author makes an eloquent plea for more pure research and for a spread of the scientific spirit among all classes of the community. Prof. Millikan sees in the pursuit of science the essential difference between our Western civilisation and that of former ones which have declined, and consequently the warnings of Spenglers and Kaiserlings, he thinks, can be ignored. But he holds that for progress, there are "three great elements" which must be preserved—(1) the idea of the Golden Rule, (2) the idea of natural law, and (3) the idea of evolution, and it is in order to maintain the first that he thinks some mild form of organized religion necessary, and further, that the greatest contribution the United States can make to the world will be to show how science and religion can be successfully combined.

Two of the best chapters are those on "Available Energy" and "The Last Fifteen Years of Physics." It is now practically certain that, in the future as in the past, the sun will remain the source of energy for mankind, since hopes placed in the liberation of energy from atoms have proved to be exaggerated. In an amusing and characteristically American address, Prof. Millikan is at pains to assess "the economic value" of his former teacher, Albert A. Michelson.

In his book, as in so many others of the same sort, one looks in vain for any mention of psychology. For however glowing are the accounts of the way in which science has increased human domination over nature—and on this topic Prof. Millikan lets himself go—few people will be deceived into believing that this alone will increase human happiness. Man's domination over the forces of nature has got ahead of his domination over himself, as Millikan readily admits, and any discussion of the value of science is incom-

plete without reference to the knowledge that has been gained as to the way in which the mind works.

There are a few errors, the worst of which is, perhaps, to describe Mr. H. G. Wells as " not a scientist at all " !

G. B. Brown.

The Science of Voice. By Douglas Stanley. [Pp. vi + 327.] (New York and Boston: Carl Fischer.)

It is always a matter of argument whether singing is an art and whether a certain amount of science is an aid to the singer in developing a beautiful voice. Certain it is that many fine singers produce their voices they know not how, and fail to make others equal their successes either through the medium of books or of personal instruction. The singer has certain sensations; in endeavouring to translate these into physical actions he is obliged to use concrete physical terms which have little or no relation to the physiological processes involved. And so the pupil is told to " throw the voice forward," " get the voice out of the throat," etc.

The present volume is a serious attempt to co-ordinate the science of acoustics and the physiology of the vocal mechanism with the arts of singing and elocution, and is a welcome change from the many unscientific treatises on voice-production which have been written, although it is evident from the perusal of this book that there is still considerable scope for research both on the physical and physiological aspects of voice production; and this necessarily leaves the treatment less exact than a scientist would wish. The book is in fact by three authors; Mr. S. A. Watkins contributes a useful section on elementary sound; the principal author is responsible for the second and major portion, on vocal technique; while in the third part he has the collaboration of Alma Stanley on the subject of " musicianship and interpretation." Like all writers on singing, Mr. Douglas has his own " system," fortunately more concrete than most. His main thesis is that the mouth should be kept at approximately the same size and shape for all vowels, but that the necessary variation in the tuning of the multiple resonator formed by the vocal apparatus should be accomplished by variation in the size of the pharyngeal cavity. Whether this is easy of accomplishment we do not know, but at any rate it forms a definite instruction to the learner, which should be capable of physical interpretation. Therefore this is a book which should be read by all interested in singing, whether as musicians or scientists.

E. G. R.

BOOKS RECEIVED

(Publishers are requested to notify prices.)

Advanced Trigonometry. By C. V. Durrell, M.A., Senior Mathematical Master, Winchester College, and A. Robson, M.A., Senior Mathematical Master, Marlborough College. London: G. Bell & Sons, 1930. (Pp. viii + 335.) Price 8*s*. 6*d*. net.

Hydrostatics. By D. K. Sen, M.Sc., Professor of Mathematics, Rajaram College, Kolhapur. London: Macmillan & Co., St. Martin's Street, 1930. (Pp. vi + 242.) Price 5*s*. net.

Algebraic Equations. An Introduction to the Theories of Lagrange and Galois. By Edgar Dehn, Ph.D., Instructor of Mathematics in Columbia University. New York: Columbia University Press, 1930. (Pp. xi + 208.) Price 21*s*. net.

Calculus. By Egbert J. Miles, Associate Professor of Mathematics, Yale University, and James S. Mikesh, Master in Mathematics in the Lawrenceville School. London: McGraw-Hill Book Co., 6 Bouverie Street, E.C.4, 1930. (Pp. xiii + 538.) Price 18*s*. 9*d*. net.

Studies in the Theory of Numbers. By Leonard Eugene Dickson, Professor of Mathematics in the University of Chicago. Chicago: U.S.A. University of Chicago Press. (Pp. 230.) Price 18*s*. net.

Infinite Series. By Tomlinson Fort, Professor of Mathematics, Lehigh University. Oxford: at the Clarendon Press, 1930. (Pp. iv + 253.) Price 20*s*. net.

A History of Mathematical Notations. By Florian Cajori, Ph.D., Professor of the History of Mathematics, University of California. Volume II. Notations Mainly in Higher Mathematics. London: The Open Court Company, 86 Strand, W.C.2. (Pp. xvii + 367.) Price $6.00.

The Heavens and the Universe. By Oswald Thomas, Ph.D., Professor of Mathematics and Physics Bundesrealschule und Bundesreformrealgymnasium, Vienna; Director Astronomische Zentrale für Wissenschaftliche Liebhaberarbeit. Translated by Bernard Miall. London: George Allen & Unwin, Museum Street. (Pp. 288.) Price 7*s*. 6*d*. net.

Beyond Physics, or the Idealisation of Mechanism. By Sir Oliver Lodge, D.Sc., LL.D., F.R.S. Being a survey and attempted extension of Modern Physics in a philosophical and psychical direction. London: George Allen & Unwin, Ltd., Museum Street. (Pp. 172.) Price 5*s*. net.

The Structure of Line Spectra. By Linus Pauling, Ph.D., and Samuel Goudsmit, Ph.D. London: McGraw-Hill Publishing Co., 6 Bouverie Street, E.C.4, 1930. (Pp. x + 263.) Price 17*s*. 6*d*. net.

Atoms, Molecules, and Quanta. By Arthur Edward Ruark, Ph.D., Lecturer in Physics at the University of Pittsburgh, and Harold Clayton Urey, Ph.D., Associate Professor of Chemistry at Columbia University. London: McGraw-Hill Publishing Co., 6 Bouverie Street, E.C.4, 1930. (Pp. xvii + 790.) Price 25*s*. net.

Matter and Radiation. With Particular Reference to the Detection and Uses of the Infra-Red Rays. By John Buckingham, M.A. London: Oxford University Press, 1930. (Pp. xii + 144, with 15 figures.) Price 7*s.* 6*d.* net.

The Mechanism of Nature. Being a Simple Approach to Modern Views on the Structure of Matter and Radiation. By E. N. da C. Andrade, D.Sc., Ph.D., Quain Professor of Physics in the University of London. London: G. Bell & Sons, 1930. (Pp. xi + 170.) Price 6*s.* net.

L'Origine della Materia Vivente. By Mario Motta. La Storia Fisico-Naturale del Mondo. Milano: Giovanni Bolla, 1930. (Pp. 155.) Price, Lire Dodici.

Das Ultrarote Spektrum. Von Dr. Cl. Schaefer und Dr. F. Matossi. Berlin: Verlag von Julius Springer, 1930. (Pp. vi + 400, with 161 figures.) Price 28 Rm.; bound 29.80 Rm.

A Class Book of Mechanics. By H. E. Hadley, B.Sc., Associate of the Royal College of Science, London. London: Macmillan & Co., St. Martin's Street, 1930. (Pp. x + 335.) Price 4*s.* 6*d.* net.

Theoretical and Practical Mechanics and Physics. With a Chapter on Chemistry. A Preliminary Science Course. By A. H. Mackenzie, M.A., B.Sc., A.R.C.Sc., and A. Forster, B.Sc. Third Edition. London: Macmillan & Co., St. Martin's Street, 1930. (Pp. xviii + 288.) Price 3*s.* net.

X-Ray Crystallography. By R. W. James, M.A., B.Sc., Senior Lecturer in Physics in the University of Manchester, with a General Preface by O. W. Richardson, F.R.S. London: Methuen & Co., 36 Essex Street, W.C. (Pp. vii + 88.) Price 2*s.* 6*d.* net.

Laboratory Manual in Physics. By A. A. Knowlton, Ph.D., Professor of Physics, Reed College, and Marcus O'Day, Ph.D., Assistant Professor of Physics, Reed College. London: McGraw-Hill Book Co., 6 Bouverie Street, E.C.4, 1930. (Pp. xi + 127.) Price 8*s.* 9*d.* net.

Leçons sur la Résistance des Fluides non Visqueux. Professées par Paul Painlevé, Membre de l'Institut, Professeur a l'Ecole Polytechnique. Rédigées par A. Metral et R. Mazet. Première Partie Rédigée par A. Metral. Paris: Gauthier-Villars, 55 Quai des Grands-Augustins. (Pp. 184.) Price 40 fcs.

An Introduction to the Study of Wave Mechanics. By Louis de Broglie, Docteur ès-Sciences, Professor in the Henri Poincaré Institute, Paris. Translated from the French by H. T. Flint, D.Sc., Ph.D. London: Methuen & Co., 36 Essex Street, W.C. (Pp. vi + 249, with 14 diagrams.) Price 12*s.* 6*d.* net.

A Text-book of Sound. Being an Account of the Physics of Vibrations, with Special Reference to Recent Theoretical and Technical Developments. By A. B. Wood, D.Sc., F.Inst.P., Senior Scientific Officer, Admiralty Research Laboratory, Teddington, Member of Council of the Physical Society of London. London: G. Bell & Sons, 1930. (Pp. xiv + 519, with 156 figures.) Price 25*s.* net.

The Principles of Quantum Mechanics. By P. A. M. Dirac, Fellow of St. John's College, Cambridge. Oxford: at the Clarendon Press, 1930. (Pp. x + 257.) Price 17*s.* 6*d.* net.

An Introduction to Organic Chemistry. By Eric John Holmyard, M.A., M.Sc., D.Litt., F.I.C., Head of the Science Department, Clifton College. London: Edward Arnold & Co., 1930. (Pp. xi + 282, with 9 plates.) Price 4*s.* 6*d.* net.

Paint, Powder, and Patches. A Handbook of Make-up for Stage and Carnival. By H. Stanley Redgrove, B.Sc., A.I.C., and Gilbert A. Foan. London: William Heinemann, Ltd., 1930. (Pp. xi + 170, with 16 plates and illustrations.) Price 7*s*. 6*d*. net.

Perfumes, Cosmetics, and Soaps. With Especial Reference to Synthetics. By William A. Poucher, Ph.D., Consulting Chemist. Volume I. Being a Dictionary of Raw Materials together with an Account of the Nomenclature of Synthetics. Third Edition. London: Chapman & Hall, 1930. (Pp. ix + 394, with 30 figures.) Price 21*s*. net.

The Measurement of Hydrogen Ion Concentra. By Julius Grant, Ph.D., M.Sc., A.I.C. London: Longmans, Green & Co., 1930. (Pp. viii + 159, with 40 figures.) Price 9*s*. net.

Stereochemie. Von Georg Wittig, Privatdozent an der Universität Marburg. Leipzig: Akademische Verlagsgesellschaft m.b.H., 1930. (Pp. xi + 388, with 127 figures.) Price 23 M., Gebd. 25 M.

A Comprehensive Treatise on Inorganic and Theoretical Chemistry. By J. W. Mellor, D.Sc., F.R.S. Volume X. London: Longmans, Green & Co., 1930. (Pp. x + 958, with 217 diagrams.) Price 63*s*. net.

The Spirit of Chemistry. An Introduction to Chemistry for Students of the Liberal Arts. By Alexander Findlay, Professor of Chemistry, University of Aberdeen. London: Longmans, Green & Co., 1930. (Pp. xvi + 480, with 62 portraits and illustrations and 88 figures.) Price 10*s*. 6*d*. net.

Simple Geological Structures: A Series of Notes and Map Exercises. By John I. Platt, M.Sc., F.G.S., and John Challinor, M.A., F.G.S., Lecturer in Geology, University College of Wales, Aberystwyth. London: Thomas Murby & Co., 1 Fleet Lane, E.C.4; New York: D. van Nostrand Co., 250 Fourth Avenue. (Pp. 56, with 9 maps.) Price 3*s*. 6*d*. net.

Monographs of the Geological Department of the Hunterian Museum, Glasgow University. No. IV. Reports on Geological Collections from the Coastlands of Kenya Colony made by Miss M. McKinnon Wood and others. Glasgow: Jackson, Wylie & Co., 1930. (Pp. 232, with 24 plates.) Price 42*s*. net.

Principles of Tropical Agriculture. By H. A. Tempany, D.Sc., F.I.C., F.C.S., Director of Agriculture, S.S. and F.M.S., and G. E. Mann, M.A., Instructor, Malay Agricultural Officers, S.S. and F.M.S. Published under the auspices of the Incorporated Society of Planters, Malaya, Kuala Lumpur, F.M.S., 1930. (Pp. xxiii + 327, with 25 figures.)

The Spore Ornamentation of the Russulas. By Richard Crawshay. With Preface by Frederic Bataille, Ancien Vice-President de la Société Mycologique de France. London: Baillière, Tindall & Cox, 7 Henrietta Street, Covent Garden, 1930. (Pp. 234, with 48 plates and 151 figures.) Price 12*s*. 6*d*. net.

Our Catkin-bearing Plants. An Introduction. By H. Gilbert-Carter, M.A., M.B., Ch.B., Director of the Cambridge University Botanic Garden and University Lecturer in Botany. Oxford: at the Clarendon Press, 1930. (Pp. xi + 61, with 16 plates.) Price 4*s*. 6*d*. net.

Further Illustrations of British Plants. By Roger W. Butcher, B.Sc., F.L.S. Drawings by Florence E. Strudwick, Nat. Sc. Tripos (Cantab), M.A. (Dublin). Forming with Fitch's Companion Volume to Bentham's Handbook a Collection of Illustrations of most of the Species in the

British Flora. Ashford, Kent: L. Reeve & Co., Lloyds Bank Buildings, Bank Street, 1930. (Pp. vii + 476, with 485 illustrations.) Price 12*s*. net.

Guide to the Study of Animal Parasites. By William A. Riley, Ph.D., Sc.D., Department of Zoology, University of Minnesota, and Reed O. Christenson, M.A., Department of Zoology, University of Minnesota. London: McGraw-Hill Publishing Co., 6 Bouverie Street, E.C.4, 1930. (Pp. xv + 131.) Price 7*s*. 6*d*. net.

Introduction to Human Parasitology. By Asa C. Chandler, M.S., Ph.D., Professor of Biology, Rice Institute, Houston, Texas. Fourth Edition. Rewritten and Enlarged. New York: John Wiley & Sons; London: Chapman & Hall, 1930. (Pp. xiv + 655, with 308 figures.) Price 25*s*. net.

The Great Apes. A Study of Anthropoid Life. By Robert M. Yerkes, Professor of Psychobiology in Yale University, and Ada W. Yerkes. New Haven: Yale University Press; London: Oxford University Press, 1929. (Pp. xix + 652, with 172 illustrations.) Price 45*s*. net.

A General Textbook of Entomology. Including the Anatomy, Physiology, Development, and Classification of Insects. By A. D. Imms, D.Sc., F.R.S., Chief Entomologist, Rothamsted Experimental Station, Harpenden. Second Edition, Revised. London: Methuen & Co., 36 Essex Street, W.C. (Pp. xii + 703, with 607 illustrations.) Price 36*s*. net.

Human Biology and Racial Welfare. Edited by Edmund V. Cowdry and assisted by twenty-six contributors. With an Introduction by Edwin R. Embree. London: H. K. Lewis & Co., 1930. (Pp. xvii + 612, with illustrations.) Price 28*s*. net.

Studies on the Structure and Development of Vertebrates. By Edwin S. Goodrich, M.A., D.Sc., LL.D., F.R.S., Fellow of Merton College, Linacre Professor of Zoology and Comparative Anatomy in the University of Oxford. London: Macmillan & Co., St. Martin's Street, 1930. (Pp. xxx + 837, with 754 figures.) Price 36*s*. net.

Materialism and Vitalism in Biology. By Sir Peter Chalmers Mitchell, C.B.E., F.R.S., D.Sc., LL.D. The Herbert Spencer Lecture delivered at Oxford, June 3, 1930. Oxford: at the Clarendon Press, 1930. (Pp. 30.) Price 2*s*. net.

The Larval Stages of Caridion. With a description of a New Species, C. Steveni. By Marie V. Lebour, D.Sc., F.Z.S., Naturalist at the Plymouth Laboratory. From the Proceedings of the Zoological Society of London, 1930. Published May 9, 1930. (Pp. 181–94, with 8 plates.)

The Development of Sex in Vertebrates. By F. W. Rogers Brambell, B.A., Ph.D., D.Sc., Lecturer in Zoology, King's College, London. With a Preface by Julian S. Huxley. London: Sidgwick & Jackson, 1930. (Pp. xvi + 261, with 24 plates and 25 figures.) Price 12*s*. net.

The Fauna of British India, including Ceylon and Burma. Volume I, Cestoda. By T. Southwell, D.Sc., Ph.D., A.R.C.S., F.R.S.E., Lecturer in Helminthology, School of Tropical Medicine, Liverpool. London: Taylor & Francis, Red Lion Court, Fleet Street, 1930. (Pp. xxxi + 391, with 221 figures.) Price 22*s*. 6*d*. net.

The Coconut Moth in Fiji. A History of its Control by Means of Parasites. By J. D. Tothill, D.Sc., assisted by T. H. C. Taylor, B.Sc., and R. W. Paine, B.A. London: The Imperial Bureau of Entomology, 41 Queen's Gate, S.W.7, 1930. (Pp. vii + 269, with 34 plates and 119 figures.)

An Introduction to Vertebrate Embryology. By H. L. Wieman, Professor of Zoology, University of Cincinnati. London: McGraw-Hill Publishing Co., 6 Bouverie Street, E.C.4. (Pp. xi + 411) Price 20s. net.

Animal Breeding. By Laurence M. Winters, Animal Breeder and Associate Professor of Animal Husbandry, University of Minnesota. Second Edition, Rewritten and Reset. New York: John Wiley & Sons; London: Chapman & Hall, 1930. (Pp. x + 389, with 103 figures.) Price 18s. 6d. net.

Animal Ecology and Evolution. By Charles Elton, Department of Zoology and Comparative Anatomy, University Museum, Oxford. Oxford: at the Clarendon Press, 1930. (Pp. 96.) Price 4s. 6d. net.

Sensation and the Sensory Pathway. By John S. B. Stopford, M.D., F.R.S., Professor of Anatomy, the University of Manchester. London: Longmans, Green & Co., 1930. (Pp. xii + 148, with 19 figures.) Price 7s. 6d. net.

Industrial Hygiene for Schools. By Jesse Feiring Williams, A.B., M.D., Professor of Physical Education, Teachers' College, Columbia University, and Delbert Oberteuffer, Ph.D., Supervisor of Health and Physical Education, Department of Education, State of Ohio. London: McGraw-Hill Publishing Co., 6 Bouverie Street, E.C.4, 1930. (Pp. xiv + 280.) Price 10s. net.

An Introduction to Physical Anthropology. By E. P. Stibbe, F.R.C.S., Senior Demonstrator in Anatomy at University College, University of London. London: Edward Arnold & Co., 1930. (Pp. vii + 199, with 42 figures.) Price 12s. 6d. net.

Your Character from Your Handwriting. A Guide to the New Graphology. By C. Harry Brooks. With a Foreword by Robert Saudek. London: George Allen & Unwin, Ltd., Museum Street. (Pp. 155.) Price 3s. 6d. net.

The Investigation of Atmospheric Pollution. Report on Observations in the Year ended March 31, 1928. Fourteenth Report, Department of Scientific and Industrial Research. London: Published under the Authority of His Majesty's Stationery Office, 1930. (Pp. xi + 67.) Price 3s. 6d. net.

The Use of the Microscope. A Handbook for Routine and Research Work. By John Belling, Cytologist, Carnegie Institution of Washington. London: McGraw-Hill Publishing Co., Ltd., 6 Bouverie Street, E.C.4, 1930. (Pp. xi + 315, with 28 figures.) Price 20s. net.

The Progress of Life. A Study in Psychogenetic Evolution. By Alexander Meek, D.Sc., Professor of Zoology in the University of Durham. London: Edward Arnold & Co., 1930. (Pp. vii + 193.) Price 10s. 6d. net.

The Sex Factor in Marriage. By Helena Wright, M.B., B.S., with an Introduction by A. Herbert Gray, M.A., D.D. London: Noel Douglas, 1930. (Pp. 97.) Price 3s. 6d. net.

Gestalt Psychology. By Dr. Wolfgang Köhler, Ph.D., Professor of Philosophy at the University of Berlin. London: G. Bell & Sons, 1930. (Pp. xi + 312.) Price 15s. net.

The Retreat from Parenthood. By Jean Ayling. London: Kegan Paul, Trench, Trübner & Co., 68 Carter Lane, E.C., 1930. (Pp. xvi + 293.) Price 10s. 6d. net.

Science in Soviet Russia. By J. G. Crowther. London: Williams & Norgate, Ltd., 1930. (Pp. 127, with 24 illustrations.) Price 7s. 6d. net.

Cambridge University Agricultural Society Magazine, 1930. Cambridge: W. Heffer & Sons. (Pp. 78.) Price 2*s*. 6*d*. net.

The History of Biological Theories. By Emanuel Ràdl, Professor of Natural Philosophy in the University of Prague. Translated and adapted from the German by E. J. Hatfield, M.Sc. London: Oxford University Press, 1930. (Pp. xii + 408.) Price 17*s*. 6*d*. net.

A Hundred Years of Publishing. Being the Story of Chapman & Hall, Ltd. By Arthur Waugh, Managing Director, 1902–1930. London: Chapman & Hall. (Pp. xvii + 262, with 41 illustrations.) Price 15*s*. net.

A Critical Review of Literature on Amplifiers for Radio Reception. Department of Scientific and Industrial Research. Radio Research. Special Report, No. 9. London: Published under the Authority of His Majesty's Stationery Office, 1930. (Pp. viii + 239.) Price 5*s*. net.

The Science of Voice. By Douglas Stanley, M.A., A.C.G.I. With a Preface by H. H. Sheldon, Ph.D. Boston: Carl Fischer, 252 Tremont Street. (Pp. vi + 327.)

Popular Research Narratives. Volume II. Collected by the Engineering Foundation of New York. London: Baillière, Tindall & Cox, 8 Henrietta Street, Covent Garden, W.C.2, 1930. (Pp. viii + 174, with 5 plates.) Price 4*s*. 6*d*. net.

Babel, or the Past, Present, and Future of Human Speech. By Sir Richard Paget, Bart., Fellow of the Physical Society of London. London: Kegan Paul, Trench, Trübner & Co., 68 Carter Lane, E.C., 1930. (Pp. 93.) Price 2*s*. 6*d*. net.

Autobiographical and Other Writings. By Alan A. Campbell Swinton, F.R.S., M.Inst.C.E., M.I.E.E. London: Longmans, Green & Co., 1930. (Pp. 181, with 18 portraits.) Price 10*s*. 6*d*. net.

The Science of Folk-Lore. By Alexander Haggerty Krappe, A.M., Ph.D. London: Methuen & Co., 36 Essex Street, W.C.2. (Pp. xxi + 344.) Price 10*s*. 6*d*. net.

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SCIENCE PROGRESS

RECENT ADVANCES IN SCIENCE

ASTRONOMY. By R. W. Wrigley, M.A., Royal Observatory, Edinburgh.

The Rotation of the Galaxy.—Due doubtless to a general analogy with other celestial bodies, this has long been a subject for rather vague speculation, but until recently the solar motion and the two stellar drifts have been the only well-authenticated systematic movements among the stars. The modern view of galactic rotation was first advanced by Lindblad, and more fully developed by Oort in a series of mathematical papers published in 1927 (*B.A.N.*, 120). Since then the subject has attracted considerable attention, and, in general, Oort's position for the centre of rotation (galactic longitude 324°·5) has been supported. In Monthly Notices, *R.A.S.*, 90, p. 233, Sir Frank Dyson examines the proper motions of stars in Boss's catalogue of types B0—B5, B8, B9, A0, and A2—A5 lying within 30° of the galactic plane. The proper motions of the A2—A5 stars showed such irregularities when meaned for each 10° of longitude that they were finally excluded from the results. Analysis gives for the combined group:

$$\mu_l = -\ ''\cdot 0036 + ''\cdot 0020 \cos 2\,(l - 330^\circ) + ''\cdot 0046 \cos 3\,(l + 5^\circ).$$

The first two terms are in good accordance with Oort, and it is suggested that the term in $3l$ is due to the peculiar motions of the stars, and that an accurate result could be obtained by using fainter stars of the same types of spectrum distributed over the two hemispheres within 30° of the galactic plane. This would furnish a greater number of stars with smaller proper motions, and should therefore decrease the irregularities affecting the mean value of μ_l.

Different conclusions are reached by H. Mineur (*M.N.R.A.S.*, 90, p. 516), who maintains that the centre of rotation may preferably be fixed at longitude 243°, a position which corresponds to the centre of the local star cloud. He supports his contention by a general study of the radial velocities of 2,146 stars, the proper motions of Sir Frank Dyson's investigation, and a study of the Solar Apex, and concludes that the move-

ment of the nearest stars is composed of two rotations, one round the centre of the local cloud and the other a rotation of this cloud round the centre of the galaxy. The former is at longitude 243° and the latter at 325°, and the total movement is not unlike that of the Moon in the solar system.

These views are adversely criticised by Bart J. Bok in *Bulletin* 876 of the Harvard College Observatory. It is pointed out that in Mineur's analysis of the Lick radial velocities the values of the rotation co-efficients are in some cases less than their mean errors. Doubt is thus thrown upon his proof of a rotation of the local cluster round an axis in Carina, and it is further urged that Oort's centre is the only one dynamically possible, in view of the available evidence from radial velocities and proper motions. The evidence from the latter is uncertain in amount, but definite at least as to the sign, and therefore the direction of the rotation can be regarded as fixed.

The evidence given by the radial velocities of the high-temperature stars of Types O to B5 is admirably summarised by J. S. Plaskett in the 1930 George Darwin Lecture (*M.N.R.A.S.*, 90, p. 616). Following an analytical discussion making use of the radial velocities of 680 stars, the longitude of the centre of rotation is found to be 326°·4, a result very near to Oort's value, and in close agreement with that found by Shapley for the direction of the centre of the system of globular clusters. The investigation at Victoria of these high-temperature stars has not only furnished the most convincing evidence yet brought forward of the reality of the differential rotation of the galactic system, but it has also thrown light on several other points of interest. It has long been recognised that stars of early type show a tendency to an excess of positive radial velocities, as if the system was expanding with respect to the Sun, and hitherto this " K " term, as Campbell named it, has been unexplained. The situation is complicated by the practical non-existence of the term in the motions of B type stars fainter than the sixth magnitude, and by its persistence for O type stars whether faint or bright. Plaskett concludes that one single explanation will not suffice for both types of stars. The high K term for the brighter B stars he ascribes mainly to high systematic group motions of recession in the southern sky, very marked between galactic longitudes 240° and 290°, while for the O stars he holds the Einstein displacement of the spectral lines towards the red to be chiefly responsible. These O stars are all of enormous mass, and it is calculated that for Type O9 the displacement would reach + 2·5 km. per sec., and would be still greater for earlier types. For B stars the values would be smaller, but would still be

sufficient to reduce materially the magnitude of the K term, practically removing the + ve residual in the brighter northern stars, and leaving that in the southern to be explained by group motion.

In *B.A.N.*, No. 196, J. H. Oort obtains further support for his theory of the galactic rotation from an analysis of the velocities of the extra galactic nebulæ. Assuming that the large outward radial velocities of these nebulæ are approximately proportional to their distances, he determines the solar motion from the residual nebular motions which remain after the term proportional to the distance has been removed. Certain groups of nebulæ have been treated as single units with, in general, rather higher weight. A least-squares solution gives a solar motion agreeing in direction, and approximately in magnitude, with the rotational velocity of the galactic system.

Temperature of Stars.—In Monthly Notices, *R.A.S.*, 90, p. 636, Prof. R. A. Sampson continues an earlier discussion (85, p. 212), and publishes the effective temperatures of 80 stars ranging down to magnitude 3, and, therefore, with but few exceptions, giants of high luminosity. The spectra were taken, as before, with a Merz objective prism of 12° angle placed in front of a 6-inch photovisual telescope of 9 feet focus, and the dispersion from (B) to (K) amounted to 22 mm. The method used is in essentials the same as was described in the earlier paper. Seven hundred and thirty spectra are employed in the discussion, giving a mean of 9·5 per star, and, in addition, Polaris was taken first and last on each plate for comparison, while α U.Ma and β U.Ma were each taken 30 times at different zenith distances in order to investigate the effect of the earth's atmosphere. The photographic theory postulates nothing except that for standard development the blackening of any batch of plates for any wave length is a regular function of log (intensity of light) and the time of exposure. One plate of each batch is exposed to measured light, and "interpretation curves" are derived therefrom connecting log (intensity) with density of blackening, the latter being measured by a Koch microphotometer. The equation used is

$$\log (J_\lambda/J_\lambda^1) - \log (I_\lambda/I_\lambda^1) = \text{constant}$$

where J_λ, J_λ^1 are the intensities for the same wave-lengths of the radiations of the star and of Polaris, and I_λ is the intensity from the "interpretation curve" relative to I^1_λ, an arbitrary standard or zero. The quantities are plotted and the gradient determined from the measures for all wave-lengths for the star and Polaris. In order to assign definite values to the tempera-

tures obtained from the gradient, it is necessary to adopt some standard temperature, terrestrial or stellar, and in this investigation Capella has been used with an assigned figure of 5,500°. Any terrestrial standard is objected to as lying too near the border of the lowest of the stellar temperatures for extrapolation to be reliable, while the colour index method, depending for its numerical constants on certain adoptions, cannot have the same authority as a more direct procedure. Atmospheric absorption is dealt with more carefully than in the earlier paper, and a zenith distance correction is applied in all cases. From the mean curve the following values are obtained:

Bo	20,000°	Go	6,200°
Ao	12,600°	Ko	4,400°
Fo	8,600°	Mo	3,500°

A comparison with the Greenwich results, after reduction to the same standard (Ao 11,000°), shows substantial agreement, which is satisfactory when the difference of method employed is taken into consideration.

Absorption of Light in Space.—This would be caused by the presence of interstellar matter in any form—free electrons, atoms, molecules, or solid bodies of larger size—and the importance of the question lies in the effect which such absorption would have on observations which have been used as foundations or supports for various theories concerning the structure of the universe. Statistical determinations of stellar distribution based on star counts, and photometric distance determinations such as spectroscopic parallaxes and variable star parallaxes would all be affected, while if the absorption were selective, distance alone would alter the apparent colour of a star. About a generation ago it was observed that the spectra of several spectroscopic binaries showed "stationary" calcium lines, and these were afterwards found by Plaskett in all O-type stars. The velocity differences left no doubt as to the relative motion of the star and of the absorbing material which produced these lines. In *M.N.R.A.S.*, 90, p. 243, Plaskett and Pearce attempt to determine the distribution of the interstellar matter by analysing the stellar and interstellar velocities with reference to a differential rotation of the galactic system. For the investigation 235 O and B type stars were selected, and, from the spectrum of each, reliable radial velocities both of the star and of the interstellar cloud were obtained. These were divided into five groups, according to their magnitude, and, assuming the usual solar motion, were analysed for the K term and the galactic rotation. For all the groups except those of the brightest and the nearest stars the rotational term

for the stars appeared to be almost exactly twice that for the clouds, suggesting that the stars are at twice the mean distance of the clouds. If the indeterminate result for the nearest and brightest stars is ascribed to their small numbers and large peculiar motions, the general inference is that the interstellar material is uniformly distributed throughout space, a hypothesis first arrived at by Eddington from theoretical consideration (Bakerian Lecture, 1926).

The same problem is dealt with by Robert J. Trumpler in the Lick Observatory *Bulletin*, No. 420 (vol. xiv, p. 154), and his deductions are stated more fully in the *Publications of the Ast. Soc. of the Pacific*, vol. xlii, No. 248. In the former paper he describes his preliminary conclusions from a study of the open star clusters, for 100 of which spectroscopic data are available. A valuable feature is the catalogue of 334 open galactic star clusters, for each of which, in addition to the equatorial, galactic, and rectangular space co-ordinates, there are given the angular diameter, the classification, and the distance in parsecs. Forty-one of these objects have not previously been listed. A study of their space distribution shows that these open clusters form a much flattened disc-like system about 1,000 parsecs thick with a diameter of about 10,000 parsecs, having a strong concentration towards a point situated at a distance of 350 parsecs in galactic longitude 247°, near to the rich open cluster N.G.C. 3532. Support is given to the view that the Milky Way System is a spiral nebula, of dimensions similar to that in Andromeda, but issue is joined with Shapley and Seares in their recent conclusions that the Sun and its surrounding star concentration are a secondary formation in a much larger system of which the centre is at a distance of 20,000 to 40,000 parsecs in the direction of longitude 325°. Trumpler prefers to consider the globular clusters as extra-galactic objects.

He investigates the absorption of light in space by comparing the distances of 100 open clusters determined by two different methods: (1) from apparent magnitudes and spectral types, and (2) from measures of their angular diameters. It is shown that for the nearer clusters the second method gives consistently greater values, but for the more distant it gives smaller ones. Assuming the improbability of a general decrease in the dimensions of the clusters depending on their distance from the Sun, the evidence points to a general absorption of light which modifies the inverse square law on which the photometric distances are based, and, on the assumption that this absorption is approximately uniform within the region considered, its numerical value comes out as 0·7 mag. for 1,000 parsecs. The studies of the diameters of globular clusters and

spiral nebulæ by Shapley and others show little or no indication of such absorption, and it would therefore seem that the absorbing matter is chiefly confined to the Milky Way System and that extra-galactic space is much more transparent. Trumpler further discusses the question of the selective nature of the absorption. The colour of a star depends mainly on its surface temperature, and if the latter be determined independently by consideration of the spectral type and the degree of ionisation in its atmosphere, then a selective absorption can be detected if an increase of distance is found to change the mean colour index of a given spectral type. From the open star clusters evidence is found in favour of an absorption in low galactic latitudes giving an increase in colour index of about 0·3 mag. per 1,000 parsecs, a result in good agreement with earlier work by Kapteyn, Miss Slocum, and others. Following an examination of the numerical observational data in conjunction with Eddington's theoretical studies, Trumpler formulates a tentative hypothesis regarding the nature of the finely divided matter spread throughout the Milky Way System. He considers it to be composed of (1) free atoms which give rise to the interstellar absorption lines observed in the spectra of distant stars, (2) free electrons, (3) fine cosmic dust maintained in space by light pressure, and producing the observed selective absorption by Rayleigh scattering, and (4) a certain number of larger meteoric bodies which obstruct equally light of all wave-lengths. The dark nebulæ are considered to be obscuring clouds of much greater opacity.

Astronomy and South Africa.—The discovery of the remarkable clearness of the South African skies marks an event of great significance in the history of astronomical research in the southern hemisphere. The chief credit is due to Dr. R. T. A. Innes, who, from the time of his appointment to Johannesburg in 1903, has lost no opportunity of making known the wonderful observing conditions obtainable on the High Veldt. New observatories are now springing up like mushrooms in this favoured land. Bloemfontein has been selected as the location for two American establishments representing Ann Arbor and Harvard Universities, Johannesburg has been given the preference by Yale and Leiden, while South-west Africa has been chosen by the Smithsonian Institution and by the Berlin-Babelsburg Observatory. As regards Great Britain interest centres in the proposed transfer of the Radcliffe Observatory from Oxford to a site near Pretoria. The removal is strongly supported by the National Committee for Astronomy, and undoubtedly has much to commend it. The climate of Oxford cannot be compared to that of Pretoria, and, in addition, observation of the southern hemisphere

still lags far behind that of the northern, though doubtless the leeway will be made up in due time. But, if it be granted that suitability of site and excellence of seeing are of such prime importance as to justify a transfer involving such a break with tradition in addition to considerable expense and temporary dislocation, surely a strong case could be made out for removing the Royal Observatory from its present unfortunate position at the Cape. There is a touch of irony in a situation which permits the best-equipped observatory in South Africa to remain on the Flats below Table Mountain, while the recognised advantages of the High Veldt are attracting astronomers from all quarters of the world.

PHYSICS. By L. F. BATES, B.Sc., Ph.D., F.Inst.P., University College, London.

New Investigations of α-Particles.—At the last meeting of the British Association in Bristol, Sir Ernest Rutherford described a new and powerful method for the analysis of α-rays. Two methods of investigation have recently been pressed forward. In France, Rosenblum (*Compt. Rend.*, **188**, p. 1401, 1929, and **190**, p. 19, 1930) has used a large electromagnet, designed by Cotton, to bend α-rays into semicircular paths of about 35 cm. diameter. In this way he obtained excellent photographs, which showed that the 4·8 cm. α-rays from ThC possessed a fine structure, and were therefore more complicated than had previously been supposed. The other line of attack has resulted from improvements of the Geiger counter, following Greinacher's demonstration (*Zeit. für Phys.*, **36**, p. 364, 1926, and **44**, p. 319, 1927), that the ionisation current produced by an α-particle passing through the counter-chamber might be amplified by thermionic valve arrangements. Geiger and Klemperer (*Zeit. für Phys.*, **49**, p. 753, 1928) so modified the arrangements that the amplification was linear. Recently, H. Fränz and W. Bothe (*Zeit. für Phys.*, **63**, pp. 370 and 381, 1930) have used this modification in an investigation of the disintegration products of boron exposed to α-rays from RaC′ and from Po, whereby they proved the existence of three distinct groups of H particles in the disintegration products. It will be noted that the Rosenblum method possesses the advantage that it is practically uninfluenced by the heavy β and γ radiations from any of the sources employed. Now in the experiments of Fränz and those described by Sir Ernest Rutherford, a powerful γ radiation was always present, and therefore special attention had to be paid to the design of the amplifying and recording systems. The systems in use at the Cavendish were designed by Wynn-Williams and Ward.

Briefly stated, the problem which confronted them was that of collecting the ions in the Geiger counter within a thousandth of a second, and amplifying and recording the charge thus collected in a time interval somewhat less than the time for collection. Hence, a special oscillograph which permitted the counting of many hundreds of α-particles per minute was necessary. One counting chamber actually consisted of a 2-mm. gap between a thinly sputtered film on mica and a plate electrode, between which a potential difference of 240 volts was maintained. This type of chamber could not be used without ambiguity for the analysis of a complex group of α-rays, because, in general, two different portions of the range of the same ray would give equal ionisations inside the chamber. This ambiguity was eliminated by using a differential chamber, which was a double ionisation chamber formed by a plate and two equally spaced metal foils. The space between the two foils formed an anterior chamber, and that between the plate and one foil formed a posterior chamber. The outer foil was connected to a source of potential of + 240 volts and the plate to − 240 volts, whilst the remaining foil was connected to the amplifier. Thus the collecting foil received a positive or a negative charge when an α-ray passed through the system, according to the particular portions of the path described in the two chambers. Naturally, if an α-ray stopped in the anterior chamber the total charge collected was positive. This differential method permitted the examination of beams of α-rays of as many as 100,000 per minute for the presence of rays of shorter range than that characteristic of the main beam. It was found that the 8·6 cm. particles from ThC′, the 7·0 cm. particles from RaC′, and the 3·9 cm. particles from Po formed homogeneous groups. The 5·5 cm. particles from Actinum C, however, were found to consist of two well-marked groups differing in range by 4·2 mm. The 4·8 cm. particles from ThC were found to be a complex group, as previously shown by Rosenblum. It is of interest to recall that for many years Sir Ernest has predicted the existence of a beam of α-rays from RaC of range about 4 cm. and present in number to an extent of one 4 cm. ray to several thousands of the 7·0 cm. rays. It has now been found by the differential method that there are two such groups, a main group of range 4·1 cm. and a smaller group of range 3·9 cm.

Ionic Mobilities.—At Bristol the members of the British Association also had an opportunity of inspecting the apparatus used by Prof. Tyndall and his collaborators in the measurement of ionic mobilities. A great deal of work has been done on gaseous ions during the last thirty years—for it was at the Bristol Meeting of the Association in 1898 that A. P. Chattock

first described his measurement of the mobility of a gaseous ion. Yet the nature of these ions still remains obscure. The work of Tyndall has shown that in the case of negative ions, large effects are produced by impurities in the gas in which these ions are formed. For, if a negative ion starts its life as an electron, it may speedily attach itself to a molecule of water-vapour or of carbon dioxide released from the walls and seals of the vessel in which the gas is contained. The effect of impurities in the case of positive ions appears to be even more pronounced. In fact, the results obtained with the latest apparatus (*Proc. Roy. Soc.*, **129**, p. 162, 1930) may be regarded as iconoclastic, for they indicate that all previous data on the mobilities of positive ions are almost valueless. The apparatus is so designed that ions of different mobilities may be resolved into their respective groups. It is constructed entirely of glass, so that it may be thoroughly baked out prior to the introduction of pure gas. In this apparatus four gauzes are separated by successive intervals of 2, 25, and 2 mm. Ions are brought up to the first gauze, and an alternating electric field of known frequency is set up between the first two gauzes, so that once in every cycle a flash of ionisation enters the space between the second and third gauzes. Between the latter a constant accelerating field is applied. When the ions reach the third gauze, the possibility of entering the space between the third and fourth gauzes, and so of passing on to the collecting electrode, depends on the potential difference established between these gauzes. This alternates in phase with that between the first two gauzes, so that, for certain values of the frequency, ions of a specified mobility arrive at the third gauze at the beginning of the accelerating or advancing phase of the potential, and some of them escape the gauzes and reach the collector electrode. Hence, on plotting the current collected by the electrode against the frequency of the alternating potential, a continuous curve with a single peak is obtained, when ions of only one specified mobility are present. To test the behaviour of the apparatus, the variation of mobility with pressure was investigated, and Langevin's law was found to hold exactly. With nitrogen all the negative ions were found to be electrons. This has generally been accepted as a test of the purity of the gas, but the experiments on positive ions showed that the test is not sufficient, for the results with positive ions were variable. This fact was even more strongly emphasised when helium was used. Here a main group of ions of mobility 14, and indications of a group with mobility 17 were recorded, although previous observers only report maximum mobilities of the order of 7 cm. per sec. per volt cm. The slight contamination of the gas, which occurred on allowing the apparatus

to stand for some time, caused the ions to become a mixture of extended mobility with a maximum at 10. Removal of the liquid air from the traps on the apparatus produced a still further decrease in the mobility. It should be noted that the maximum value, 17, approaches the classical value deduced on the kinetic theory for the mobility of the positive helium on moving through helium. The changes produced by impurities are best understood by considering the possible results of the collision between a positive ion and a foreign atom which possesses a dipole structure, or any atom or molecule of lower ionisation potential (cf. SCIENCE PROGRESS, and Smyth and Stueckelberg, *Phys. Rev.*, **32**, p. 779, 1928, and Kallmann and Rosen, *Zeit. für Phys.*, **61**, p. 61, 1930). In the first case the result is obvious. In the second the ion may capture an electron from the atom or molecule with which it collides, the probability of capture being the greater the smaller the energy transfer involved in the collision. If the colliding atom or molecule be present as an impurity, it is unlikely that it will regain the electron thus lost. Hence, even minute traces of non-polar impurities may give rise to false values of the mobility. In addition to these sources of error, metastable atoms may be produced, and these, colliding with atoms of the impurity, may give rise to ions of the impurity by virtue of collisions of the second kind. Tyndall therefore concludes that, unless the impurity is reduced to the order of a few parts in a million, the measurements of mobility are not trustworthy. Even the value, 17, obtained for the maximum mobility of a helium ion is not regarded as final, although it is approximately three times that previously recorded by other observers. The experiments are being continued with even greater precautions to ensure purity of the gases investigated, and we may now expect that the nature of the positive ion will definitely be determined.

In connection with the effects of impurities, Townsend (cf. *Proc. Roy. Soc.*, **124**, p. 535, 1929) has shown that it is impossible to obtain consistent results in the measurement of the conductivity of helium unless the gas is extremely pure. He found that the ordinary spectroscopic method of detecting impurities was not an adequate test, and that it was necessary to adopt a much more sensitive test depending on the sparking potential, which indicates to what extent currents may be affected by impurities.

American Contributions.—A contribution of general interest is that of R. S. Mulliken (*Phys. Rev.*, **36**, p. 611, 1930), who presents a report on the notation recommended with respect to the symbols used for the quantum numbers which describe the spectra of diatomic molecules. This notation has been

approved by a large number of workers. Unfortunately, the report cannot be discussed here.

According to the theory of the isotope effect in band spectra, a molecule which contains a rarer isotope of one of the atoms of which the ordinary molecule is formed, emits a band exactly corresponding to, but displaced from, the band emitted by the ordinary molecule. The amount of the displacement may be calculated, remembering that it may be due partly to vibration and partly to rotation. The measurement of the displacement is, however, best tackled by measuring the absorption spectrum, for, by making the column of the absorbing gas sufficiently long, it is possible to work with the gas at a very low pressure and thus avoid diffuseness of the absorption lines. Such experiments have recently been carried out by Laudé (*Phys. Rev.*, **36**, p. 333, 1930), in order to confirm the existence of the oxygen isotopes of masses 17 and 18, previously discovered by Giauque and Johnston (*Nature*, **123**, pp. 318 and 831, 1929), and the existence of an isotope of nitrogen of mass 15. For this purpose, the absorption spectrum of the NO γ-bands, particularly those at wave-lengths 2,269, 2,154, and 2,052, were examined. A hydrogen continuous source fitted with a crystal quartz window supplied light to an absorption tube, 90 cm. long, filled with gas. In all three cases band heads were observed, corresponding to the four types of molecules, $N^{14}\ O^{16}$, $N^{15}\ O^{16}$, $N^{14}\ O^{18}$, and $N^{14}\ O^{17}$, *i.e.* the presence of the three isotopes mentioned above was confirmed. Now Babcock (*Phys. Rev.*, **34**, p. 540, 1929) took an absorption picture of the atmospheric oxygen absorption bands, using the sun as the continuous light source. In this way he photographed the head of the displaced bands due to the $O^{16}\ O^{18}$ molecule. On the same plate he also obtained a photograph of the heads of the corresponding bands due to the O_2^{16} molecule, using a column of air in the laboratory to produce the absorption. When the band heads in the two pictures appeared to be of the same intensity, he considered that the abundance of the two types of molecules should be inversely proportional to the number of molecules traversed in the two cases, which could, of course, be calculated. He thus found that one O^{18} atom was present to every 1,250 O^{16} atoms, the limits of experimental error being rather large. This abundance factor was found by Laudé by a comparison of the intensity of the $N^{14}\ O^{18}$ band head at approximately 2,155, with the $N^{14}\ O^{16}$ band head at approximately 2,158. Two pictures were taken on the same plate when the absorption tube was filled with a mixture of NO and N_2, the total pressure in the tube being kept constant. For the photograph of the $N^{14}\ O^{18}$ band head, the amount of NO present was very large compared with that present when the photograph of the

N^{14} O^{16} band head was obtained, and from the ratio of the pressures of NO present in the two cases for equal intensities of the corresponding band heads on the photographic plate, the abundance ratio was found to be 1,075 ± 110. The abundance ratio of N^{14} to N^{15} was also found to be 700 ± 140 from an examination of the N^{15} O^{16}, N^{14} O^{16}, and N^{14} O^{18} band heads.

An interesting experiment on ionisation potentials is described by A. L. Hughes and C. M. Van Atta (*Phys. Rev.*, **36**, p. 214, 1930). Using the Hertz method of the neutralisation of space charge by positive ions, they found an abrupt increase in the gradient of the ionisation curve for potassium vapour, indicating a second ionisation potential at 0·97 ± 0·05 volt above the first one, which corresponds to the series limit. They consider that the experiment provides evidence of a second type of ionisation in the potassium atom for which they are unable to suggest any theory. Similar discontinuities in the ionisation curve of mercury vapour were previously found by Lawrence (*Phys. Rev.*, **28**, p. 947, 1926) at 0·20, 0·89, 1·30, and 1·66 volts above the ionisation potential corresponding to the series limit of mercury.

It has frequently been stated that both the modified and the unmodified X-radiation scattered by light substances, such as graphite and aluminium, possesses a fine structure, and consists of a group of lines very close together (cf. Davis and his collaborators, *Phys. Rev.*, **31**, p. 1119, 1928; **32**, p. 331, 1928; **33**, p. 871, 1929, and **34**, p. 1, 1929). The relative intensities and positions of these lines in the modified and unmodified radiation were stated to be similar. Two investigations of the scattered radiation have recently been made. In both cases the investigation was made by means of a double crystal X-ray spectrometer of high resolving power. J. A. Bearden (*Phys. Rev.*, **36**, p. 791, 1930) used the K radiation of copper and silver, and although his spectrometer was ten times as sensitive as that of Davis, he found no trace of fine structure in the unmodified scattered radiation. He noted, however, that the modified line was about twice as broad as one would have expected from the divergence of the angle of scattering. In the second investigation, N. S. Gingrich (*Phys. Rev.*, **36**, p. 1049, 1930), used X-radiation from a molybdenum target, and observed the scattered radiation from graphite at angles of 109° and 161°, and again no evidence of fine structure was obtained. Bearden's scattering block was actually mounted inside the X-ray tube, and his results have been criticised on the ground that a slight deposit of sputtered target material on the block would produce relatively strong fluorescent radiation, and thus mask fine

structure. However, Gingrich's arrangement was not open to this criticism.

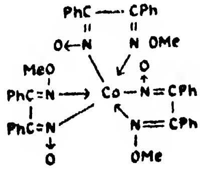
An experimental determination of the change in temperature which accompanies change in magnetisation in iron is of great interest. Ellwood (*Phys. Rev.*, **36**, p. 1066, 1930) used specimens of carbon steel containing 1·08 and 1·35 per cent. of carbon. His test specimen consisted of 104 bars of steel mounted on the surfaces of seven concentric coaxial cylinders, the lengths of the bars in the several cylinders being so adjusted as to give the complete arrangement the form of an ellipsoid of revolutions. An equal number of copper bars of similar diameter and length were mounted between the iron bars, the ends of these bars being appropriately connected to the ends of the steel bars by short lengths of copper and of constantan wire to form 102 thermocouples in series. The whole was mounted within a specially designed calorimeter inside a solenoid. As an example of the results obtained we may quote the details of a symmetrical cycle starting with the steel in a saturated condition. As the magnetising field is diminished from + 290 to + 20 gauss, the temperature of the steel rises gradually ; this is followed by a very marked cooling between + 20 and − 8 gauss ; if the magnetising field is further diminished from − 8 to − 90 gauss a sudden rise in temperature indicates the release in this region of the major portion of the hysteretic energy ; between − 90 and − 200 gauss the steel cools gradually. These cooling effects have not been previously reported, and the evidence is not sufficiently conclusive to say that the effects are a general property of all steels.

F. Bitter (*Phys. Rev.*, **36**, p. 1572, 1930) describes an extensive set of experiments on the variation of the magnetic susceptibility of gases with pressure. His apparatus is distinguished by the very symmetrical design of the glass test piece, which is rotated under the action of a non-uniform magnetic field, as in the method described by Glaser and dealt with in this section some time ago. He shows that the Glaser anomaly is due to the presence of water vapour and not to thermal effects and ionisation. We still have, however, no satisfactory picture of the mode of action of the water vapour.

ORGANIC CHEMISTRY. By J. N. E. Day, M.Sc., A.I.C., University College, London.

Brady and Muers (*J.C.S.*, 1930, 1599) discuss the structure of the metallic complexes of oximes. Of the three known O-monomethyl ethers of benzildioxime, only O-monomethyl-α-benzildioxime gives these complexes, of the type R_3Co and

R_2Ni. In the case of the cobalt complex, these authors suggest the structure :



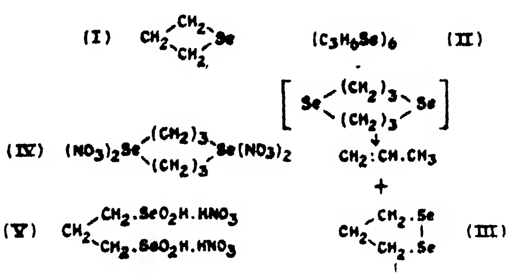
The above paper may be consulted for a summary of, and references to, other work on this subject.

In connection with the structure of compounds containing bivalent carbon, Sidgwick and co-workers (*J.C.S.*, 1930, 1876), from the measurement of the parachors and the dipole moments of the *iso*cyanides have now obtained the evidence required in favour of the formula proposed by Langmuir for these compounds and carbon monoxide :

$$R—N \overset{\rightarrow}{=} C \text{ and } O \overset{\rightarrow}{\equiv} C,$$

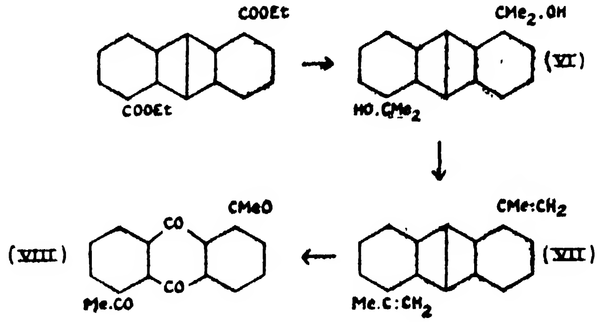
previously considered to be R—N = C and C = O.

Morgan and Burstall, in continuation of previous work (see this Journal, 1930, **25**, 12) have now prepared *cyclo*selenopropane (I) (*J.C.S.*, 1930, 1497) by the reaction of trimethylene dibromide and sodium selenide, the main product being a polymeride (II) ; this latter on heating gave propylene and *cyclo*trimethylene diselenide (III). Dilute nitric acid converts the polymeride to the nitrate (IV) ; hot concentrated nitric acid gives trimethylenediselenious acid dinitrate (V), which is also obtained from the *cyclo*trimethylene diselenide.



Coulson (*J.C.S.*, 1930, 1931) has continued the work of Morgan and Coulson (see this Journal, 1930, **25**, 12) on anthracene derivatives. 1 : 5-Dichloroanthraquinone was converted through the dicyano compound to anthraquinone-1 : 5-dicarboxylic acid, and this was reduced to anthracene-1 : 5-dicarboxylic acid. The ethyl ester with methyl magnesium iodide gave 1 : 5-di*iso*propylolanthracene (VI). Boiling this

latter compound in acetic acid converted it to 1 : 5-di*iso*propenylanthracene (VII); this was oxidised with chromium trioxide in acetic acid to 1 : 5-diacetylanthraquinone (VIII).



Further work is reported (Hickinbottom and Waine, Hickinbottom and Preston, *J.C.S.*, 1930, 1558, 1566) on the rearrangement of alkylanilines to aminoalkylbenzenes (by heating with anhydrous metallic halides) in an endeavour to work out the course of the reaction. The following two possibilities were examined with negative results :

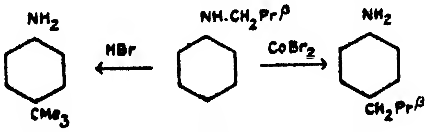
I. Ph·NH·Alk → $Ph{\cdot}NH_2 + C_nH_{2n}$ → Alk· $C_6H_4{\cdot}NH_2$.

This assumes the intermediate formation of an olefin. If this were correct it might be expected that the alkyl groups would isomerise to some extent during the reaction, but it was found that such groups as *n*-butyl-, *n*-propyl-, retained their configurations, *e.g.* *n*-butylaniline gave *p*-amino-*n*-butylbenzene. Attempts were also made, by passing a stream of nitrogen through the reacting mixture at 210°–250°, to remove any volatile intermediate compound, but the conversion into the *p*-aminoalkylbenzene was not affected.

II. The second case considered requires the formation of hydrogen halide by reaction of the metallic halide with water.

$$2Ph{\cdot}NHR + CoCl_2 + H_2O \rightarrow 2Ph{\cdot}NHR,\ HCl + CoO \rightarrow CoO + 2R\cdot C_6H_4\cdot NH_2, HCl.$$

In this case also attempts to remove volatile intermediate products with a stream of nitrogen were unsuccessful.



(XI) (IX) (X)

It was also found that *iso*-butylaniline hydrobromide (IX), heated in a sealed tube, gave *p*-amino-*tert*-butylbenzene (XI);

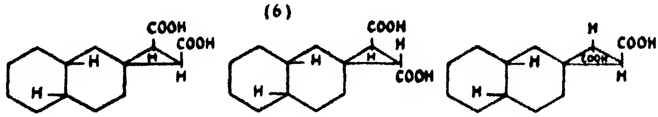
when dry *iso*-butylaniline was heated with cobalt bromide it gave *p*-amino-*iso*-butylbenzene (X). Thus it is evident that this latter reaction is not due to formation of hydrogen halide, liberated by traces of moisture on the metallic bromide, or in this case also, the compound (XI) would have been expected.

Reference may be made to a paper by Rao (*J.C.S.*, 1930, 1162) on "Strainless Rings," in which the ease of formation and stability of derivatives of decahydronaphthalene (*trans*-decalin-2 : 2-diacetic acid) (XII) is considered. It was found that these compounds behave as if the ring were nearly strainless, occupying a position between *cyclo*pentane and *cyclo*heptane.

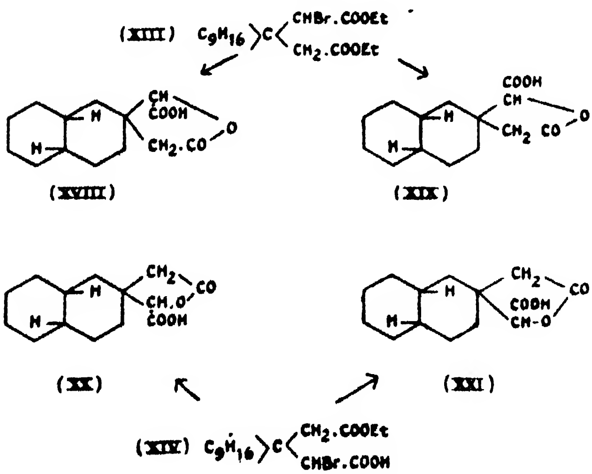
$$\text{(XII)}\quad C_9H_{16}{>}C{<}\begin{matrix}CH_2\cdot COOH\\ CH_2\cdot COOH\end{matrix}$$

From the hydrolysis of the neutral (XIII) and acid (XIV) monobromo esters with potassium hydroxide three of the four possible *trans*-decalin-β-spiro-*cyclo*-propane-1 : 2-dicarboxylic acids have been prepared (XV, XVI, XVII). Both trans-acids are decomposed by heating with hydrochloric acid under conditions which will not decompose the cyclohexane analogue. When treated with boiling sodium carbonate solution the stereoisomeric lactones of α-hydroxy-*trans*-decalin-2 : 2-diacetic acid were obtained (XVIII, XIX, XX, XXI).

(6)



(XV) (XVI) (XVII)



BIOCHEMISTRY. By P. EGGLETON, D.Sc., The University, Edinburgh.

Enzyme Chemistry.—Enzyme chemistry is one of the most difficult parts of biochemistry, and advances are infrequent. A year ago we had occasion to report the preparation of pure crystals of urease by Abel—the first pure enzyme preparation achieved. This has been followed now by the isolation of crystalline pepsin (J. H. Northrup, *Science*, **69**, 580, 1929); Northrup dialysed under pressure a potent commercial preparation of pepsin, and obtained small hexagonal crystals, containing 14·5 per cent. nitrogen. The molecular weight of the preparation, calculated from its rate of diffusion, was about 10,000, much smaller than a protein molecule, suggesting rather a peptone. The preparation attacked gelatin, casein, egg albumin, and edestin, and was rapidly inactivated by heat and by alkalis. Confirmation and extension of this important work will be awaited eagerly.

Two papers representing useful advances have appeared this year, dealing with the heat inactivation of trypsin and lipase respectively (J. Pace, *Biochemical Journal*, **24**, 606, 1930; I. H. McGillivray, *ibid.*, **24**, 891, 1930). Both enzymes, when freed from precursors and other enzymes, were found to be inactivated at a velocity characteristic of a monomolecular reaction. Trypsin showed greatest heat stability at *p*H 6·5, the resistance to heat decreasing at greater or less hydrogen ion concentrations in a manner reminiscent of the denaturation of a protein. The critical increment of heat inactivation proved to be 40,000 calories (for all values of *p*H), a value rather low for a denaturation process, but certainly too high for a hydrolysis reaction. Lipase showed similar behaviour, with optimum stability at *p*H 6·0 and a critical increment of 46,000 calories.

Lipase preparations are not very specific in their action: they will attack even esters of aromatic alcohols and acids, in addition to those of the aliphatic alcohols and non-fatty aliphatic acids, but when the substrate provided is not the glyceryl ester of a fatty acid, it is usually safer to employ the term esterase. In an attempt to measure the degree of stereochemical specificity of sheep liver esterase, Murray and King (*Biochemical Journal*, **24**, 190, 1930) compared the dextro- and lævo-isomers of certain optically active alcohols in respect of their ability to inhibit the hydrolysis of ethyl butyrate by the enzyme. The alcohols were methyl-*n*-hexyl carbinol, methyl-phenyl carbinol, and methyl-β-phenyl-ethyl carbinol. In all cases the lævo-isomer had four to five times greater inhibitory effect. The curious observation was made, however, that the esterase of rabbit liver showed no such ability to distinguish between these optical isomers.

A valuable addition has been made to the armoury of techniques available to enzyme chemists by Cannan and Muntwyler (*Biochemical Journal*, **24**, 1012, 1930), who followed the digestion of gelatin by pepsin in hydrochloric acid with full electrotitration at frequent intervals, supplemented by the usual tests of viscosity, alcohol solubility, and the like. By subtracting successive curves, the titration curve of the digestion products was obtained—on the reasonable assumption that the comparatively slight degree of hydrolysis had not altered the strengths of the titratable groups initially present. The resulting curve agreed remarkably well with the theoretical titration curve of a polypeptide having titration constants pK_1 3·5 and pK_2 7·9. These figures are very close to the titration constants of several synthetic tetrapeptides (*e.g.* glycyl-alanyl-alanyl-glycine, 3·3, 7·9). Since no excess of acid groups over basic was produced at any stage of the digestion, it is clear that pepsin acts (on gelatin, at least) only by hydrolysing peptide (–NH–CO–) linkages. Earlier reports to the contrary are shown to have been due to the use of faulty methods for determining such groups.

"pH" and "rH."—The acidity (or alkalinity) of a solution is fully defined by a capacity factor (normality) and an intensity factor (pH), the latter being measured by the E.M.F. set up at a hydrogen electrode. The oxidising or reducing power has similarly a capacity factor and an intensity factor (rH), the latter also measured by an electromotive force; but since this intensity factor is affected by the degree of acidity or alkalinity of the solution, the measurement of "redox" (reduction-oxidation) potentials is a more complicated proceeding.

In the simpler subject of pH measurements we have to report an advance in technique introduced by Anson and Mirsky (*Journal of Biological Chemistry*, **81**, 581, 1929), who, while paying tribute to the pioneer work of Dr. Kerridge, have introduced some modifications into the design of her glass electrode, with the object of simplifying its construction and use. The main alteration is in the electrode itself, which is more easily made, and contains in itself one of the necessary calomel half cells.[1]

Drs. Kirk and Schmidt and collaborators have continued their determinations of the titration constants of amino acids. Their redetermined figures are as follows:

[1] Dr. Kerridge's electrode was designed for use in English climatic conditions: some of the modifications introduced by these authors are perhaps better regarded as adaptations to the climatic conditions of the American continent.

	pK_{a_1}.	pK_{b_1}.	pK_{b_2}.	
Tryptophane (30°) . .	9·39	11·62	—	*Journal of Biological Chemistry*, **85**, 137, 1929.
Histidine (30°) . .	9·17	8·00	12·18	
Histidine (0°) . .	9·75	8·44	13·04	*Journal of Biological Chemistry*, **88**, 285, 1930.
Lysine (0°) . .	11·31	5·13	12·74	
Lysine (25°) . .	10·53	5·05	11·82	
Arginine (0°) . .	13·31	5·15	12·70	
Arginine (25°) . .	12·48	4·96	11·98	

The constants are given here in logarithmic form, recalculated from the authors' figures.

The constants of arginine and histidine have also been redetermined by L. J. Harris and T. W. Birch (*Biochemical Journal*, **24**, 564, 1930). They find, at 23° C., inflexions in the titrations curves at the following pH values :

	Carboxyl.	Iminazole.	Amino.	Guanidine.
Arginine . .	2·18	—	9·09	13·0
Histidine . .	1·78	5·97	8·97	—

It will be seen that the authors allocate the constants differently from Kirk and Schmidt, but there is good numerical agreement. The method used to allocate the points of inflection in the titration curves to the particular groups is interesting. In the case of arginine, the constant at 9·09 was found to be shifted in the direction of diminished basicity when the titration was performed in the presence of formaldehyde (which attacks amino groups), whilst that at 2·18 was not affected. The former constant was therefore assigned to the amino group and the latter to the carboxyl. This left the third constant (at pH 13·0) to be assigned to the guanidine group. Titrations of guanidine itself revealed a constant in the same neighbourhood. An alternative proof of this assignment was provided by titrations at different temperatures. The pK value of an acid changes a little with temperature, but that of a base will change more, for it is the difference between the true basic constant and the dissociation constant of water, which changes considerably with the temperature. By measuring all pK values at two temperatures and applying the above line of reasoning, a probable allocation is obtained. This method led to the same conclusions as the first method.

A figure that may assume importance presently in connection with carbohydrate metabolism and fermentation is the third titration constant of orthophosphoric acid. This has been redetermined recently by Kugelmass (*Biochemical Journal*, **23**, 587, 1929). At 20° and at 38° his values are 11·99 and 11·83 respectively for pK'_3.

Reference was made in the last of this series of reviews to the unexpected developments in the chemistry of glutathione, leading to the adoption of a tripeptide formula. The

titration constants of pure reduced glutathione (isolated by Hopkins' new method) have been obtained by Pirie and Pinhey (*Journal of Biological Chemistry*, **84**, 321, 1929); they find one sulphydryl group of pK 9·62, one amino group of pK 8·66, and two carboxyl groups of pK 2·12 and 3·53 respectively.

In the newer and almost unexplored field of reduction potentials, Michaelis and Eagle (*Journal of Biological Chemistry*, **87**, 713, 1930) have described three new "redox" indicators: Gallocyanine, a useful substitute for methylene blue; Gallophenine, a substitute for indigo monosulphonate, having considerably greater solubility; and Brilliant Alizarin Blue, an indicator covering a more negative range than any of the indigo sulphonates.

Much work has appeared recently on the reduction potentials of bacterial cultures and culture media. It will be remembered that Gillespie, who first drew attention to the existence of reduction potentials, was himself working with bacterial cultures. Early work on this subject was largely vitiated by an incomplete understanding of the conditions under which potential measurements of any significance could be made. Ideally the system should be reversible and at equilibrium in order to yield any real information.

Bacterial culture media kept under some standard oxygen pressure (usually zero) register a potential which drifts in the course of hours or days to some definite and reproducible negative potential, apparently characteristic of the medium, since different types of electrodes give the same values. Work on this subject has appeared from C. B. Coulter (*Journal of General Physiology*, **12**, 139, 1928; *ibid.*, **10**, 197, 1929), P. Fildes (*British Journal of Experimental Pathology*, **10**, 151, 1929), R. Dubos (*Journal of Experimental Medicine*, **49**, 507, 1929 *et seq.*; *ibid.*, **50**, 143), Hewitt (*Biochemical Journal*, **24**, 512, 1930), B. C. J. G. Knight (*Biochemical Journal*, **24**, 1066 and 1075, 1930), and others.

Coulter showed that bouillon samples kept in absence of oxygen registered potentials which drifted in the negative direction to a steady state at − 50 to − 60 millivolts (pH 7·6). Knight has extended the results of Coulter, notably by observing the effect of varying the pH of the culture media. He also introduced the thermionic valve potentiometer of D. T. Harris (*Journal of Scientific Instruments*, **5**, 161, 1928), which has the enormous advantage of passing no current whatever, thus avoiding polarisation of even the most poorly poised systems. When the reduction potential finally reached was plotted against the pH of the broth, the curve obtained resembled that given by any reversible system at a fixed

degree of reduction, in its relation to *p*H. There were two well-marked inflections in the curve, suggesting hydrion dissociation constants at *p*H 6·6 and 7·65. Knight also describes a method for maintaining any desired reduction potential in bacterial cultures by varying the oxygen partial pressure in the gas mixture bubbling through the culture.

A contribution has been made by Michaelis and Flexner (*Journal of Biological Chemistry*, **79**, 689, 1928) to the study of the curious behaviour of the cysteine-cystine system. It will be remembered that Dixon and Quastel discovered that the potential of the cysteine-cystine system was independent of the concentration of cystine—a perplexing exception to the usual conditions of equilibrium for balanced reactions. Michaelis and Flexner have confirmed their finding, and express the potential of the system as—

$$E_h(\text{volts}) = -0{\cdot}001 - \frac{RT}{F}\log[\text{cysteine}] + \frac{RT}{F}\log[H^{\cdot}].$$

They were not in agreement with the views of Dixon and Quastel on this phenomenon, but were unable to suggest a plausible explanation.

Hexosephosphates and phosphatases.—Since Harden and Young demonstrated in 1906 the formation of a hexose-diphosphoric ester in the course of yeast fermentation, the phosphoric esters of sugars have been discovered in a variety of situations, and found to have several distinct uses in living organisms. The original ester of the group has been shown by Robison and Morgan to be fructose-1-6-diphosphoric ester (*Biochemical Journal*, **22**, 1270, 1928), and simultaneously Levene and Raymond (*Journal of Biological Chemistry*, **80**, 633, 1928) showed that the hydrolysis rate of the methyl glucoside was in agreement with the same conclusion, though their evidence was not sufficient to form a proof. It has been found that this same ester accumulates in minced muscle tissue in certain conditions, and also a second diphosphoric ester of different solubility and hydrolytic constant (Meyerhof, lectures at University College, London, March 1930). But in normal muscles only a hexose-monophosphoric ester is found, or rather a mixture of two such esters, one a fructose derivative and the other a glucose derivative (Pryde and Waters, *Biochemical Journal*, **23**, 573, 1929). Such a mixture of esters had been found in yeast fermentation liquors by Robison, who has since shown them to be 6-hexosephosphoric esters, and lately Robison and Morgan have discovered yet another ester from the same source, an ester which on acid hydrolysis yielded glucose, but which on more gentle hydrolysis by means of Robison's "bone

phosphatase" yielded trehalose (R. Robison and W. T. J. Morgan, *Biochemical Journal*, **22**, 1277, 1928). These authors have now published details of their methods of separating the esters of yeast fermentation mixtures (*Biochemical Journal*, **24**, 119, 1930).

The "bone phosphatase" which Robison uses in the examination of these esters is the enzyme discovered by him in 1923 (R. Robison, *Biochemical Journal*, **17**, 286, 1923). He published an improved method of preparation of the enzyme last year (M. Martland and R. Robison, *Biochemical Journal*, **23**, 237, 1929). Bones of young rabbits are extracted with chloroform water for 7–10 days. The extract is precipitated with alcohol and ether, and the precipitate extracted with 50 per cent. alcohol. If this process is repeated the product is soluble in water and highly potent. It attacks sodium salts best at pH 7·0 and barium salts at pH 8·6.

There is no doubt that this enzyme plays a most important part in bone formation, enabling the growing bone to deposit insoluble calcium phosphate from the soluble calcium hexose-monophosphate brought to it by the circulating blood. But it may serve as a general agent for dephosphorylating: Kay showed the phosphatase power of various tissues to fall into the same order as their "pyrophosphatase" potency, making it probable that the same enzyme is concerned. Extracts of growing bone headed the list in both series (Kay, *Biochemical Journal*, **22**, 1446, 1928). Neuberg and Jacobsohn (*Biochemische Zeitschrift*, **199**, 498, 1928) found phosphatase preparations able to attack such wholly unnatural esters as potassium methyl-propyl-carbinol-phosphoric acid, cholesterylphosphoric ester, and the pyrophosphoric esters of ortho- and metacresol and α-naphthol. Conversely, M. Vogt reports that synthetic glyceric acid-monophosphoric ester is hydrolysed by phosphatases from a number of different sources (*Biochemische Zeitschrift*, **211**, 1, 1929).

PHYSICAL CHEMISTRY. By R. K. Schofield, M.A., Ph.D. (Cantab.), Rothamsted Experimental Station, Harpenden.

Contact Catalysis.—As is aptly pointed out by J. C. W. Frazer in a recent article (*J. Phys. Chem.*, 1930, **34**, 2129), "Any real explanation of the mechanism of catalysis must go back to the nature of the chemical bond and the interplay of energies that are involved in the making and breaking of such union." At least two types of bonds are now recognised, namely, the heteropolar or electrovalent bond, and the homopolar or covalent bond.

The heteropolar bond received at least a partial explanation

on the older quantum theory, according to which it was the electrostatic attraction resulting from the migration of an outer electron of a metallic atom to the incompleted shell of a non-metallic atom. No clear case has yet been established in which contact catalysis is necessary for the making or breaking of a heteropolar bond, so that further consideration of this type of linkage is unnecessary in this connection.

The development of wave mechanics has opened up an entirely new approach to the study of the homopolar bond. The physical chemist is here placed in some difficulty. A realisation that it is not, as yet, possible to describe wave mechanics in terms of classical thought inclines one who has not a first-hand acquaintance with this new mathematical method to refrain from plunging in. On the other hand, he cannot afford to neglect a line of approach which appears likely to throw light on the central problem of physical chemistry. Consequently, it is permissible, even when there is the certainty of some inexactness of treatment, to attempt a general account of this new line of approach.

It was G. N. Lewis who first pointed out that two atoms when linked by a homopolar bond appeared to share two electrons. On the classical theory, the sharing of electrons between two atoms could be explained only with the help of additional and very arbitrary assumptions. The stability of the hydrogen molecule, for instance, was exceedingly difficult to account for on the classical theory, as the spontaneous migration of an electron from one atom to the other appeared at variance with the principle of the conservation of energy. This principle has only a statistical application in the new mechanics, and such an exchange is now seen to have a small but finite probability. It further emerges from the new mechanics that exchange leads to binding, the strength of the bond increasing with the probability of exchange. It is important to realise that no *extension* of the new mechanics is involved here, but that binding by exchange is a necessary consequence of the fundamental postulates on which it rests, and that it would be a very serious blow to the whole system if binding by exchange did not take place in nature.

London (*Naturwiss*, 1929, **17**, 516) has shown that the solution of the wave equation gives two possible values for the exchange energy of two atoms, one of which indicates attraction and the other repulsion. The ambiguity arises because the exchange energy is made up of two terms, one of which is due to coulomb forces, and the other, known as the resonance energy (which has no classical analogy), is either positive or negative (London, *Z. Electrochemie*, 1929, **35**, 552). The magnitude of the exchange energy depends

upon the distance between the atoms. The same holds good for the approach of another atom to a molecule. The atom (which may be part of another molecule) will in general first be repelled, but may, on closer approach, be attracted ; while the resonance energy of the molecule may simultaneously be reduced or even altered in sign. This line of reasoning leads to the further result that, in general, the more widely separated the atoms of the molecule, the less will be the repulsive force exerted on the third atom and the more likely a disruption of the molecule due to its closer approach. This result is of great importance for the theory of catalysis, as it suggests a possible way in which adsorption on to a solid surface may decrease the energy of activation, namely, by stretching the molecule.

A recent investigation by Aborn and Davidson (*J. Phys. Chem.*, 1930, **34**, 422) is particularly interesting in this connection. An X-ray examination was made of Cu-ZnO catalysts used to decompose methyl alcohol. A series, in which the composition varied from 100 per cent. of one to 100 per cent. of the other, was examined, and it was found that while the crystalline form remained the same throughout, the *size* of the lattice varied with the composition. These catalysts are remarkable for their selectivity, for whereas ZnO decomposes methyl alcohol to CO and H_2, Cu converts it into methyl formate. A comparison of (*a*) the curve-connecting length of the ZnO lattice unit and composition, and (*b*) the curve-connecting percentage of CO produced and composition shows that a sharp break in one corresponds with an equally sharp break in the other. Similarly, a close relationship is found between the size of the Cu lattice and the percentage of methyl formate produced.

It has been realised for some years that all parts of the surface of a catalyst do not promote a given chemical reaction to the same degree. For instance, Pease and Stewart (*J. Amer. Chem. Soc.*, 1925, **47**, 1235) found that catalytic activity can be inhibited by an amount of CO much smaller than that required to form a monomolecular layer. The proportion of the surface which is active depends upon the reaction catalysed. Vavon and Huson (*Compt. Rend.*, 1927, **175**, 266) were able so to adjust the amounts of various sulphur compounds that a platinum catalyst was poisoned for some hydrogenation reactions but not for others. Schwab and Pietsch (*Z. Electrochemie*, 1929, **35**, 573 ; *Z. phys. Chem.*, 1929, **B1**, 385, and **B2**, 265) support the idea that has been championed by H. S. Taylor (*Proc. Roy. Soc. A.*, 1925, **108**, 105, and *J. Phys. Chem.*, 1926, **30**, 142) that the " active spots " may be identified with the corners and edges of crystals by showing that the idea is not in conflict with the kinetic equations deduced by Langmuir.

Evidence for the existence of active spots has been brought forward by Smekal from quite a different angle (*Z. Electrochemie*, 1928, **34**, 472, and 1929, **35**, 567 and, *Z. phys. Chem.*, 1929, B**5**, 60). It had become necessary, in order to account for the electrical conductivity and long-wave photoelectric absorption of crystals, to recognise that a crystal of tangible size is in general a mosaic of very small perfect crystals imperfectly joined to one another. The number of imperfect crystal units rarely amount to less than 10^{-5} of the whole, and their number is considerably increased by the presence of impurities or by a rapid rate of growth. The work of Fromherz and Menschick (*Z. phys. Chem.*, 1929, B**3**, 1), on the adsorption of phosphorescent substances, indicates that adsorption is much more powerful on imperfect than on perfect crystal units. Smekal therefore considers that crystal imperfections, when they occur at the surface, must be regarded as catalytically " active spots." The view that a crystal is a mosaic is further confirmed by the calculation of Lennard-Jones (*Proc. Roy. Soc. A.*, 1928, **121**, 247), but the existence of powerful adsorption on the imperfect portions of crystal surfaces is not necessarily a proof that these are seats of catalytic activity. For whereas it appears that surface catalysis does not take place apart from adsorption, adsorption does not necessarily lead to catalysis.

Even where it does, it is evident, from a number of recent investigations, that there are several ways in which contact catalysis may come about. For instance, Bodenstein (*Z. phys. Chem.*, 1929, B**2**, 345), in a re-examination of some earlier work on the oxidation of sulphur dioxide in platinum in the light of recent work, considers that the gases are adsorbed in a monomolecular layer over the whole surface, but that oxidation only occurs at certain " active spots." He adopts the idea of Volmer (*Z. phys. Chem.*, 1925, **115**, 239, and 1926, **119**, 46) that the adsorbed molecules move about in two dimensions under the influence of thermal agitation just as gas molecules move about in three dimensions, and that the reaction rate is controlled by the rate at which the adsorbed molecules " diffuse " to the active spots. Bodenstein had formerly supposed the reaction rate to be controlled by the rate of diffusion of the reacting gases towards the surface through a layer of sulphur trioxide many molecules thick. The temperature coefficient is such as would be expected from the new theory, but is distinctly greater than would be anticipated from the old one, the difference being due to the marked diminution of the surface concentration of sulphur trioxide with increase of temperature as shown by the large heat of adsorption.

Rupp (*Ann. Physik*, 1929, **1**, 801 ; *Z. Electrochemie*, 1929, **35**, 586), following up the work of Davisson (*Proc. Nat. Acad.*

Sci., 1929, **14**, 619) and Germer (*Z. Physik*, 1929, **54**, 408), has used the reflection of electrons from crystal surfaces to study the conditions in the surface layers of the crystal lattice of metals. Evidence is obtained that "adsorbed" hydrogen actually enters the nickel lattice, giving the effect of a hydrogen crystal built into the nickel crystal. When the peculiar position of hydrogen is borne in mind, it does not appear that the above has necessarily any general bearing on the adsorption of gases by metals. Nevertheless, the line of investigation is an interesting one, and deserves further attention.

An interesting case has recently been brought to light by Allen and Hinshelwood (*Proc. Roy. Soc. A.*, 1928, **121**, 141), in the case of the bimolecular decomposition of acetaldehyde on heated wires. Here the molecules adsorbed on the wires are thermally activated, and react with gaseous molecules which collide with them. The reaction is peculiar, in that its rate does not depend upon the material of which the wire is made. In the case of exothermic reactions, a solid surface may promote the reaction by acting as acceptor for the heat of reaction. Still more important, it is possible, as Hinshelwood and Donnelly have indicated (*J. Chem. Soc.*, 1929, 1727), that a chain reaction may be initiated at a solid surface and persist as a homogeneous reaction. To some such effect as this is attributed the influence of an inert gas in lowering the temperature to which a platinum wire can be raised in a mixture of oxygen and hydrogen before an explosion occurs. As inert gases increase the rate of homogeneous reaction, and cannot be supposed to accelerate any heterogeneous reaction, it must be concluded that a homogeneous reaction is spreading from the wire.

The existence of effects such as these show that considerable care must be exercised in the interpretation of the velocity rates of heterogeneous reactions. Nevertheless, it is clear that the effects we have just been considering are of the nature of complications, and that in the great majority of cases contact catalysis is highly specific in nature and is confined to certain active spots. Balandin (*Z. phys. Chem.*, 1929, **B2**, 289, and **B3**, 167) has treated this kind of catalysis in considerable detail. He concludes that when a molecule is attached to a surface by only one of its atoms, the oriented adsorption which results is not usually associated with high activation, and the molecule usually re-evaporates from the surface without dissociation. When, however, two spots, each attracting different parts of the molecule, are situated close to one another on the surface, the molecule may be stretched and disruption be effected. It will be seen that he has arrived from a consideration of chemical evidence at a view which has much in common

with the one developed from wave mechanics at the beginning of this article. These ideas are still in an embryonic form, and it will be a matter of great interest to see how they will fit into the mosaic theory of the crystal surface, and whether the view of Taylor that catalytic activity is greatest at the corners and edges of crystals holds good in cases where attachment at more than one point is required for activation.

GEOLOGY. By G. W. TYRRELL, A.R.C.Sc., Ph.D., University, Glasgow.

Regional Geology and Stratigraphy.—A very large amount of work has recently been published on the geology of Polar and sub-Polar regions, especially Greenland and Spitsbergen. As Greenland lies midway between the two continents that are best known geologically, its geology is of prime importance in discussing problems of continental structure, inter-correlation, and lateral movements. Hence L. Koch's exhaustive new memoir (" The Geology of East Greenland and Stratigraphy of Greenland," *Medd. om Grönland*, **73**, 1929, pp. 1–204, 207–320), the culmination of sixteen years' work, is of the greatest value to European and American geologists. It is, of course, impossible to summarise its many conclusions in this place, but the reader is referred to a synopsis and discussion of the work by Prof. C. Schuchert (*Amer. Journ. Sci.*, XIX, 1930, pp. 337–50).

Prof. H. G. Backlund's " Contributions to the Geology of North-East Greenland " (*Medd. om Grönland*, **74**, 1930, pp. 209–95) concern the region between the Tiroler and King Oscar Fiords. He confirms Wordie's observations on the discovery of a sedimentary series on the western side of the crystalline axis, and shows that there is no normal junction-line between the sediments and the supposed Archæan rocks, the line everywhere being disjunctive and magmatic, with extraordinary penetration effects. Dr. Backlund also deals exhaustively with the post-Caledonian evolution of the region, and with the fiord-making of North-Eastern Greenland.

The most recent British exploration of East Greenland is that of the Cambridge Expedition of 1929 under Mr. J. M. Wordie. The geological results of that expedition are summarised in Wordie's geographical article (*Geogr. Journ.*, LXXV, 1930, pp. 481–504), and are dealt with in more detail in a paper by Wordie and W. F. Whittard (*Geol. Mag.*, LXVII, 1930, pp. 145–58). They differ from Backlund in regarding the crystalline axis as of pre-Cambrian age. The main geological result is the discovery of the Caledonian front in the Petermann Peak region. The southward trend of this orogenic margin may bring it to the unexplored region south of Scoresby

Sound, and suggests a junction with the Caledonian front already determined on the eastern side of North America (see paper by Prof. E. B. Bailey discussed in SCIENCE PROGRESS, July 1929, p. 23).

Norwegian geologists have also entered the East Greenland field, and publication of their results has begun with the appearance of a paper on the Upper Devonian of East Greenland by A. K. Orvin (*Norges Svalbard- og Ishavs-Undersökelser. Skr. om Svalbard og Ishavet*, No. 30, 1930, pp. 1–30). The formation consists of a basal conglomerate, and five alternations of grey and red sandstone, containing characteristic fish fossils, two of which are new to science, and have been described by A. Heintz in the same publication (pp. 31–46). The basal conglomerate rests on metamorphic rocks which are believed to correspond in great part with the Hecla Hoek of Spitsbergen.

Disco Bay in West Greenland was visited by a Hessian expedition in 1925, and Dr. H. K. E. Krueger has summarised the geological results (*Medd. om Grönland*, **74**, 1928, pp. 99–136). The region is built of a *Grundgebirge* consisting of an older and a younger (Agpat) series, covered by Cretaceous sediments and Tertiary basalts. The directions of the joint and fracture systems have been exhaustively mapped, and the results applied to the elucidation of the tectonics of the region.

Dr. H. Frebold's extensive investigations of the palæontology, stratigraphy, and palæogeography of the Trias in Spitsbergen, based mainly on the collections made by official Norwegian expeditions, and also on those made by Dr. K. Gripp's Hamburg-Spitsbergen expeditions ("Untersuchungen über die Fauna, die Stratigraphie, und Paläogeographie der Trias Spitzbergens," *Norges Svalbard- og Ishavs-Undersökelser. Skr. om Svalbard og Ishavet*, No. 26, 1929, 66 pp.; "Faunistisch-stratigraphische Untersuchungen über die Trias Spitzbergen und der Edge Insel," *Abh. Naturwiss. Ver. Hamburg*, XXII, 2–4 Heft, 1929, pp. 294–312), have led him to the conclusion that the configuration of continent and ocean in the Arctic region during the Triassic period was not essentially different from that of the present day. The evidence afforded by the Spitsbergen Trias, as also by the Jurassic and Lower Cretaceous, is in favour of the permanence of continents and oceans.

Dr. Frebold's equally voluminous work on the Jurassic and Cretaceous material collected by the State Norwegian expeditions and others ("Oberer Lias und Unteres Callovian in Spitzbergen," *Norges Svalbard- og Ishavs-Undersökelser. Skr. om Svalbard og Ishavet*, No. 20, 1929, 24 pp.; "Das Festungsprofil auf Spitzbergen: Jura und Kreide," *ibid.*, No. 19, 1928, 39 pp.; "Die Schichtenfolge der Jura und der Unterkreide an der Ostküste Südwest-Spitzbergen," *Abh. Naturwiss. Ver.*

Hamburg, XXII, 2–4 Heft, 1929, pp. 252–92), is conveniently summarised in a short paper entitled, " Stratigraphie und Paläogeographie des Jura und der Kreide Spitzbergens " (*C. f. Min.*, etc., Jahrg. 1928, Abt. B, pp. 625–9). He shows that the beginning of the post-Triassic marine transgression began, not in the Oxfordian, as hitherto supposed, but in the Upper Lias. The transgression was repeatedly interrupted by the temporary emergence of the Spitsbergen region as an archipelago, although the distribution of land and sea was never very different from that of the present day.

While the greater part of Dr. K. Gripp's important memoir on the results of his 1927 Spitsbergen expedition is glaciological (*Abh. Naturwiss. Ver. Hamburg*, XXII, 2–4 Heft, 1929, pp. 149–249) it contains a short stratigraphical section dealing with the Jurassic and Triassic of the northern coasts of the Stor Fiord. S. Obruchev's work with a Russian expedition has also provided a store of useful stratigraphical facts concerning the Jurassic and Cretaceous of the same region (" Geologische Skizze der Östkuste von Spitzbergen zwischen den Busen Whales Bay und Agardh Bay," *Ber. d. Wiss. Meeresinstitut*, Bd. II, Lief. 3, Moscow, 1927. Russian text, pp. 59–80, German résumé, pp. 81–8).

C. B. Bisset provides some geological notes on the granite-gneiss complex at the North Cape of North-East Land, on the geography and geology of the islands between North-East Land and Franz Josef Land, and on the basaltic rocks of Northbrook Island in Franz Josef Land (*Trans. Edin. Geol. Soc.*, XII, pt. 2, 1930, pp. 196–206).

Analysis of field observations by Dr. G. V. Douglas and other observers (G. V. Douglas, " Topography and Geology of South Georgia," " *Quest* " *Exped. Report, British Museum (Natural History)*, 1930, pp. 4–27), together with a study of D. Ferguson's and G. V. Douglas's collections, have led the writer (G. W. Tyrrell, " The Petrography and Geology of South Georgia," *ibid.*, pp. 28–54) to the following view of the geological history of South Georgia :

Orogenic Period.—With folding, cleavage, faulting, thrusting, and low-grade cataclastic metamorphism of rocks on north-eastern side of the island.

Cumberland Bay Series.—Shales, mudstones, and tuffs, with radiolaria and other fossils. Unconformity ? *Godthul Harbour Series.*—Quartzose greywackes, silt-stones, slates, and phyllites.	*Drygalski Fiord Igneous Series.* — Dolerite sills and dikes ; gabbro intrusions, etc. ; lavas of spilitic suite.

O. Holtedahl's recent work on the same region ("On the Geology and Physiography of Some Antarctic and Sub-Antarctic Islands," *Norske Vidensk.-Akad., Oslo*, 1929, 172 pp.; South Georgia, pp. 50–8) largely confirms Tyrrell's view of the succession, and through new fossil collections makes it fairly certain that the Cumberland Bay Series is of Mesozoic age. On this evidence Holtedahl, in his chapter entitled "On the South Antillean Arc Problem" (*ibid.*, pp. 104–17), favours Suess's original view that this island arc represents the connection between the Patagonian Andes and the Antarctandes of Graham Land. In his discussion of the problem, however, Tyrrell holds that the geological and palæontological evidence is still too scanty to be decisive.

In his notable Presidential Address to the Geological Society on "The Geological History of the Pacific Ocean," Prof. J. W. Gregory (*Quart. Journ. Geol. Soc.*, LXXXVI, 1930, Proc. lxxii–cxxxvi) comes to the conclusion that the Pacific Ocean has not existed as such throughout geological time, as claimed by many geologists. His main results are embodied in the following quotation: "The geological evidence indicates for several periods that the Pacific area was occupied by isolated land-locked seas, which usually had their main extension east and west, and sometimes continued across Asia to Europe or across America to the Atlantic. The existence of these seas and their dividing lands is known for nearly all the periods since the Cambrian. The lands appear to have been at times fully trans-Pacific, and to have provided routes by which animals and plants migrated between North America and Asia, and between Eastern Australia and South America. These lands across the Central Pacific seem to have survived until the Lower Kainozoic Era. . . ."

Wales is, as usual, well to the fore in recent British stratigraphical work. Mr. R. O. Roberts describes the geology of a Bala, Valentian, Wenlock succession in the district around Abbey-Cwmhir, Radnorshire (*Quart. Journ. Geol. Soc.*, LXXXV, 1929, pp. 651–76). Dr. C. A. Matley and Dr. A. Heard deal with the geology of the country around Bodfean, South-western Carnarvonshire (*ibid.*, LXXXVI, 1930, pp. 130–68). The area consists chiefly of Ordovician extrusive and pyroclastic volcanic rocks, including keratophyres, soda-trachytes, pyroxene-andesites, and a basalt, associated with marine sediments, the fossils of which indicate a Lower Bala age.

Dr. D. Williams's paper on the geology of the country between Nant Peris and Nant Ffrancon, Snowdonia (*ibid.*, pp. 191–233) deals with an area which is the northward continuation of the area described by Dr. Howel Williams in his recent Snowdon memoir (see SCIENCE PROGRESS, Oct. 1928, p. 221).

The general stratigraphical succession is from the Green Slates (Paradoxidian ?), through Upper Cambrian grits and slates, blue-black Lower Llanvirn slates, Llandeilo slates and grits, Gwastadnant grits, followed by rhyolitic lavas and tuffs of the Snowdonian groups. Two large granitic intrusions are believed to be co-magmatic with the Lower Rhyolitic Series. The folding, and the conspicuous cleavage-fans, are attributed to Siluro-Devonian earth movements.

The geology of Ramsey Island (Pembrokeshire) has been exhaustively described by Mr. J. Pringle (*Proc. Geol. Assoc.*, XLI, 1930, pp. 1–31) with the publication of a detailed map. The island is composed of Cambrian (Lingula flags) and Ordovician (Arenig and Llanvirn) sediments, with some intrusive igneous rocks.

An extremely useful pamphlet on the geology and physiography of the Bristol District (*Brit. Assoc. Adv. Sci.*, *Bristol*, 1930, 59 pp.), with an excellent coloured geological map, was compiled by Prof. S. H. Reynolds, R. Crookall, L. S. Palmer, A. E. Trueman, J. W. Tutcher, and F. S. Wallis, for the use of members of Section C (Geology) at the Bristol Meeting of the British Association. Prof. O. T. Jones's Presidential Address to Section C (*Brit. Assoc. Adv. Sci.*, *Bristol*, 1930, 27 pp.) dealt with " Some Episodes in the Geological History of the Bristol Channel Region." The main conclusion of this striking address was that the Bristol Channel came into existence as a definite basin by folding during the Miocene period, and that the present form of the surface in Devon and South Wales owed its origin to warping of an ancient (Triassic) surface of erosion during the same period.

Dr. Stanley Smith has described the Carboniferous inliers at Codrington and Wick, Gloucestershire (*Quart. Journ. Geol. Soc.*, LXXXVI, 1930, pp. 331–54). The several inliers expose a succession of beds ranging from the Tournaisian (Upper *Zaphrentis* Zone) to Coal Measures containing a Yorkian flora. The upper part of the *Syringothyris* and the base of the *Seminula* Zones are not exposed, and within the " Millstone Grit " a non-sequence undoubtedly occurs.

Mr. H. P. Lewis describes the Avonian succession in the south of the Isle of Man (*Quart. Journ. Geol. Soc.*, LXXXVI, 1930, pp. 234–90) as resting on a Basement Conglomerate, and extending from the *Michelinia grandis* Zone of Garwood to the *Posidonomya becheri* Beds in the Goniatites Zone of Bisat. These rocks are affected by overthrusting, which was probably contemporaneous with similar movements in the Craven, Carnforth, and Rush districts.

The Maryport district, of which the Geological Survey memoir by T. Eastwood has just appeared (*Mem. Geol. Surv.*

England and Wales, Expl. of Sh. 22, 1930, 137 pp.), is mainly Carboniferous, and includes the complicated Maryport Coalfield. Besides the Carboniferous Limestone and Coal Measures, the district also includes Ordovician and Triassic rocks.

The tectonic structure of the Howardian Hills and adjacent areas (Yorks), as elucidated by Dr. H. C. Versey (*Proc. Yorks Geol. Soc.*, XXI, pt. 3, 1929, pp. 197–227), appears to be somewhat complicated. It is dominated by two fault systems crossing approximately at 60 degrees. Ingenious experiments by Dr. Versey show that such an arrangement could be the result of pressure from two directions, one of which was accompanied by torsion. The existence of minor folds of Caledonian trend is indicated. Three periods of movement—intra-Jurassic, post-Jurassic and pre-Chalk, and post-Cretaceous—are suggested, and their relationship to the Market Weighton Axis is discussed.

Mr. Murray Macgregor's masterly review of Scottish Carboniferous Stratigraphy, modestly called an introduction to the study of the Carboniferous rocks of Scotland, represents his Presidential Addresses to the Geological Society of Glasgow, 1927–9 (*Trans. Geol. Soc. Glasgow*, XVIII, pt. 3, 1930, pp. 442–558). He first gives a general account of the Carboniferous sediments, follows with sections on the palæontological subdivisions, distribution of the rock-types, correlation of coals, and concludes with some valuable correlation tables. The memoir will prove of the utmost value both to students and the mining men in the Scottish coalfields.

The Geological Survey memoir on the Geology of North Ayrshire, by J. E. Richey, E. M. Anderson, and A. G. MacGregor (*Mem. Geol. Surv., Scotland*, Expl. of Sh. 22, 2nd ed., 1930, 417 pp.), is an exhaustive description of a complicated and fascinating region of sedimentary and igneous rocks. A full review of this work appears in the *Geol. Mag.*, LXVII, 1930, pp. 331–3. The stratigraphical interest of North Ayrshire chiefly resides in the evidences of overlap against the Lower Carboniferous volcanic hills in the south-eastern part of the area, and in the extraordinary variations in thickness of the Limestone Coal and Upper Limestone Groups in the neighbourhood of the Dusk Water and Inchgotrick Faults. It is suggested that these faults were in intermittent movement during the deposition of the sediments.

Mr. M. Black's paper on "Drifted Plant-beds of the Upper Estuarine Series of Yorkshire" (*Quart. Journ. Geol. Soc.*, LXXXV, 1929, pp. 389–439) contains a good discussion of the conditions of deposition of this series. He regards the rocks as having been formed during one cycle of the building of the Middle Jurassic delta, a lower current-bedded series (Moor Grit,

etc.) representing the foreset beds, and an upper level-bedded series interrupted by "wash-outs," the topset beds. The plants are mostly drifted; plant-beds *in situ* are very poorly developed.

The same series in Northamptonshire and Northern Oxfordshire is dealt with by Mr. Beeby Thompson (*ibid.*, LXXXVI, 1930, pp. 430–62). The beds consist largely of variegated clays containing an abundance of probably freshwater vegetable matter, interspersed with brackish-water, and also marine strata. The object of the paper is to establish more definite correlations than hitherto between the sections exposed in the two counties at this geological horizon.

Dr. E. Wiman's important memoir, entitled "Studies of some Archæan Rocks in the Neighbourhood of Upsala, Sweden, and of their Geological Position" (*Bull. Geol. Inst. Upsala*, XXIII, 1930, pp. 1–170), is a very comprehensive study of the stratigraphy, tectonics, petrology, and metamorphism of these very ancient basement rocks. The Upsala region is built of two Archæan formations, the oldest being volcanic rocks of acid and intermediate characters (Hälleflint-leptite formation), and a much younger formation of hypabyssal and plutonic rocks, which are mainly of granitic composition (Upsala and Vänge Granites).

The "porphyry-sandstone" region of the Brumund Valley in South Norway described by H. Rosendahl ("Brumunddalens porfyrsandstein lagrekke," *Norske Geol. Tidsskr.*, X, 1929, pp. 367–448), is situated in the south-eastern part of a Cambro-Silurian area between two overthrust blocks of the Lower Cambrian Sparagmite formation. The small Devonian outlier in the valley consists of sandstone and four "porphyry" sheets, which are rhomb-porphyries of larvikitic affinity belonging to the famous Oslo petrographic province.

Two important tectonic memoirs on West European regions (H. Scholtz, "Das varistische Bewegungsbild," *Fortschr. Geol. u. Pal.*, VIII, 1930, pp. 235–316; K. Hummel, "Die tektonische Entwicklung eines Schollengebirgslandes (Vogelsberg und Rhön)," *ibid.*, pp. 1–233), are too long and detailed for comment here. Similarly, three exhaustive memoirs on regions in Southern and Eastern Europe can only be mentioned in this place (M. M. Blumenthal, "Beiträge zur Geologie der betischen Cordilleren beiderseits des Rio Guadalhorce," *Eclog. Geol. Helvet.*, **23**, 1930, pp. 41–294; "Die Neogenebucht von Varna und ihre Umrandung, Nord-öst Bulgarien," *Abh. Math.-Phys. Kl. Sächs. Akad. Wiss.*, XLI, 1929, 90 pp.; F. R. Cowper Reed, "Contributions to the Geology of Cyprus," *Geol. Mag.*, LXVI, 1929, pp. 435–46; LXVII, 1930, pp. 241–71).

In his new provisional geological map and descriptive notes thereon, Sir A. E. Kitson (*Gold Coast Geol. Surv.*, Bull. 2, 1928,

pp. 13, 20 plates and geol. map) has provided a useful summary of Gold Coast geology. In the first memoir of the Survey Mr. O. A. L. Whitelaw has described the geology and mining features of the Tarkwa-Abosso goldfield (*Gold Coast Geol. Surv.*, Mem. No. 1, 1926, 46 pp.). This goldfield has a remarkable structural and petrological resemblance to that of the Witwatersrand. The author of the memoir and Sir A. E. Kitson are strongly of the opinion that the gold was originally of alluvial (placer) origin, and that it has since been dissolved and reprecipitated at or near its original position.

Other important papers on African geology, on which limitations of space preclude further comment are: K. S. Sandford, "The Pliocene and Pleistocene Deposits of Wadi Qena and of the Nile Valley between Luxor and Assiut (Qau)" (*Quart. Journ. Geol. Soc.*, LXXXV, 1929, pp. 493–548), and F. Dixey, "The Geology of the Lower Shire—Zambezi Area" (*Geol. Mag.*, LXVII, 1930, pp. 49–60).

The investigations of T. T. Quirke and W. H. Collins, on the northern shore of Lake Huron, which have been published under the intriguing title of "The Disappearance of the Huronian" (*Canada Geol. Surv. Mem.*, **160**, 1930, 112 pp.), were directed towards ascertaining what became of the 23,000-ft. Huronian formation east of a line extending north-east from Killarney, at which they seem to end off against granite. It was found that they do not end at this line. Highly-metamorphosed vestiges of the Huronian sediments, transformed into gneisses of igneous aspect, were found to the east of the line, and the suggestion is made that much of the Killarney Granite is really completely fused and recrystallised Huronian. A good deal of the evidence supporting this conclusion falls to be dealt with under the heading of metamorphism.

In discussing some Huronian problems Prof. A. C. Lawson (*Bull. Geol. Soc. Amer.*, **40**, 1929, pp. 361–84) rejects Quirke's conclusions as to the age and "ultra-metamorphism" of the Killarney Granite (expressed in an earlier paper than that commented upon above). He regards the Killarney Granite as post-Cobalt in age. He also argues that the Sudbury Series is not pre-Huronian as supposed, but is equivalent to the Cobalt Series. The Killarney Granite is therefore included among the granites (Algoman) of his second great period of revolution in the Canadian pre-Cambrian. It may also be mentioned that the conclusions of Quirke and Collins on the relations of the Killarney Granite at Killarney are disputed by W. A. Jones, in a paper on "The Petrography of the Rocks in the Vicinity of Killarney, Ontario" (*Contributions to Canadian Mineralogy*, 1930, *Univ. of Toronto Studies, Geol. Ser.*, No. 29, pp. 39–60) which has just come to hand.

Mr. J. E. Hawley discusses the question as to whether certain schistose sediments associated with basic Keewatin volcanics in the Rainy River district of Ontario (*Journ. Geol.*, XXXVIII, 1930, pp. 521–47) belong to a stratigraphical position below (Coutchiching), or above (Seine), the Keewatin. The data now available, although incomplete, favour a Seine age for these sediments.

The major conclusion of Prof. C. Schuchert's valuable memoir on "The Geological History of the Antillean Region" (*Bull. Geol. Soc. Amer.*, **40**, 1929, pp. 337–60) is, in his own words, that: " . . . the now very deeply-sunken Gulf of Mexico, along with Florida and the Bahamas, are but the altered flat plate or foreland of the Greater Antilles and Central America, and that the two last-named lands make the very mobile southernmost frame of the North American continent. The great deepening of the Gulf of Mexico took place during the Cretaceous, with further additions during the Oligocene and Pliocene, since which times the perimeter of this basin has risen a few hundred feet."

Dr. C. A. Matley's memoir on "The Basal Complex of Jamaica, with Special Reference to the Kingston District" (*Quart. Journ. Geol. Soc.*, LXXXV, 1929, pp. 440–92) deals with the basement rocks underlying Jamaica, consisting of various metamorphic representatives of sedimentary, volcanic, and plutonic rocks, the petrography of which is dealt with by Mr. F. Higham. The metamorphic part of the complex is shown to be almost certainly of pre-Mesozoic age. A valuable comparison and correlation with other West Indian islands is given.

The United States Geological Survey has recently published an exhaustive summary of the geology of South-Eastern Alaska, by A. F. Buddington and T. Chapin (*Bull. 800, U.S. Geol. Surv.*, 1929, 398 pp.). A connected account of the great Coast Range batholith is not the least valuable part of the memoir.

A useful short summary of the geology of Queensland has been published by Dr. F. W. Whitehouse for the Brisbane Meeting of the Australasian Association for the Advancement of Science (*Handbook Aust. Assoc. Adv. Sci.*, Brisbane Meeting, 1930, pp. 23–39).

ZOOLOGY.

By F. W. Rogers Brambell, Ph.D., D.Sc., Professor of Zoology, University College of N. Wales, Bangor.

An important discovery is announced by Popa and Fielding (*Jour. Anat.*, **65**, 1930) in a preliminary note on "A Portal Circulation from the Pituitary to the Hypothalamic

Region." A system of vessels in the stalk of the human pituitary which collect blood from the pars anterior, pars intermedia, pars tuberalis, and pars posterior of the pituitary and distribute it in the hypothalamic region is described. It is proposed to call this system the hypophyseo-portal veins. Plain muscle has been found in the walls of these vessels. The vessels acquire, as they ascend the stalk of the pituitary, thick neuroglia sheaths. Superiorly these sheaths disappear, and the vessels open out into a secondary distributing network of very fine channels beneath the infundibular recess of the 3rd ventricle. It is suggested that this network may be distributed in connection with certain nuclear groups, but this is being investigated farther. Long processes from the ependymal cells lining the ventricle contribute to the supporting tissue of the fine vessels of this network. The blood, which is sent into all four parts of the pituitary by the arterial supply, is therefore conveyed away in part by the hypophyseo-portal vessels and in part by the systemic veins. Colloid material is found in association with the vessels of the hypophyseo-portal system, sometimes within them, and sometimes in the perivascular sheaths. It can also be detected among the cells of certain hypothalamic nuclei. The size of the lumina of the hypophyseo-portal vessels appears to be actively and precisely controlled. The mechanism by which the lumina of the vessels is regulated is being investigated farther.

It is too soon to comment on these extraordinary and interesting results. In view of the importance of the pituitary as an endocrine organ and the complexity of its hormonal control, the full statement of the results described in this preliminary paper will be awaited with much interest.

The anatomy of the arterial system of *Loris Lyddekerianus* is described in a valuable paper by Rau and Rao (*Jour. Mysore University*, **4**, 1930). The most interesting feature of the arterial system is the pronounced plexiform condition of the subclavian, external iliac, and the middle sacral arteries. Such vascular plexuses have been described in a variety of animals, including fish, birds, ungulates, cetaceans, and lemurs. The functional significance of the plexiform condition of these vessels is discussed in the present paper, and support is lent to the view that they act as blood reservoirs.

An experimental investigation on pigment-formation in the mesodermal pigment cells of the fowl *in vitro* has been carried out by Koller (*Arch. f. exp. Zellforschung*, Bd. **8**, 1929). Branched pigment cells occur in the connective tissue of fowls, chiefly of dark breeds. These appear in explants of mesoderm from three-day embryos of black leghorns after twenty-four to thirty-six hours' cultivation *in vitro*. It was observed that

the pigment arose from colourless granules in the cytoplasm, which could be stained *intra vitam* with neutral red. These granules transformed into the dark-brown pigment in eight to ten hours. These mesodermal pigment cells exhibited active amœboid movement, and they differed from retinal pigment cells in this respect as well as in their scattered distribution and more rapid development *in vitro*. It was found that pigment cells developed in explants of black leghorn mesoderm even when cultivated in white leghorn embryo extract and plasma. Pigment cells never developed in explants of white leghorn mesoderm, even when cultivated in black leghorn embryo extract and plasma. These results are exceedingly interesting. since genetically the white leghorn is dominant to the black leghorn for colour. Consequently the black pigment does not develop in a leghorn heterozygous for these characters. It might be expected therefore that extract or plasma from white leghorn embryos would inhibit the development of pigment cells in explants of black leghorn mesoderm in a similar manner *in vitro*, but this is not the case. Koller's result is supported by Danforth and Foster (*Proc. Soc. Exp. Biol. and Med.*, **25**, 1927), who found that pigment was developed in transplants of black leghorn skin grafted on a white leghorn fowl.

A series of experiments, designed to find a suitable fluid in which to keep mammalian sperms, taken from the epididymis, at body temperature at maximum activity for a maximum length of time, are described by Baker (*Q. Jour. Exp. Physiol.*, **20**, 1930). A buffered glucose-saline solution was found to be the best. The results obtained with it are far better than those obtainable with Ringer's fluid, the majority of the sperms being still active six or seven hours after the start of the experiment. It is stated that the solution is also suitable for keeping sperms alive for much longer periods at low temperatures.

The effect of temperature on the survival *in vitro* of rabbit spermatozoa obtained from the vas deferens has been investigated by Walton (*Jour. Exp. Biol.*, **7**, 1930). A useful technique for collecting and keeping the sperms is described. The optimum temperature for survival of the sperms was found to be in the vicinity of 15° C. The maximum time of survival of spermatozoa at this temperature, as judged by fertility of artificial insemination experiments, was found to be seven days.

Hammond (*Jour. Exp. Biol.*, **7**, 1930), in a parallel series of experiments, has investigated the effect of temperature on the survival *in vitro* of rabbit spermatozoa obtained from the vagina. Temperatures of 35° C., 10° C., and 0° C. were employed. The maximum time of survival was ninety-six

hours at 10° C., as compared with fourteen hours at 35° C., and sixteen hours at 0° C. Neither Hammond nor Walton could detect any significant effect on the sex-ratio of the young produced from sperms that had been kept *in vitro*.

The effects of parasitic castration on Crustacea have attracted much attention since the appearance of the classical papers of Geoffrey Smith on this subject. A new and important contribution to the subject has been made by Tucker (*Q.J.M.S.*, **74**, 1930), who has investigated the effects of *Gyge branchialis* on the host *Upogebia littoralis*. *Gyge branchialis* is a member of the family Bopyridæ of the Isopoda epicarida, which is parasitic in the branchial chamber of the host, *Upogebia littoralis*, one of the Thalassinidea, a group of decapod crustaceans. This paper provides the first complete account of the effects of an epicaridon isopod on its host.

The host becomes parasitised very early in life, thus reducing to a minimum the variation in the effects of parasitism due to parasitisation occurring late in the life of the host. The first larva of the parasite which becomes attached to the host develops into a female, apparently in response to the large supply of nourishment available. The next larva becomes attached to the body of the first and develops into a male. It is probable that other larvæ, should they be present, fail to develop beyond metamorphosis.

The parasites produce a remarkable effect on the development of the hosts when these are males. The chelæ of parasitised males agree in size and shape with those of the female and the appendages of the first abdominal segment are developed, although they are present normally only in the female. Parasitised females have chelæ that tend to average very slightly smaller than those in normal females, and are otherwise unaltered externally. The growth curves of the parasitised male chelæ approximate closely to those of the normal female, whereas those of the parasitised female are not altered significantly. All the parasitised males are modified on account of the early age at which parasitisation occurs.

The testes of normal males of *Upogebia littoralis* usually contain a small number of scattered oocytes. This tendency to develop oocytes is much accentuated in parasitised males. The oocytes tend to become conspicuously more numerous and to develop farther than in normal males. They may even form yolk. In some cases a whole tract of the testis becomes completely converted into ovary. The condition of the testis as a whole may vary from being slightly reduced to being almost completely atrophied. Spermatogenesis may proceed in some parasitised males, whereas it may cease at an early stage in others. The condition of the testis is not,

however, closely correlated with the degree of modification of the secondary sexual characters. The gonads are completely absent in the large majority of parasitised females. Thus the effects of this parasite on the sexual characters of the host are to produce feminisation of the males, but no corresponding transformation in the females. They agree in this respect with other examples of parasitic castration in Crustacea already known.

It has been suggested by Huxley (*Biol. Zentralb.*, **47**, 1927) that in some animals, such as Crustacea, which exhibit disharmonic growth, there may exist growth centres correlated with actively growing organs. Further, the growth of neighbouring organs may be affected by the growth of the heterogonic organ. Tazelaar (*Jour. Exp. Biol.* **7**, 1930) has investigated the relative growth of parts in *Palæmon carcinus* with a view to testing this hypothesis. The heterogonic cheliped in this form is the second pereiopod. Measurements were made of this appendage, the two anterior to it (*i.e.* the 3rd maxilliped and the 1st pereiopod), the three posterior to it (*i.e.* the 3rd, 4th, and 5th pereiopods), and the carapace length in both sexes. It was found that the cheliped exhibited heterogonic growth in both sexes, but more markedly in the male (♂ $k = 1 \cdot 8$, ♀ $k = 1 \cdot 48$). The other pereiopods were slightly heterogonic in both sexes, and the relative growth-rates were graded from the 3rd to the 5th, the former being slightly greater than the latter. The growth-rate of the 1st pereiopod is smallest in the male and largest in the female. The difference between the rates of growth of the 1st and 3rd pereiopods in the male and female was greatest where the rate of growth in the heterogonic cheliped was greatest in the male. The growth of the 3rd maxilliped was slightly negatively heterogonic in both sexes (♂ $k = 0 \cdot 93$, ♀ $k = 0 \cdot 95$). It is concluded that there is a correlation between the marked heterogony in the cheliped and the growth-rate of neighbouring appendages. The growth-rate of those appendages immediately posterior to the cheliped was accelerated, while that of those appendages immediately anterior was retarded.

Our knowledge of tracheal respiration in insects is greatly enhanced by a recent paper by Wigglesworth (*Proc. Roy. Soc.*, B., **106**, 1930). The paper is devoted to the problem of how the supply of oxygen to the terminations of the trachioles is maintained. There has been in the past much divergence of opinion as to whether the terminal portions of the tubules normally contain liquid or gas, but no measure of general agreement has been attained. The forces which determine the supply of oxygen to the terminal portions of the tubules have not previously been elucidated, but the work of Krogh (*Arch.*

f. ges. Physiol., **179**, 1920) has provided a valuable contribution to this problem. Krogh has shown that in the tracheæ, as distinct from the smaller tracheoles, diffusion is sufficient to explain the supply of those quantities of oxygen to the tissues which they actually consume. Simple diffusion is only supplemented in the largest tracheal trunks by inspiratory and expiratory movements. The assumption that diffusion would also account for the gaseous exchange in the smaller tracheoles is probably justifiable. Wigglesworth, while admitting the importance of Krogh's work, rightly points out that it makes no allowance for temporary and local increases in the requirements of oxygen brought about by local increase in the activity of the tissues. Yet some mechanism must exist which provides for increased requirements of oxygen locally brought about by local changes in the respiratory metabolism. Wigglesworth has formulated a theory, complementary to that of Krogh, which satisfies these requirements. He points out that if the terminal portions of the tracheoles contain liquid, then "the oxygenation of the tissues will increase as the extent of this liquid up the tubes becomes less; for the farther the liquid extends, the more remote will become the column of air from which the oxygen must diffuse. Now the demand for oxygen will increase with any increase in the activity of the tissues. Therefore, what is required by the insect is that the column of air shall extend more deeply into the tissues when and where their activity is greatest." If it is assumed that the ends of the tracheoles are closed by a semi-permeable membrane, then water will tend to be driven into them by the hydrostatic pressure in the tissues, and will be drawn along them by capillarity. Its extent in the tubule will be limited by the pressure of the air, and by the osmotic pressure of the tissue fluids. Normally the hydrostatic pressure in the tissues and the gas pressure in the tubules will be approximately equal to atmospheric pressure. The capillarity of the tubules may be expected to remain constant, irrespective of the activity of the tissues. The osmotic pressure of the tissue fluids, on the other hand, will vary with the activity of the tissues, since activity is commonly associated with the breakdown of large into small molecules, such as the production of lactic acid from glycogen during muscular contraction.

It follows that increased activity will raise the osmotic pressure of the tissue fluids, and will result in the extraction of fluid from the tracheoles. The column of air in the tubule will extend as the terminal column of fluid decreases, with the consequent increase in the diffusion of oxygen, until the increased capillarity of the finer tubule balances the increased osmotic pressure. With recovery the osmotic pressure

will fall, the column of liquid in the tubule will increase, and oxygenation will be reduced once more. This theory thus provides an explanation of the mechanism whereby the supply of oxygen is increased in those regions where there is an increased demand for oxygen due to activity. The results of a series of experiments on the larvæ of the mosquito, *Stegomyia*, provide evidence in favour of this theory. It was found that the terminal portions of the tracheoles were filled with liquid in the resting larva, but that during asphyxiation this liquid was absorbed, and the column of air rapidly extended, especially towards active muscles. When asphyxiation was stopped by readmission of air, the liquid in the tubules slowly rose to its original level. During asphyxiation the amount of lactic acid in the tissues increased. Hypertonic solutions and the tissue fluids from asphyxiated larvæ, when introduced into resting larvæ, both caused extension of air down the tracheoles, while hypotonic solutions had a slightly negative effect or no effect at all.

A paper on " The Feeding Mechanism, Formation of the Tubes, and Physiology of Digestion in *Sabella pavonina*," by Nicol (*Trans. Roy. Soc. Edinburgh*, **56**, 1930) provides interesting reading. The branchial crown of this common British polychæte functions as a ciliary filtering organ. The crown is formed of two symmetrical portions, and can be opened and closed. Each half consists of a basal lamella bearing numerous filaments, each of which have a pair of basal folds and two rows of short pinnules. The terminal mouth is surrounded by two lateral lips and a dorsal lip, which bears two elongated palps. The floor of the œsophagus is formed by the fused lateral lips, the free edges of which are continued as a pair of ventral sacs, which lie immediately in front of the collar folds of the first body segment.

The pinnules of the branchial crown are provided with cilia, which cause a current of water to flow between the filaments into the branchial funnel. Small particles in suspension in the water are collected and carried down a groove in each filament to the basal folds, where they are sorted according to size. The largest particles are rejected by the palps. The medium-sized particles are carried to and stored in the ventral sacs. Only the smallest particles are conveyed to the mouth.

The function of the ventral ciliated groove, the palps, and the mucous glands is to reject waste material from the anus, the inside of the tube, and the branchial funnel.

Contractions of the body maintain a constant current of water within the tube. The tube is added to at the anterior end. The particles stored in the ventral sacs are mixed with mucus, added to the edge of the tube by rotation of the

anterior end of the worm combined with the action of the collar folds, and cemented in place by mucus secreted by the first body segment.

Four regions can be distinguished histologically in the alimentary canal, which is straight and without diverticula. Secretion takes place in the second of these regions. Peridinians, flagellates, algal spores and diatoms, as well as fine sand and detritus, constitute the gut contents. The passage of food through the gut at 16° C. takes about twenty-two and a half hours. The enzymes found to be present were an amylase, a protease, and a lipase.

Much discussion has centred around the problem of whether ciliates can be cultured indefinitely without conjugation. Some protozoologists, headed by Calkins, maintain that the division-rate in cultures gradually decreases, and that they finally die unless conjugation takes place. If conjugation occurs, the division-rate reaches its initial value again, and a new life-cycle is begun which, in its turn, ends either in conjugation or death. Other workers, including Woodruff, maintain, from experimental evidence also, that such cultures can be continued indefinitely without conjugation. It has been pointed out, in support of this contention, that the length of life of the cultures has increased as the methods of culture have been refined and improved. Darby (*Jour. Exp. Biol.*, **7**, 1930) has described an interesting series of experiments on the effects of the H-ion concentration of the medium employed on the life of the cultures. He employed *Paramœcium caudatum*, *P. aurelia*, and *Stylonichia pustulata* for this purpose, and cultured them in media of which the *p*H was known and carefully controlled. He found that a constant division-rate could be maintained in these species of ciliates by keeping the culture medium at a constant optimum H-ion concentration. The optimum for *Paramœcium* was found to be *p*H 6·95 and for *Stylonichia* *p*H 7·6. Moreover, the variations in the division-rate found previously in the typical life-cycle, including the gradual decline and ultimate death, can be produced experimentally by altering the *p*H of the medium. The life-cycle disappeared in cultures maintained under optimum conditions, and encystment and conjugation could take place at any age. It was found also that neither conjugation nor endomixis had any effect on the subsequent division-rate under optimum conditions. The H-ion concentration was found also to affect the length of the endomictic period in *P. aurelia.* It is pointed out in conclusion that the concept of the life-cycle theory is drawn from the Sporozoa in which the passage from one host to another is marked by drastic changes in behaviour and appearance. It is suggested that these

changes may be responses to different environmental conditions. "The H-ion concentration, temperature, and salt-balance met with in the human blood by a Plasmodium are totally different from those in the mosquito. It is open to experimentation to alter the medium in one of these respects and attempt to produce at will the various forms displayed in the life-history of a parasite."

ENTOMOLOGY. By H. F. Barnes, B.A., Ph.D., Rothamsted Experimental Station, Harpenden.

Owing to a slip it was stated in Recent Advances in the July number, p. 36, that parthenogenesis had only been recorded in Hemiptera, Coleoptera, Hymenoptera, Diptera, and Psocoptera. Its occurrence in Orthoptera, Lepidoptera, and Thysanoptera should also have been mentioned.

General Entomology.—Three interesting publications have been recently issued by the Imperial Institute of Entomology, viz.: (1) *A List of the Entomologists Employed in the British Empire* (16 pp., 1930); (2) *A Summary of Data Relating to Economic Entomology in the British Empire* (23 + 2 pp., 1930); and (3) *Report of the Third Imperial Entomological Conference, 17th–27th June*, 1930 (59 pp., 1930). A useful book, entitled *Histological and Illustrative Methods for Entomologists* (xii + 139 pp., 1 pl., 18 figs. Oxford: The Clarendon Press, 1930), has been prepared by H. Eltringham. Volume V of the Smithsonian Scientific Series (*Insects: Their Ways and Means of Living*, by R. E. Snodgrass, 1930, iv + 362 pp., 186 figs., 14 coloured plates) is a beautifully edited book, written in a semi-popular manner. The coloured plates are very good. The chapter headings are as follows: The Grasshopper, The Grasshoppers and Cousins, Roaches and Other Ancient Insects, Ways and Means of Living, Termites, Plant Lice, The Periodical Cicada, Insect Metamorphosis, The Caterpillar and Moth, Mosquitoes and Flies. An altogether interesting and instructive book. A short textbook on entomology (theoretical and practical) in Russian, by M. N. Bogdanov-Kathow (535 pp., 416 figs. Moscow: Government Printing Office, Leningrad, 1930), has recently appeared.

H. J. Hansen (*Studies in Arthropoda*, vol. III, 1930, 376 pp., 16 plates, Copenhagen) has issued the second and concluding part of his work on the comparative morphology of the appendages in the Arthropoda. In this part he deals firstly with a comparison of the legs in Insecta with their larvæ, in Myriapoda and in Arachnida with the walking legs in Malacostraca. Secondly, he has investigated the mouth-parts in types of nearly all orders of Insecta and Myriapoda, in comparison with those

of Isopoda and Amphipoda. Perhaps the most important part of this work is the chapter on the mouth-parts of the Insecta, where Hansen attempts to prove definitely the theory, first set forth by him in 1893, that the four pairs of mouth-limbs are homologous with the four pairs in Isopoda and Amphipoda.

A new theory of the evolution of the class Insecta by R. J. Tillyard (*Roy. Soc. Tasmania, Papers and Proc.*, 1930, 89 pp., 19 figs., 1930) has recently appeared. First, he deals with the better-known theories of insect evolution: the descent from Trilobita—Handlirsch's Theory; the descent from Crustacea culminating in Crampton's Theory; the general theory of the terrestrial origin—Versluy's Theory; the descent from Myriapoda—Brauer's Campodea Theory. Then the new theory is set out. Very briefly, it is the evolution of the Progoneata (Symphyla, Pauroda, Diplopoda), of the Opisthogoneata (Chilopoda, Schizotarsia), and of the Insecta (Collembola, Protura, Thysanura, Pterygota) from an hypothetical protomorphic ancestor. The hexapod tendency set in even before anamorphosis, and produced the Collembola in the lower Devonian. Only a single anamorphic group is left, viz. the Protura, distinguished by the loss of the antennæ. Then came the Thysanura Entrotrophica, possessing five-segmented legs, but already fully epimorphic. At a slightly higher level came the Thysanura Ectotrophica with three-segmented tarsi, inserted mouth-parts, and a more complete tracheal system. These divided into the ventrally flattened running types (Lepismatidæ) and the laterally flattened jumping types (Machilidæ). The Lepismatoid got ahead of the Machiloid, and developed compound eyes and a five-segmented tarsus. From such an ancestral form the whole of the Pterygota arose.

A further important paper on the anatomy of the spiracles of insects has been published by A. Melis (*Redia*, **18**, 1930, 125–62). In this contribution he deals with the larvæ of various Lepidoptera (including *Carpocapsa pomonella*, *Pieris brassicæ*, *Pyrausta nubilalis*, and *Mamestra brassicæ*), and Coleoptera (including *Melolontha hippocastani*, *Sphæroderma testaceum*, and *Cleonus luigionii*).

Following a paper pointing out that the *p*H of the intestinal juice in various groups of insects varies according to the Orders to which they belong (*Jl. Biochem.*, **11**, 1930, 100–23), O. Shinoda (*Anniversary Volume*, Kyoto, 1930, 9–24) has shown that the *p*H optima of proteases are more or less coincident with the *p*H of their intestinal juice. On the other hand, the *p*H optima of carbohydrases are more influenced by the food habits.

N. L. Sacharov (*Ecology*, **11**, 1930, 505–17), in a study of

cold resistance of insects, pays special attention to the caterpillars of *Euxoa segetum.* Using the method of dilatometer and criohydrate solutions, he has found that cold hardiness depends on the minimum quantity of water and the accumulation of fat in the organism, and that all hibernating stages of insects prepare for the winter by reducing the quantity of free water in their tissues. An important fact is that, if the normal environmental conditions are disturbed, the insects are killed by cold whether they are prepared for it or not. The underlying principles of insect epidemics are discussed by F. S. Bodenheimer (*Zeit. f. angew. Ent.* **16**, 1930, 433–50). In this connection a recent article by V. E. Shelford (*Qtr. Rev. Biol.*, **5**, 1930, 207–16), describing the evolution of the modern ideas on the threshold of growth, the developmental unit and velocity charts for temperature and humidity, is interesting.

A very readable report on the organisation and progress of the work of Farnham House Laboratory has been written by W. R. Thompson ("The Biological Control of Insect and Plant Pests," *E.M.B.*, **29**, 1930, 124 pp., 8 plates). In this publication the whole theory and practice of biological control is fully discussed, while the buildings of the laboratory, as well as its practical work, are described. The same author has written (*Ann. Appl. Biol.*, **17**, 1930, 306–38) a very interesting general survey of the principles of biological control. All those interested in this subject should read these two papers.

Several good studies on insect ecology have recently been made. O. W. Richards and G. V. B. Herford (*Ann. Appl. Biol.*, **17**, 1930, 367–95) have written an account of the insects found attacking or in association with stored products (cacao, spices, and dried fruits) in London warehouses. S. L. Hara (*Phil. Trans. Roy. Soc. London*, **B 218**, 1930, 171–282) has been studying the ecology, bionomics, and evolution of the torrential fauna, with special reference to the organs of attachment. The bulk of this contribution deals with insects. E. Percival and H. Whitehead (*Jl. Ecology*, **18**, 1930, 286–302) have made a survey of the invertebrate fauna of the river Wharfe. It would appear that the great majority of the species recorded require water well charged with oxygen; this is interesting from the point of view of pollution in this river. The animal community, inhabiting rotten posts, has received the attention of O. W. Richards (*Jl. Ecology*, **18**, 1930, 131–8).

Biological races in insects form the subject of a review paper by W. H. Thorpe (*Biol. Rev.*, **5**, 1930, 177–212). This paper lays stress on the value of the contributions of applied entomology to the more academic side of the whole problem.

E. P. Mumford and D. H. Hey (*Nature*, **125**, 1930, 411–12) have drawn attention to the water balance of plants and the

nitrogen content of the sap as factors in the susceptibility of plants to insect attack.

A second series of collected records relating to insect migration has been compiled by C. B. Williams (*Trans. Ent. Soc. London*, **78**, 1930, 139–70). It will be remembered that the first series was published last year (*loc. cit.*, **77**, 1929, 79–91).

Orthoptera.—G. F. Fause (*Ecology*, **11**, 1930, 307–25) has made an ecological study of Orthoptera in the Northern Caucasus. He finds that the connection between an abundance of species and ecological conditions follows the law of Gauss, which is analogous with data obtained by botanists.

Coleoptera.—D. Oglobin (*Eos*, **6**, 1930) has made an interesting study of thirty-five of Motschusky's species of Halticine beetles. This paper is well illustrated, and is particularly valuable because it was thought that the types had been lost. In a paper on the genitalia and wing venation of the Cucujidæ and related families (*Ann. Ent. Soc. America*, **23**, 1930, 305–58), J. W. Wilson has come to the conclusion that the Mycetophagidæ is the most primitive of those studied and should be placed in the Byrrhoid series rather than with the Cucujoidea. He also sees, from a study of the genitalia, wing venation and body characters, no reason to separate the family Cucujidæ into the four families suggested by Böving. On the other hand, the family Silvanidæ as proposed by Böving may be accepted.

The evaporation from the meal worm has been studied by P. A. Buxton (*Proc. Roy. Soc.*, **B 106**, 1930, 560–77). It is shown that the larva can maintain the proportion of water in its body nearly constant during a month's fast at humidities from 0–60 per cent. It apparently does this by consuming some stored substance and holding the water produced in metabolism. The effect of continuous light upon full-grown meal worms is shown by R. T. Cotton (*Proc. Ent. Soc. Wash.*, **32**, 1930, 58–60), to accelerate the transformation to the pupal state; by making use of this fact a supply of all stages of the worm can be obtained throughout the year.

The external morphology and internal anatomy of the larva of *Dorcus parallelopipedus* has been described by E. E. Edwards (*Jl. Linn. Soc., Zoo.* **37**, 1930, 93–108). M. C. Swingle (*Jl. Agric. Res.*, **41**, 1930, 181–96) has made a detailed study of the anatomy and physiology of the digestive tract of the Japanese beetle; while H. Kôno (*Jl. Fac. Agric. Hokkaido Imp. Univ.*, **29**, 1930, 36 pp.) deals with the biological groups of *Rhynchites*, *Attelabius*, and *Apoderus*.

The association of a new species of nematode, *Neodiplogaster pinicola*, with *Pissodes strobi*, the white pine weevil, is

recorded by G. Steiner (*Jl. Agric. Res.*, **41**, 1930, 125–30). The feeding rate of the Australian lady beetle, *Rodolia cardinalis*, on *Icerya purchasi* has been studied by A. W. Cressman and J. O. Dumestre (*Jl. Agric. Res.*, **41**, 1930, 197–203). They found that the daily rate was a function of temperature, age, seasonal change, and sex.

Lepidoptera.—An interesting, and perhaps important, case of what seems to be an accidental introduction of an insect has recently been recorded (*Ent. Mo. Mag.*, **66**, 1930, 224–50), namely, that of *Pieris rapæ* L., the Small White Butterfly, into New Zealand. It was accidentally introduced into Canada in 1860, and has since spread through Canada and the United States. More recently (1897) it was introduced into the Hawaiian Islands.

L. E. S. Eastham, following up a paper already mentioned (*hic op.*, **89**, 1928, 58–9) on the embryology of *Pieris rapæ*, deals with the organogeny of the same insect (*Phil. Trans. Roy. Soc. London*, **B 219**, 1930, 1–50, 9 plates). The development of body from the gastrulation stage to the complete caterpillar is described as well as appendage and head development.

The pleural and sternal sclerites of the Lepidopterous thorax have been studied in some detail by H. H. Shepard (*Ann. Ent. Soc. America*, **23**, 1930, 237–60).

Sibyl Wachter (*Ann. Ent. Soc. America*, **23**, 1930, 381–9), in an investigation of the moulting of the silkworm, has described the various physiological processes and also the moulting glands.

Hemiptera.—A good book on the biology of the Hemiptera (*Biologie der Hemipteren, Biol. Studienbücher*, **11**, vii + 543 pp., 329 figs., Berlin: Julius Springer, 1930), by H. Weber, has recently been published. W. E. China (*Insects of Samoa*, Part II, fascicle 3, 1930, 81–162) deals with the whole of the Hemiptera-Heteroptera, with the exception of the Miridæ and the aquatic families which have already (fascicle 2) been dealt with by T. Esaki.

J. I. Roberts, in a study of the Tobacco Capsid (*Engytatus volucer* Kirk.), has found that the macerated contents of its thorax induces the rolling and puckering of the leaf surface of tobacco which is termed "crinkle" (*Bull. Ent. Res.*, **21**, 1930, 169–83). The extreme asymmetry of the male external genitalia of this Capsid is interesting.

An investigation of the anatomy and histology of the alimentary canal of the cotton flea hopper (*Psallus seriatus*) has been carried out by R. H. Painter (*Jl. Agric. Res.*, **40**, 1930, 485–515). The effect of its feeding on plant tissues also received attention, and the presence of cell inclusions near the

site of the puncture is noted. These inclusions, which apparently are injected by the hopper, do not move far from the point of injury.

A neat method of artificially feeding *Myzus persicæ* has been described by Marion A. Hamilton (*Ann. Appl. Biol.*, **17**, 1930, 487–92). Sally Hughes-Schrader (*Ann. Ent. Soc. America*, **23**, 1930, 359–80) has made a further contribution to the life-history of Iceryine Coccids, with special reference to parthenogenesis and hermaphroditism.

Hymenoptera.—A continuation of the work of A. H. Hamm and O. W. Richards on the biology of the British fossorial wasps has recently appeared. Their first paper on the British Crabonidæ (*Trans. Ent. Soc. London*, **74**, 1926, 297–331) was published in 1926. The present paper (*loc. cit.*, **78**, 1930, 95–131) deals with the families Mellinidæ, Gorytidæ, Philanthidæ, Oxybelidæ, and Trypoxylidæ.

W. H. Thorpe, in a study of the parasites of the pine-shoot moth (*Bull. Ent. Res.*, **21**, 1930, 387–412), enumerates twenty-eight species of primary and secondary parasites, but of these thirteen are probably of little economic importance, while two are hyperparasites, and at least two are liable to live as hyperparasites. The dominant parasites in this complex appear to be the Braconid, *Orgilus obscurator*, and two Ophionines, *Cremastus interruptor* and *Omorgus mutabilis*. The salient facts in the biology of each species are mentioned, and the diagnostic characters of the adults are given. J. W. Evans (*Jl. Council Sci. and Indust. Res. Australia*, **3**, 1930, 106–16) discusses the possible utilisation of *Trichogramma* for the control of the Codlin moth in Australia.

F. Ökland (*Biol. Zentralbl.*, **50**, 1930, 450) shows how important is the inter-relationship of the redwood ant (*Formica rufa*) and aphids. The quantity of aphis-sugar collected by a colony of ants during a season is estimated to be 10 kgm.

Diptera.—W. J. Cavanaugh and J. E. Tilden (*Ecology*, **11**, 1930, 281–7) have made a study of the midge-fly, *Tanytarsus dissimilis*, in which they show the importance of algæ as a basic food, and also that the larvæ, which are not plankton feeders, exercise selection in collecting their food. V. B. Wigglesworth (*Q.J.M.S.*, **73**, 1930, 593–615) has found that in the larvæ of mosquitoes the secretion from the cells of the cardia, in the proventriculus, is drawn through an annular press and thereby moulded to form the peritrophic membrane. A detailed description of this press is given. Analogous structures were found in most of the main orders of insects. Mary V. F. Beattie (*Jl. Ecology*, **18**, 1930, 67–80) has been studying some physico-chemical factors in relation to mosquito prevalence in ponds, and has come to the conclusion

that *p*H, hydrogen sulphide, organic nitrogen, and dissolved oxygen have some bearing on mosquito breeding. Lucy J. Howland (*loc. cit.*, 81–125) has made a bionomical investigation of the larvæ with special reference to their algal food. She finds that Culicines eat more algæ than the Anophelines.

In two short but important notes (*Jl. Econ. Ent.*, **23**, 1930, 322–6 and 326–8) R. H. Painter deals with biological strains in and unisexual progenies of the Hessian fly. There is a full account of the biology of the gall midges whose larvæ destroy the seeds or prevent their formation in Meadow Foxtail grass by H. F. Barnes, in *Ann. Appl. Biol.*, **17**, 1930, 339–66. He has also made a study of some of the factors governing the emergence of gall midges, with a view to determining whether any such factors govern the emergence of their parasites to the same extent (*Proc. Zoo. Soc. London*, 1930, 381–93). A second paper on the zoophagous gall midges of the world by the same writer (*Bull. Ent. Res.*, **21**, 1930, 319–29) deals with those attacking Tingidæ, Psyllidæ, Aleyrodidæ, and Coccidæ.

A paper on the bionomics of some Tabanidæ, by A. Stone (*Ann. Ent. Soc. America*, **23**, 1930, 261–304), includes some keys, which should be useful, for the separation of the immature stages. In Part V, fascicle 2 (1930, 197 pp.), of *Diptera of Patagonia and Southern Chile*, which is being issued by the British Museum, Miss D. Aubertin deals with the Stratiomyidæ, O. Kröber with the Tabanidæ, and F. W. Edwards with the Bombyliidæ, Nemestrinidæ, and Cyrtidæ. In another fascicle (Part V, fascicle 1, 1930, 92 pp., 3 plates) in the same series, M. C. van Duzee deals with the Dolichopodidæ. An important taxonomic revision of the British species of Sphæroceridæ (Borboridæ) has been made by O. W. Richards (*Proc. Zoo. Soc. London*, 1930, 261–345). The latter part of the paper deals with the biology of the family.

A remarkable new nematode which is parasitic in the frit fly attacking oats has been described by T. Goodey (*Phil. Trans. Roy. Soc. London*, **B 218**, 1930, 315–43). The females enter the frit larvæ and remain within the host through its metamorphosis, becoming parasitic in the abdominal cavity of the adult flies. The presence of the parasite causes sterility of the fly.

F. G. Holdaway and A. C. Evans (*Nature*, **125**, 1930, 598–9) show that parasitism may be a stimulus to pupation in *Lucilia sericata* Mg. Another fascicle in *Insects of Samoa* has recently appeared. In this (Part VI, fascicle 5, 1930, 215–37) J. R. Malloch deals with the Ortalidæ and the Calliphoridæ. G. S. Graham-Smith (*Parasitology*, **22**, 1930, 47–115) has made a detailed study of the anatomy of the proboscis of the blow-fly, *C. erythrocephala*, and of the functions of the structures found

in it. Particular attention is paid to the different methods in which the fly can feed, viz.: it can filter out the larger particles of its liquid food, or can scrape surfaces with its prestomal teeth after first moistening them with vomit or saliva, subsequently ingesting the emulsion so produced, or it can suck up materials, such as sputum or fæces containing helminth eggs, without the use of its filtering apparatus or its teeth. Significant variables in the blow-fly environment are dealt with by R. A. Wardle (*Ann. Appl. Biol.*, **17**, 1930, 554–74). He finds that the resistance of the environment to the potential abundance of the pre-imaginal life-cycle stages is most pronounced during the first larval and the prepupal stages. The variables that limit the potential abundance are considered in detail.

D. Keilin and P. Tate (*Parasitology*, **22**, 1930, 168–81) have made anatomical studies of the buccal-pharyngeal armature of certain Anthomyid larvæ.

A very useful book (*Les Tsé-tsés*, vol. I, xiv + 742, 327 figs., 15 coloured plates, 1929, Brussels) has been prepared for tropical workers by E. Hegh. It is really a compilation of all that is known concerning the anatomy, systematics, reproduction, pupal sites, and predaceous enemies and parasites. The plates and figures are good, though very many of them have already appeared in other works. The second volume is to contain, among others, chapters on the distribution of the flies, biology of the adults, the effect of external factors, and means of controlling the flies. T. A. M. Nash (*Bull. Ent. Res.*, **21**, 1930, 201–56) has made a valuable contribution to our knowledge of the bionomics of *Glossina morsitans*. The fly density is studied in relation to the season, and its preference for the different vegetational communities has been worked out. It was found that only large game movements caused noticeable effects upon the fly community. From various field and laboratory experiments it would appear that *Glossina* hunts its food entirely by eyesight, and that, having sighted the moving object, the stimulus of smelling induces the reflex of probing.

Other Orders.—The Ray Society has done a valuable service to students of Odonata by publishing a volume by W. J. Lucas on the aquatic or naiad stage of British Dragonflies (*The Aquatic (Naiad) Stage of the British Dragonflies*, xii + 132 + 70 pp., 35 plates, 1930). Keys are given for the separation of species as well as detailed descriptions. A vast amount of interesting notes are also given. The plates are excellent.

The migratory habits of the Mayfly, *Blasturus cupidus* Say, are dealt with by F. Neave (*Ecology*, **11**, 1930, 568–76). As a result of the migration the nymphs are brought into a new environment, physically, chemically, and biologically, which is

a disadvantage. The advantage, on the other hand, lies in a plentiful food supply and a temporary freedom from predaceous enemies.

The bionomics of *Frankliniella insularis* Franklin in the Adelaide area have been dealt with by J. Davidson and J. G. Bald (*Bull. Ent. Res.*, **21**, 1930, 365–85). An interesting technique for rearing the insects is described. This species has been shown previously to be able to transmit the virus of spotted wilt disease of tomatoes. The British South Africa Company has issued (Publication 1, 1930, 56 pp., 8 plates) an account of the South African Citrus Thrips in Rhodesia by W. J. Hall. The bionomics and control, together with the distinguishing marks of damage by this insect, are dealt with fully. A. Körting (*Zeit. f. angew. Ent.*, **16**, 1930, 451–512) has made a valuable contribution to what is known concerning the bionomics of various corn thrips.

Enid K. Sikes (*Parasitology*, **22**, 1930, 242–59) has described the life-history of *Ceratophyllus wickhami*, and the external morphology of its third larval instar in detail.

D. Keilin and G. H. F. Nuttall (*Parasitology*, **22**, 1930, 1–10) have published some iconographic studies of *Pediculus humanus* that were intended for a monograph, which has been abandoned, on the lice infesting man.

ARCHÆOLOGY. By E. N. Fallaize, Royal Anthropological Institute, London.

Archæology was well represented in the programme of the Anthropological Section of the British Association at the Bristol meeting in September last. A report of the proceedings appears in the November number of *Man*, from which in part the following notes are taken. The Presidential Address, by Dr. H. S. Harrison, on "Evolution in Material Culture," was a contribution to the discussion of the rival theories of diffusion and independent origin of permanent value. While not denying the possibility of independent origin in the case of simple inventions and discoveries, he showed himself a staunch diffusionist. He gave a much-needed precision to the terms "discovery" and "invention," and by an analysis of the psychological factors which underlie the latter, demonstrated the inherent improbability of the sequence of coincidences demanded by the theory of independent origin of any given material product. In regard to the question of America he asked why, if the culture of the American Indian was the result of independent origin, it stopped exactly where it did in many cases, and had not advanced equally with its parallels in the Old World. If the American Indian had discovered cereal

culture, pottery-making, and metal-working, why had he not gone on to invent the plough, the rotary quern, the potter's wheel and iron? Dr. Harrison inclined to look to South-Eastern Asia for the place of origin of most of American culture.

The most important item of archæological interest in the programme was undoubtedly a joint discussion, in which the Geographical, Geological, and Anthropological Sections took part, dealing with the relation of pluvial and glacial periods in prehistoric times. Evidence was brought forward from Europe, East and South Africa, America, Palestine, and China. Evidence pointing to two major pluvial periods, with a dry period intervening, was cited from the Faiyum by Misses Gardiner and Caton-Thompson, from East Africa by Mr. L. S. B. Leakey, and from South Africa by Mr. A. Leslie Armstrong. As the result of her recent excavations in Palestine, Miss D. A. E. Garrod has been led to identify a relatively late pluvial period in that area. Mr. C. E. P. Brooks, referring to Dr. Simpson's theories of intense variations in solar radiation, gave the meteorological grounds for the correlation of pluvial and glacial periods—the first pluvial of the pleistocene being correlated with Günz and Mindel, and the second with Riss and Würm. In China, Prof. F. G. Barbour stated, study of the superficial deposits of late Pliocene and Pleistocene indicated two periods of aggradation separated by a period of erosion. The second aggradation, that of the Middle and Upper, was that of the great loess development; but loess also occurs in the upper levels of the basal pleistocene earlier series. The loess is formed in semi-arid conditions, and it would seem that periods of low temperature in Europe and America correspond with those of semi-aridity in North China. As far as present palæontological data go, these drier times correspond to the twin glacial epochs Günz-Mindel and Riss-Würm in Europe. Mr. L. A. Cammiade brought forward evidence from the Eastern Ghats and the Coromandel coast of India based on the formation and erosion of laterite which agreed with the view taken by Prof. Barbour.

In this connection we may leave the proceedings of the British Association for the moment to refer to a paper by Mr. L. A. Cammiade and Mr. Miles Burkitt, which appears in *Antiquity* for September, entitled "Fresh Light on the Stone Ages in South-Eastern India," in which the question of the evidence from the laterite and associated stone-age industries is developed in more detail. On stratigraphical and typological grounds a succession of four different cultures is distinguished: (1) an industry characterised by the presence of hand-axes of various types which can be closely paralleled

among various finds in South Africa ; (2) flake industries with more neatly made hand-axes ; (3) industries characterised by the presence of slender blades with blunted backs, a few burins, planing tools and end scrapers ; (4) microlithic industries. From the evidence of the laterite a climatic sequence has been worked out in which a long damp period is marked by the formation of laterite, and shows no human relics. This is followed by a long dry period, in which the people of the hand-axe cultures settled on the laterite plain. A period of violent rain followed, in which the weathered laterite was swept down and redeposited, the hand-axe culture disappeared, and the country was largely depopulated. A dry period followed, in which the people of the flake industry settled on the beds of detrital laterite and clay. A further wet period followed, less violent than the preceding, in which the alluvium was deposited. Herein occur the flake industries of series 2. A decrease in rainfall was followed by a period of denudation leading up to the conditions of to-day. Industries 3 and 4 are associated with these last periods. This succession of changes can be correlated in general with the climatic changes in Kenya, while the series of industries have exact counterparts in Africa, especially South Africa. The hand-axes, for instance, are the type of tool first recognised at Victoria West. In many ways the industries of series 4 are closely allied with the Wilton culture of South Africa, which is also found in Kenya and Uganda. The distinct rarity in India of industries of Upper Palæolithic type, which occur in South Africa, are common in North and East Africa, and range as far north-east as Transjordania, suggests to the authors that in Southern India we are on the periphery of Upper Palæolithic civilisation.

On this last point it is possible that further light may come from farther East. In a recent number of the *Proceedings of the American Philosophical Association* (vol. 69, No. 4), Prof. Roland B. Dixon gives a brief account of what would appear to be a remarkable archæological discovery in the Philippines, where Prof. Bower, of the Philippine University, has recovered some thousands of archæological specimens from a prehistoric village and cemetery site, which was first brought to light in 1926 by the damning of a river in the course of engineering operations. It is said that these antiquities range in date from the Iron Age to the Upper Palæolithic or even the Mousterian. The classification of the stone implements, however, seems to be purely typological, and no information as to stratigraphy is yet available.

Returning to the proceedings of the anthropologists at Bristol, Mr. A. L. Armstrong, in discussing the bearing of the geological and archæological evidence from the Victoria Falls

on the problem of the antiquity of man in Africa, suggested that it demonstrated the relation of the stone implements to pluvial and arid periods in Africa, and thought it possible that if the pluvial periods are to be held contemporary with the glacial epochs of Europe, the African series of Lower Palæolithic implements is probably a whole period earlier than the European parallels. Mr. L. S. B. Leakey reported finds of pottery *in situ* in the lower part of the Upper Aurignacian deposits of Kenya beneath a series of undisturbed strata. The pottery apparently was made by lining a basket with clay and then burning off the basket.

Among the papers on British archæology, reference may be made to the investigations at Barnwood, near Gloucester, described by Mrs. E. M. Clifford. The lower gravels of the site are rich in a mid-pleistocene fauna. In the gravels and brick-earths Aucheulian, Mousterian and Aurignacian implements have been discovered. No human remains were found in these strata; but burials of a later date—a beaker burial, La Tène II, and an extensive Roman cemetery of the first and second century A.D.—were examined. In the last-named over a hundred inhumations and thirty cremations were found. The evidence suggests that the people buried here were native British, and the site is thus of considerable evidential value as to the culture of the West when it was reached by the Romans.

A distinguished foreign guest of the Association, Dr. Miloje M. Vassits, of Belgrade, gave a description of his excavations at Vinča, a neolithic site on the Danube, in the current year. A full report of the paper also appears in *Man* for November (No. 141). The excavations were continued on the same area as had been excavated in 1929. At various parts of the site and at various levels remains of buildings which had contained clay stoves have been found. These buildings are rectangular with roughly horizontal floors. The walls consist of frames on posts supporting wattle-and-daub walls. The floors were horizontal beams laid over with a mud plaster, the surface of which had been fired. Similar floors have been discovered at Tcherna-Voda in Bulgaria and in South-West Russia. In a building 7·45 metres below the surface, on a stand, a pithos was discovered, on which, over a chequer pattern and meander motives, rises a rough representation of a human face with modelled nose and mouth and eyes made by incised lines. Implements and weapons were of stone, bone, and horn. For small knives flints, obsidian, and other rocks were used. Axes, mainly unbored, were of various shapes and of various stones. The shoe-last celt, comparatively common in the later levels, was rare in the earlier. No sources of these rocks are known

near Vinča, and they must have been brought from a distance. The obsidian may have come from Bükk, in Hungary. A large number of figurines, both human and animal, were found. They are made of stone, bone, and clay. The human figures are both male and female, the latter in the majority. A hermaphroditic figure was found, steatopygous, with fully modelled abdomen and breast. The form of head suggests not a divinity but a masked person connected with some cult. Masked faces are common, and some show that the masks carried ox-horns. A prominent type of figurine has a conical cap. Those found in a good state of preservation are provided with male genitals. It is suggested that one type of these capped figurines may represent a warrior or sky-god. It is a type of wide distribution in the Ægean and neighbouring lands. Examples have been discovered as far apart as West Prussia and China. It has been dated to the fifteenth century B.C. in the Ægean, and in Cyprus to the second millennium before Christ. This dating would agree with its position at Vinča. A plume on the head of one characteristic figure definitely suggests the plumes on the head of a figure on a fresco in the palace of Knossos.

Knowledge of the pottery has been much enriched. In form there is a rich gradation from rough pithoi to fine drinking vessels. Prominent forms are a vessel mounted on a stem and painted red and the anthropomorphic vase. A rhyton, now known as the Hyde vase, is in the form of a fantastic bird. This, and another fragment of a similar kind, is ornamented with ribbons of black material which burns in the flame of an acetylene lamp. The nearest analogy to this material is from Bükk. A special form of pottery resembles Minyan ware, if it is not indeed an offshoot of it—a further argument for the author's dating of Vinča at the beginning of Troy II. A sherd found at a depth of 6·5 metres bears a painted decoration for which parallels are to be sought in Cappadocian, Hittite, and Mycenæan ceramics. Vinča would appear to have had a form of civilised life superior to any between it and the Ægean. This was derived from the south-east, the Ægean, Asia Minor, and Cyprus. The author concludes that the relation was one of commerce and not of colonisation.

The October number of *Man* is a special Indian number devoted to certain aspects of the archæology of India. It is the work of a committee of the Indian Research Committee of the Royal Anthropological Institute, which is investigating the archæology of the Indian bead. Mr. Horace Beck contributes "Notes on Sundry Asiatic Beads," which is illustrated by two plates, one of which is in colour. Mr. Beck describes a large number of beads from Southern India, Sarawak, and the

Federated Malay States, and discusses their origin, affinities, and analogues; many are of considerable antiquity. Mr. L. A. Cammiade describes a number of sites in the Madras Presidency on which he excavated urn-burials, and discovered some of the beads described by Mr. Beck. The number concludes with a paper by Mr. K. de B. Codrington on "Cairn- and Urn-burials," in which he makes a general survey of the pottery forms of the cairn- and urn-burials found in Southern India and the Deccan, and concludes that the evidence on the whole favours unity of culture in South India. The pottery wares fall generally into two classes—coarse unpolished wares used for the urns and large vessels, and a fine polished ware either black, black-and-red, or all red. The polished black wares are closely paralleled by the brightly polished black ware found on the classical sites of North India, Taxila, and Bhita. The types of burial are analysed, and as regards burial customs, Mr. Codrington comes to the conclusion that the ceremonies must have been of a dual nature. In some cases the entire body was placed in the urn, in others only the skull and long bones. The urn-burials represent only the final stage of the ceremony, and probably were of a communal character.

The recently published *Journal of the Royal Anthropological Institute* (vol. 60, pt. i) contains a number of communications on archæological subjects. The Rev. Neville Jones writes on the occurrence of implements of rostro-carinate type at Hope Fountain, Rhodesia. Plaster casts of the implements were made and forwarded to Mr. J. Reid Moir, by whom the drawings were made for the plates which illustrate the paper. Mr. Jones considers that the occurrence of this form of implement at Hope Fountain suggests the identity of the race that evolved it in South Africa and Europe.

Mr. Ernest Mackay, writing on painted pottery in modern Sind, suggests that in the small village of Balreji, situated near the ancient site of Mohenjo Daro, the pottery industry, which in its type of wheel and in other respects differs from the potting of the rest of India, and also in certain features resembles some pottery types of the ancient East, may represent a survival of the ancient culture of the Indus valley, of which the now famous sites are being excavated by the Archæological Survey. Mr. Daryll C. Forde writes on the use of greenstone (Jadeite Callais, etc.) in the megalithic culture of Brittany. A communication of considerable controversial interest, by Mr. H. J. E. Peake and Prof. H. J. Fleure, "Megaliths and Beakers," puts forward views which are in opposition to theories at present current. They argue, as regards the megalith, for a megalithic evolution based on ideas that are analogous to those

behind the Messara civilisation of Crete—an evolution attended by simplification and reduction as well as local specialisation. They hold that the megalithic idea was carried via the Iberian coasts to Brittany, Ireland, and the Baltic. Interior France and parts of Britain may represent another extension zone of south-east Spanish influence. This then is why all ideas may not have come the same way. It is noted, for instance, that South France and the Marne have stones sculptured with a female figure and so has Guernsey, but these do not occur in either Morbihan or Finistère, Brittany. As regards the beaker culture, Sir Arthur Evans and others have thought that the bell-beaker was evolved in the Iberian peninsula, and that it originated from pre-existing pottery and from the tradition of esparto grass vessels. It is here suggested that the beaker was at least standardised in Central Europe, and owes its origin to influences around the fringe of the painted pottery in the south-east. It is further suggested that it reached out to Denmark, Holland, England, and Spain by land, making contacts in each region with the megalithic cultures which had spread mainly by sea-routes. In *Man* for November, No. 142, Prof. Gordon Childe stresses the desirability of working out a pedigree for the bell-beaker culture on the lines suggested by Fleure and Peake, and says that as the result of a careful examination of large collections in Continental museums, he has had it forced upon him that the bell beakers with their associated culture stand out as a closed group in marked contrast to all the neolithic and early bronze age cultures there represented.

Another paper in the *Journal* of no little interest is by Mr. Blake Whelan. It deals with the flint industry of the Northern Irish (25 ft.) raised beach in its relation to the Asturian industry of Portugal. His conclusions are that the period of deposition of the raised beach included the local post-glacial optimum, and that the Spanish Asturian is referable to a similar period. The contents are the sweepings of an earlier horizon, probably of boreal date, a period of marked elevation in Southern Ireland. The Asturian culture of the coast of Western Europe exhibits a northward trend which may well have brought the race by land to Ireland in late boreal or early Atlantic times. The Irish industry is pre-Neolithic in every feature, but is not related to the early county Antrim culture of Aurignac type, also pre-Neolithic. The Portuguese Asturian stations suggest comparison with Irish sites. The Irish raised beach artifact represents a somewhat crude translation of the Iberian quartzite implement into flint. In the leading implemental types of the peninsula the cultures correspond so closely as to establish close relation if not identity.

At a meeting of the Peking Geological Society in July last, Dr. Davidson Black announced the discovery of a second skull from the caves at Chou Kou Tien. A brief account of the circumstances of the discovery and of the skull is given by Prof. G. Elliot Smith in the *Scientific American* for November. The fragments were discovered in material which had been taken from the cave in the previous October, and was being examined in the laboratory of the Geological Survey. With them were associated a number of teeth, presumably belonging to the same individual. Dr. Black is of the opinion that it is a young adult male. The greater part of the vault and a portion of the base of the skull have been recovered. It is said to yield valuable information as regards certain parts, especially the sphenoid and nasal bones. The frontal bulges are not so prominent nor the bone so thick as in the first skull. Prof. Elliot Smith points out in his article that both *Pithecanthropus* and the Piltdown man were discovered in gravels, and that though there can be little doubt that the heterogeneous fossils discovered with them were deposited at the same time, in the case of Peking man there can be no possible question that the bones of animals deposited alongside of them provide absolutely certain data for the estimation of their age. Dr. Davidson Black is of the opinion that Peking man represents an early or Proto-Neanderthal man, but Dr. Hrdlička in a survey of the skeletal remains of early man, recently published by the Smithsonian Institution, is not prepared to differentiate it from the Neanderthal group.

ARTICLES

SYMBIOSIS: PROLEGOMENA TO THE STUDY OF ŒCOLOGY

By JAMES BAKER, B.Sc., A.R.C.S., University, Cambridge

Part II

GENETICALLY HETEROGENEOUS SYSTEMS

To associations involving unlike individuals a false colour is often lent by the greater evolutionary initiative of one group over the other. This is well shown by the remarkable case of the crab *Eupagurus*, which has attached to its shell one or two individuals of certain sea-anemone, *Adamsia palliata*. When the crab, which is a hermit, has occasion to change its shell, it is said to pluck the anemone from the old resting-place, hold it aloft in its pincers, and subsequently to plant it again on the outside of its new abode. Of this association, Stebbing[1] remarks: "Surmises are sometimes made as to the advantages which the companions may hope to obtain from the alliance. The anemone may obviously obtain a greatly increased range for supplies of food, by the superior locomotive power of the hermit, and though the weight of both anemone and shell may seem an unnecessary encumbrance to the crustacean, that objection is gradually diminished by the circumstance that the anemone in course of time almost entirely absorbs the shell. On the other hand, the presence of the anemone may be a very valuable protection to the hermit, since numerous fishes are in the habit of swallowing these recluses shell and all, merely spitting out the shell after they have digested its inmate. But it is most probable that to many fishes an *Adamsia palliata* would be by no means an agreeable morsel. It is also not unlikely that the anemone may contribute to the commissariat by throwing out its darts as some swift-gliding shrimp passes by, and thus reducing it to a condition in which it can be captured by the pagurid."

[1] Stebbing, *A History of Recent Crustacea*, 1893. Kegan Paul, pp. 167, *et seq.*

Now, if this were an isolated instance among the crabs, or even an exceptional one, we should be disinclined to attribute to it any particular significance beyond that of an effort upon the part of an enterprising species of crab to further its own ends; and for the evident advantages that accrue to the anemone from the association, we should be disposed to invoke the operation of fortuitous circumstances. Further acquaintance, however, with the pagurids renders this view untenable. Again and again we find the hydroids involved in different ways with crabs in a manner that suggests advantage sometimes to one group, sometimes to the other, and not infrequently to both. Another hydroid, this time a colonial one (*Epizoanthus americanus*), affixes itself to the gastropod shells which harbour the hermit crab *Eupagurus pubescens*. "First a single polyp finds lodgement, and as its basal membrane spreads over the shell, buds arise from it forming fresh polyps, and gradually this same membrane absorbs the shell though retaining its spiral shape. In this absorption there is a great advantage to the hermit, because as it grows its increase of bulk still finds room in the yielding polyp mass, without any necessity arising for further change of domicile." In this case, unlike that of *Adamsia palliata*, it would be impossible to claim that the association was established by an overt act upon the part of the crab, for the Eupagurus *appears* to play a rôle just as much passive as that which the anemone played in the first instance. Sometimes the alliance is still more involved, for the gastropod shells that harbour yet another pagurid, *Catapagurus Sharrerii*, are colonised first by *Epizoanthus americanus*, which replaces the hard structures of the shell, and then by the simple polyps of *Adamsia sociabilis*, which establishes itself on the surface of the Epizoanthus. Further careful observations on this subject have been made by Aurivillius,[1] who has found that between certain hermit crabs and their attendant hydroids there exist relations a good deal more refined than had previously been supposed: "*Hydractinia echinata* is frequently found coating the outside and inside of various shells that are occupied by hermit crabs. The mode

[1] Aurivillius, C. W. S., *Kongl. Svensk. Vetensk. Akad. Handl.*, 1891, Bd. xxiv, pt. 9, pp. 1–37, Stockholm.

I have not had an opportunity of studying this paper, but Miss Gordon, of the British Museum, writes me as follows:

". . . From the plates it would appear that the shell tenanted by the pagurid is sometimes completely overgrown with either a sponge or a hydroid colony. The shell is thus greatly enlarged: the figures leave no doubt as to the scientific accuracy of the observations. In fact, since then polyzoa and sea anemones also have commonly been found associated with hermit crabs, scientists are generally agreed also that this partnership is of advantage to both animals, though the actual extent of this benefit is probably a matter on which all would not quite agree."

of growth is such that the hydroid not only repairs in effect the damaged mouth of the shell, but also frequently extends its boundaries. This is especially the case where shells are few and where the growing pagurid might be put to much inconvenience to find a larger lodging. By the extension of the hydroid colony, which sometimes gives a quite monstrous appearance to the shell, the hermit is saved the trouble of making any change of abode. The hydroids are saved from the danger and damage they would be exposed to from the rolling about of an empty shell. On the inside of it they do not develop any of the nutritive polyps which might incommode the hermit and also suffer injury from its movements, but they line the interior with a network, to the satisfactory smoothness of which the hermit itself contributes. It might be supposed that this was effected by the friction of its body, but Aurivillius shows that there is a secretion from the glands in the side of the carapace adapted for the purpose, and that the joints in the fourth and fifth pairs of the legs of Eupagurus are nicely arranged to assist in distributing the secretion."

The discussion of these forms will serve at least to make us more careful before we attribute a merely chance significance to the relations between, say, the crocodile-bird (*Pluvianus ægyptius*) and its host, or even to those between the crow and the sheep from which it assiduously picks off the ticks—relations which are probably neither much more nor much less " fortuitous " than the association of the Eupagurus with its captive anemones. In this connection it is rather interesting to find that the African tick-bird (*Buphaga Africana*) is so closely associated with the ungulates (*e.g.* the hartebeest buffalo), from which it derives its food, that it makes its nest almost exclusively from the hair of these animals.[1] To the rhinoceros, moreover, it serves as an efficient sentry, warning the beast of approaching danger.

As a rule due weight has not been attached to the reciprocal relationship that exists between the two members of an association, and the distinction between " parasite " and " host " has been over-emphasised, often, as in the case of the insect-galls and the so-called " ant-plants," on quite insufficient, or even radically contradictory, evidence. Among the lichens, for example, the view was for a long time held that the fungal elements are parasitic upon the gonidia, the latter deriving no advantage from the association. This state of affairs was summed up by Warming (1902) in the world " helotism,"[2] of

[1] Percival, A. B., *A Game Ranger's Note Book*, London, 1924, p. 344; Borrodaile, L. A., *The Animal and its Environment*, London, 1923, p. 109.

[2] Warming, E., *The Œcology of Plants* (transl. Groom and Balfour, Oxford Univ. Press, 1926), p. 85.

which the sense is sufficiently obvious. However, the more accurate investigations of Chodat [1] and the school of Geneva have shown the error of this assumption: that the algæ concerned are by no means identical with those that occur freely in nature and that in many cases they require, or at least decidedly prefer, a medium containing those organic nutrients which would normally be supplied by the fungus. Thus, to take an example: of the known species of Nostoc that occur in the wild condition, those that have been examined appear quite indifferent to the presence of sugar in the medium on which they are grown. Bouilhac, in fact, found that *Nostoc punctiforme*, which he was studying, was killed by sugar above a concentration of one per cent. *Nostoc Peltigeræ*, on the other hand, a lichen-forming species, shows a very decided preference for a medium containing organic carbon, upon which it not only grows more rapidly, but the individual cells are larger and better developed. Moreau,[2] indeed, studying other aspects of the association, has even gone to the extent of claiming that the alga is parasitic upon the fungus and that the lichen represents a "gall" due to the infection of the fungal mycelium by parasitic gonidia. He says: "Nous sommes donc conduits à considerer que, dans un Lichen, l'Algue et le Champignon se comportent comme un agent infectieux et son hôte, l'agent infectieux imposant a l'hôte des modification morphologiques. Un Lichen nous apparait comme une organisme déformé par son parasite, comme l'est un vegetal attient par une galle, une Euphorbe parasitée par un Uromyces; toute la partie aerienne d'un Lichen ordinaire est une galle, une cecidie, une algocecidie; c'est une biomorphose generalisée . . . Les formes speciales prises par le Champignon sont la traduction de la virulence de l'Algue, les cadavres de gonidies dans les zones necrales temoignent de la severité des ripostes du Champignon."

When all the evidence is taken into consideration it is difficult to escape the conclusion that the evolution of the Lichens has involved *pari passu* modifications among the two active groups that are concerned in their formation, even though in particular cases the one or the other of the symbionts may exert a preponderating influence.

The conditions that prevail among the ants are much the same, for, as we have previously pointed out, the beetles and other insects that go to make up the community are largely blind (*Claviger*, *Atmeles*, *Beckia*, *Platyarthrus*) and—though some, like *Atmeles emarginatus*, are capable of carrying on an

[1] Chodat, R., *Monographies d'Algues en culture Pure* ("Materiaux pour la flore cryptogamique suisse"), 1913 t. iv, fasc. 2.

[2] Moreau, F., *Les Lichens* (Le chevalier). Paris, 1928, p. 110.

autonomous existence—others have become dependent upon their hosts. In certain cases they have even become adapted to a single species of ant. V. Hagens, moreover, considers that, among those beetles which frequent the nests of two or more species of ant, varieties are from time to time produced. " . . . he has observed that the specimens of *Thiasophila angulata* in nests of *Formica congruens* are darker than those found with *F. exsecta*. *Hetærius sesquicornis* found with *Lasius niger* and *Tapinoma erraticum* are smaller than those which occur in the nests of larger ants; and the form of *Dinarda dentata*, which is met with in the nests of *F. sanguinea*, has rather wider wing-cases than the normal type."[1] Similar specialisation is presented by those ants which are associated with certain genera of tropical plants —described technically as " myrmecophytes." *Allomerus*-8-*articulatus*—one of the commonest guest-ants of *Cordia nodosa*—is known only from this plant and a few related species. " It is characterised by building earthern galleries which extend from the entrances of the " domatia," down the outer surface of the branches and stem, to the level of the ground.[2] Another " obligatory " guest from the same plant is *Azteca alci* var. *Cordiæ*.

All these observations point towards the same conclusion: that the evolution of a heterogeneous system demands a certain measure of reciprocal reponse on the part of its component units or, to put the matter differently, that the system evolves as a *whole*, just as the homogeneous cell-community of the metazoan has developed as a discrete entity. In each case the symbiotic impulse is manifest as a tendency towards new groupings, apparent at first as loose complexes, only later taking on the semblance of true individuals, and this only if their evolutionary impetus can survive the dissipating influences that attend their early development.

In an earlier part of this paper we have stated reasons for regarding parasitism as of two kinds representing, respectively, the disturbance of a primitive equilibrium, and the failure to attain such an equilibrium. Now, it is very generally recognised among pathologists that the diseases of plants converge about two types of which one, the " parasitic," is represented by *Botrytis unerea* on the vine; the other, the " symbiotic," by *Puccinia graminis* on wheat. The characters which have led to this segregation are brought out by the following table:

[1] Lubbock, *Ants, Bees, and Wasps*, London, 1904.

[2] Bailey, " Notes on Neotropical Ant-plants." **111**, *Cordia nodosa Bot. Gaz.*, 1924, lxxvii, 32–48.

Botrytis Type.	*Puccinia Type.*
Non-specialised: pleophagous, readily cultivated on artificial media, attacks the whole of the host plant, kills the host cells, no hypertrophy. Usually attacks the host under conditions unfavourable to the latter.	Highly specialised, cannot be cultivated except on its host, attacks only a limited area of the host. No killing except at a late stage, hypertrophy due to hyperplasy. Attacks host only when the latter is growing well and healthily.

In some groups of fungi, as in the Peronosporineæ, to which we have previously referred, the transition from one type to the other can be followed among different species, and there is every reason to believe that the Botrytis type is, in this instance, primitive, the Puccinia type derivative—both of them steps in the direction of a still more refined relationship, of which the final shape can be seen in the endotrophic mycorrhiza of the Orchidaceæ and Ericaceæ. Whereas diseases of the Botrytis type, primitive diseases in which the fungus is unspecialised, manifestly "parasitic," and usually lethal, are as a rule not accompanied by any decided morphological changes, those of the Puccinia type, which involve a delicate balance between the host tissue and that of the parasite, are for the most part accompanied by hypertrophies or changes in shape due to an unusual activity of the cells in the infected area—*i.e.* the host and the parasite are alike active in giving rise to a "gall." That this mutual interaction may become remarkably intimate is shown by the case of *Ustilago Treubii*, parasitic on the Euphorbia, with which it gives rise to pileate galls of such a form that strands of host tissue traverse the spore mass and serve as a "capillitium" in the dispersal of the spores. Even more striking is the relation that obtains between *Urocystis violacæ* and its hosts. This fungus is parasitic on the anthers of the Caryophyllaceæ, giving rise to characteristic "smut" galls; of the genera which it attacks, however, several are diœcious—the male and female flowers being produced on different plants—nevertheless, when the *Urocystis* chances to infect a female plant the male organs are anomalously developed and serve to harbour the black spore masses of the parasite. This relation is closely paralleled by that of the crustaceans *Sacculina* and *Bopyrus*, which are parasitic on several species of hermit and other crabs (*e.g.* *Inachus*), and, when they chance to infect a male, cause it to assume the external characters of the female, *i.e.* broad abdomen, egg-bearing abdominal appendages, and shape of chelæ. Moreover, if the crabs survive the attack, the male gonads regenerate as hermaphrodite organs producing both ova and spermatozoa. If it is the female that is parasitised, the gonads, though atrophied in the same way, eventually recover their

original form. There is good reason, therefore, to believe that the Sacculina is capable of influencing its host in such a way that its metabolism changes from the type of the male to that of the female, and that this change is beneficial to the parasite by "enabling it to obtain from the blood the nutriment which the female normally stores in her ova as yolk."[1] Again, in the unicellular galls of *Synchytrium papillatum* on *Erodium cicutarium*, an annular "thinning" of the wall towards the face of the host cell causes the upper part to become detached, carrying the fungus with it so that it can complete its life cycle on the damp earth.

Very interesting symbiotic relations are found among the bacteria. It is well known that, flowering plants being unable of themselves to assimilate nitrogen, they are dependent upon certain bacteria in which this capacity has been very highly developed. Were it not for the intervention of the latter, the available supply of nitrogen in the form of mineral salts would eventually be leached away by rain or lost irrevocably to the atmosphere by the activity of putrefactive organisms. Accordingly, it is not in any way surprising to find that the two groups have evolved symbiotically. Whereas there exist in the soil organisms, like *Azotobacter* and *Clostridium*, that live quite, or at least nearly, independent of larger vegetation, others have become more closely associated with flowering plants and at the same time more specialised—the strains of the nodule forming *Ps. radicicola* infecting particular species of leguminous crops.[2] The *Pseudomonas* can, and apparently does, live autonomously in the soil, where, however, it is not capable of carrying on the operation of nitrogen fixation independently[3]; while the "host" plants can be grown aseptically in culture solutions, though they are by no means so luxuriant under these conditions.

There is reason to believe that the final stage in the evolution of this association is found in two other families of flowering plants, the Myrsinaceæ and the Rubiaceæ, many of whose species have nodules on their leaves that are inhabited by bacteria. From the leaves of *Ardisia*, Miehe[4] succeeded in

[1] Borrodaile, L. A., *The Animal and its Environment*, London, 1923, p. 373.

[2] *Vide* Waksman, S., *Principles of Soil Microgology*, London, 1927, p. 136, where a list of leguminous plants is given, divided on the basis of inter-inoculation.

[3] During December 1929 and January 1930 respectively two papers have appeared describing independent examples on this point. The authors conclude that there exists absolutely no evidence of the capacity of *Ps. raticicola* for fixing nitrogen apart from its host. (*a*) Löhnis, M. P., *Soil Sci.*, xxix, January 1930, p. 37; (*b*) Allison, F. E., *Jour. Agr. Res.*, xxxix, December 1929, p. 893.

[4] Miehe, H., "Weitere Untersuchungen über die Bakterien Symbiose bei Ardisia crispa." *Jahrb. wiss. Bot.*, liii, 1913, pp. 1–54; lviii, 1917, p. 29. *Ber. Bot. Gesell.*, xxix, 1911, p. 156; xxxiv, 1916, p. 576.

isolating a nitrogen-fixing organism, *B. foliicola*, which, like *Ps. radicicola*, has both a flagellate motile form and a "bacteroidal" form. Unlike that species, however, the bacteria are present in the seeds between the embryo and the endosperm, so that the "disease" is hereditary. When the young plants grow, the organisms follow the growing-point and are eventually found distributed in the intercellular spaces throughout the whole plant. In the two Rubiaceous plants *Pavetta indica* and *Chomelia asiatica*[1] the bacteria enter through the stomata and form nodules on the leaves, where they live by fixing nitrogen from the air. They are found at all life-stages of the plant and pass into the seeds so that, again, the association is hereditary, indicating a much higher order of symbiosis than that found among the leguminosæ. Plants freed from bacteria, by warming for twenty-five minutes at 50° C., develop very slowly and suffer from lack of nitrogen.

The progression from the biological horizon of Azotobacter and Clostridium, across that of *Ps. radicicola*, to the advanced symbiotic association of the organism from the rubiaceous leaf nodules is reminiscent of the corresponding transition from the parasitic disease to mycorrhiza plants, through the medium of the "symbiotic" diseases.

Quite comparable are those galls produced by insects, commonly distinguished as "cecidia" or, more exactly, as "zoocecidia." All naturalists who have given their attention to these structures have been impressed by the manner in which the plant makes provision for the larvæ harboured within it. In the galls of *Eriophyes similis* on *Prunus spinosa*, the inner epidermis is beset with large "nutritive hairs," on which the larvæ feed voraciously. These hairs are rich in plasma, thin walled, and have an extraordinarily wide lumen: they represent "formless sacks full of food material."[2] Again, the galls produced by *Nematus pedunculi* on the leaves of the Grey Willow (*Salix incana*) are provided internally with a soft sappy tissue which is eaten by the maggots of the parasite but is rapidly replaced by proliferation of the underlying cells. Of these Kerner says: "The cells of the gall pith remain in a regenerative condition as long as the larvæ need nourishment. Just as new leaves and shoots spring up in a field that cattle have grazed upon, or in which the sward has been mown down, so, in the cavity of the gall, those layers of cells that have been eaten away are in a short while replaced from beneath."

Becher[3] has pointed out that whereas in general the nitrogen

[1] Zimmermann, A., "Über Bakterienknoten in den Blättern einiger Rubiaceen," *Jahrb. wiss. Bot.*, xxxvii, 1902, pp. 1–11.

[2] Becher, E., *Die fremddienliche Zweckmässigkeit der Pflanzengallen*, Leipzig, 1917, p. 24.

[3] *Loc. cit.*, p. 21.

content of a gall is low, in the immediate neighbourhood of the parasite the cells are rich in nitrogenous compounds. "Food materials are either fashioned by the special assimilating tissue of the gall or else they are led off from the ordinary active tissues of the organs out of which the gall has been engendered. To meet advancing requirements the conducting system in the neighbourhood of the gall becomes greatly enlarged, while in other cases new conducting tracts are laid down."[1]

It very commonly happens that the channel made by the insect in depositing its eggs is later on blocked up by a corky extravasation of the walls or by the growth of hairs round the sides of the orifice. In this case the insect can only emerge by boring its way through the tissues of the gall. Nevertheless, in many of those forms that pupate on the ground after passing their larval stage within the cecidium of a host-plant, special provision is made for this change of habit. A little "plug" of gall tissue falls out, apparently spontaneously, and provides a free passage for the larva that is about to pupate. This occurs, for instance, in the galls of *Cecidoses Eremita* on the twigs of the South American *Duvalia longifolia*, and, somewhat modified, in galls on *Quercus Cerris*, produced by *Cecidamia Cerris*. In other galls the same purpose is served in a different way, for example, those of *Tetraneura Ulmi* open by a lateral valve. "At a place about midway between the tip of the gall and its point of insertion, the wall bursts apart, the rims of the opening are thrust outwards, and the inhabitant has free access to the outer world. In the galls of *Pemphigus vesicarius* on the poplar there are several openings."

Porsch has summarised the ways in which galls conform to the requirements of their inhabitants as follows[2]:

1. The maggots are shut off from the outer world.
2. The inlet orifice is blocked up by growth of the cortical cells.
3. The gall is protected by the development of mechanical tissues (and sometimes by spines or prickles, Becher).[3]
4. Provision of internal air-space by the development of a special type of tissue (stellate parenchyma).
5. Frequent formation of special assimilating tissue.
6. The development of a steadily regenerating nutritive tissue.
7. Position of the assimilating tissue.

[1] Porsch, O., "Wechselbeziehungen zwischen Pflanze und Tier Allgemeine Biologie hrsg. v. C. Chun u. W. Johannsen unter Mitwirk v.a. Guntjurt," *Kult. d. Gegenw.*, **III**, 1915, Leipzig u. Berlin, p. 555.

[2] *Loc. cit.*, p. 553.

[3] Becher, E., *loc. cit.*, p. 47.

8. Diversion of the existing conducting tracts towards those of the gall.

9. Subsequent construction of new conducting tracts leading to the gall.

10. Richness of the gall tissue in tannin.

11. The provision of anatomically demarcated structures for the exit of the mature animal, together with mechanisms for opening them.

What we may term the "closeness of fit" between insect and plant sometimes reaches an extraordinary degree of refinement, particularly in those cases where the relationship is associated with cross-pollination. Of this kind is the well-known "complex" between the Fig and the Gall-Wasp (*Blastophaga grossata*). In *Ficus Carica*—the species of fig ordinarily cultivated in Europe—the inflorescences are of two kinds, namely: (1) those containing female flowers only, and (2) those which have male flowers near the orifice and, below them, rudimentary females known as "gall-flowers."[1] It is in the latter that the Blastophaga lays its eggs. The first insects to emerge are wingless males, which make good their escape into the interior of the fig by biting through the gall. The winged females then hatch out and are fertilised by the males, subsequently crawling out through the mouth of the inflorescence—an operation which involves their passing over the male flowers, so that they emerge dusted with pollen. They now seek exclusively inflorescences in an earlier stage of development, and in these they deposit their eggs; if the eggs are laid in gall-flowers they develop into wasps, while, if they are deposited in ordinary female flowers, which have long styles, they remain abortive. At the same time, however, the stigmas of the female florets get dusted with pollen from the body of the insect, and cross-fertilisation is accomplished.

It would be irrelevant here to enter upon a discussion of the benefits conferred by cross-pollination, but we may observe that Darwin himself was almost certainly mistaken on this point: the great variety of plants that are habitually self-pollinated without undergoing any degeneration therefrom, precludes us from asserting that self-fertilisation is in itself harmful, *e.g.* cereals—a conclusion that has been extended by East and Jones to the whole sphere of "inbreeding."[2] In the tropical genera *Myrmecodia*, *Unona*, *Artobotrys*, *Goniothalamus*, and *Cyathocolyx*, Burck[3] has discovered species in which

[1] Kerner, *Nat. Hist. Pl. Eng.*, Ed. II, pp. 159–62.

[2] East and Jones, *Inbreeding and Outbreeding*, Philadelphia, 1919.

[3] Burck, W., "Über Kleistogamie in weitere Sinne und das Knight-Darwin'sche Gesetz." *Aus dem hollandischen Manuscripta Übersetzt von Paul Hernsohn. Justs. bot. Jahresber*, xviii, 1892, pp. 467–8.

the flowers always remain completely closed. The Egyptian kings, moreover, were accustomed to preserve the purity of their dynasty by a system of brother-sister marriages that appeared to be without ill-effects. The real advantages of cross-fertilisation would appear to be bound up with those of sexuality itself: they enable the race to obtain a "re-shuffling" of characters, providing in this way new material for evolutionary advance (Von Hartmann,[1] Von Uexkull[2]). Sexuality and, to a greater extent cross-fertilisation, and out-breeding in general, constitute the transverse strands in an expanding evolutionary web.

The first step in the establishment of cross-pollination is the prevention of self-pollination. This has been, for the most part, achieved in two ways: separation of the sexes in space (monœcism, diœcism), and separation of the sexes in time (protogyny, protandry), while over and above these we have the prepotency of alien pollen which, as Fritz Müller has shown, may actually amount to the lethal action of pollen from the same flower.[3] In so far as the mechanisms referred to above can be, and are, equally effective among plants that are pollinated by wind, it is only at this horizon that we discover the specifically symbiotic relation between insects and plants.

On the part of the insects we have the transition from "mandibulate," to "haustellate" mouth-parts and, among the bees at any rate, independent modifications associated with the collection of pollen. The specialisation of mouth-parts for the purpose of sucking up nectar is only a particular case of the tendency, widespread among insects, to absorb their food in the liquid form, rather than to ingest it as a solid. This tendency has often taken the form of what is called "external digestion"—as in the bugs and plant lice which have their maxillæ so constructed that they fit together as a pair of channels or tubes, through one of which a digestive juice is pumped into the tissues of the plant and through the other the dissolved nutritive substance sucked back into the mouth. An earlier stage is represented by the larva of Dytiscus, which feeds on worms, tadpoles, etc., and in which a fine tube runs through each mandible. Through this the larva pumps out a digestive fluid which dissolves the prey, and the solution is then sucked up again. The insects that are most perfectly adapted to a diet of "nectar" are the Lepidoptera, which feed exclusively on this fluid and have their maxillæ enormously extended as

[1] Von Hartmann, "Das Problem des Lebens," Haacke, 1906, *Die geschlechtliche Fortpflanzung in der Natur.*

[2] Von Uexküll, *Theoretical Biology*, Kegan Paul, 1920, p. 259 and elsewhere.

[3] Müller, Fritz, "Über einige Befruchtungserscheinungen," *Bot. Zeit.*, xiv, 1869, p. 224.

two half-tubes which fit together in the form of a proboscis. The latter is sometimes of enormous length, and in one case, that of *Macrositia Cluentius*, reaches a length of 25 cms. It is particularly interesting to observe that exactly the same mechanism has been evolved among the Coleoptera by the beetle *Nemognatha* (one of the Melöidæ), which was observed by Fritz Müller sucking nectar from the flowers of the Convolvulus. The only difference lies in the fact that whereas the maxillæ of the lepidoptera are capable of being rolled up beneath the head, the beetle does not possess this capacity. As a rule, neither butterflies nor moths show any special structural modifications associated with the collection of pollen, the only exception being that of the female *Pronuba* moth, responsible for the fertilisation of the *Yucca*, which carries a ball of pollen by means of the last joints of the maxillary palps, which are specially modified for this purpose.

Among the flies only certain groups are important from the point of view of flower pollination. The Syrphidæ, or "Hover-flies," for example, developed elaborate suctorial mouth-parts and depend largely on flower-food. Unlike the lepidoptera, however, they devour pollen as well as honey and, in the case of *Volucella pellucens* and several smaller species which I observed on the purple inflorescence of *Veronica imperialis*, their attention seemed to be confined exclusively to the anthers. This was particularly conspicuous in so far as the Hymenoptera visiting the same plant were engrossed almost entirely with the nectaries.

But it is among the Hymenoptera, and in particular among the bees, that the last refinements in the inter-relationships between flowers and insects have been imparted, for here we find, not only a transition from typical mandibulate to typical haustellate mouth-parts but, concomitantly, a progressively increasing elaboration of the hind legs in the form of a pollen-collecting apparatus, represented by the series *Prosopis, Sphecodes, Halictus, Andrena, Dasypoda, Panurgus, Anthophora, Eucera, Macropis, Bombus, Apis*. Of these the most primitive, both as regards mouth-parts and pollen-collecting apparatus, is *Prosopis*, which is naked except for a feebly developed brush on the tibia and basal joint of the tarsus ; the most elaborate is the honey-bee (*Apis mellifica*), with a well-defined pollen-basket and collecting apparatus on its hind legs, and elaborate haustellate mouth-parts. To insects thus perfectly adapted to the flowers they visit, the name "*eutropous*" has been applied, as distinct from those which, like the Syrphidæ, show only incomplete adaptation. To this group, too, belong another class of Hymenoptera, the fossorial wasps, or Sphegidæ, which—though carnivorous—are often to be seen

sucking the nectar from flowers. Finally, the ants, which show no modification whatsoever towards pollination, together with the great majority of insects, that are either indifferent to plants or parasitic upon them, fall into the group of "*dystropous*" insects and, conformably with what we have already said with reference to symbiotic evolution in general, *it is to the relationship of the dystropous insects with their hosts that we must look for the historical background of that culminating phase of entomophily expressed by the bees, the Sphingidæ, and the plants specifically associated with them.*

On the part of the plant these developments have found their counterpart in every conceivable combination of structural and physiological peculiarity, and some idea of the attention which these have attracted may be gained by a glance at the four thousand odd references in the bibliography of Knupf's "Handbook." We have at least the satisfaction of orthodoxy if we quote Darwin's own comment on the movements of the pollinia in a certain group of orchids. "A poet," he says, "might imagine that whilst the pollinia were borne through the air from flower to flower, adhering to an insect's body, they voluntarily and eagerly place themselves in that exact position, in which alone they could hope to gain their wish and perpetuate their race."

So striking does this appear from an authority of Darwin's outlook that we may with advantage recall the evidence that spurred him to this observation :

"In most species of Orchis the stigma lies directly beneath the anther cells, and the pollinia simply move vertically downwards. In *Orchis pyramidalis* there are two lateral and inferior stigmas, and the pollinia move downwards and outwards, diverging to the proper angle, so as to strike the two lateral stigmas. In *Gymnadenia* the pollinia move only downwards, but they are adapted for striking the lateral stigmas by being attached to the upper lateral surfaces of the proboscides of the lepidoptera. In *Nigritella* they move upwards, but this depends merely on their being always affixed to the lower side of the proboscis. In *Habenaria* the stigmatic surface lies beneath and between the two widely separated anther cells, and the pollinia here converge instead of diverging, as in *Orchis pyramidalis*, and likewise move downwards."[1]

In the perfection of their relations with insects the flowering plants have exploited almost every combination of colour, form, and scent. Many aroids have both the colour and the smell of decaying flesh, thereby attracting the carrion flies responsible for their pollination. Moreover, the plants that are pollinated by lepidoptera appear to attract those insects by mimicking the scent of the female, which, as Fritz Müller, and

[1] Darwin, C., *Fertilisation of Orchids*, London, 1877, p. 79.

later Col. Longstaff,[1] have shown, is often very decided and is borne in special organs on the wings, legs, or abdomen. Somewhat similar is the case of the Australian orchid *Cryptostylis*, exhibited at a recent meeting of the Linnæan Society, which so closely resembles the females of an ichneumon wasp that the males approach in the same way as they approach the females, that is to say, backwards, receiving the two pollinia on the tip of their abdomen.

If we pass in review the matter of the foregoing discussion there emerges the idea of a symbiotic evolution proceeding from an horizon, or group of horizons, represented by mere association of different organisms, or the active parasitism of one upon another, to a more advanced horizon wherein the beginning of a new individual becomes manifest. It is hardly necessary to remark that this contention does not imply, nor even suggest, that any one particular group of associations has been derived directly from another, nor that the direction of evolution is invariably towards a condition of greater integration. Reasons have already been given, in the discussion of parasitism, for repudiating this contention. Nevertheless, all evidence points to the conclusion that evolution has proceeded towards the establishment, not only of those genetically homogeneous systems that we describe as "organisms," in a limited sense, but also towards other systems, involving genetically unlike individuals, which are themselves organisms in a broader, but still perfectly genuine sense.

SCHEME TO ILLUSTRATE SYMBIOTIC EVOLUTION

Biological Horizon.	PARTICULAR SYMBIOTIC ASSOCIATIONS.					
	Cell Associations.	Societies.	Fungal Diseases.	N.-fixing Organisms.	Myrmecophily.	Insect Pollination.
Loose association or active parasitism	Loose Colony	Gregarious (Locusts) (Army Worms)	"Parasitic" (phytophthora")	Azotobacter and Clostridium Type	Ant-epiphyte Associations	Dystropous (Ants)
Symbiosis with some degree of specialisation	Cœnobium (Pediastrum)	Primitively Social	"Symbiotic" (Rusts)	Pseudomonas Radicicola Type	Acacia Sphærocephala Type	Hemitropous (Wasps)
Advanced symbiosis	Cell Dominion (Volvox)	Advanced Social (Ants, social Bees)	Mycorrhiza	Rubiaceous Leaf-nodule Type	Macaranga Type	Eutropous (Bees and Sphingidæ)

[1] Longstaff, G. B., *Butterfly Hunting in Many Lands*, London, 1912, (Longmans), pp. 490–516.
At the end of this work translations of several of Fritz Müller's papers are given.

THE REGULATION OF THE OVARIAN CYCLE

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In this essay an attempt is made to give a connected account of the experimental work responsible for the development of our present knowledge of the regulation of the ovarian cycle. Though the evidence has been accumulating from various sources for many years, experiments have only recently given definite proof that the anterior pituitary body actually controls the ovarian cycle. In order to present a clear survey of the present situation, it is necessary to present the available data in logical sequence rather than in historical order.

Evidence that the Ovary does not Regulate its own Activity

Three different lines of evidence show that the periodic endocrine and morphological activity of the ovary does not result from any inherent property of the ovary itself.

As early as 1900, Foa showed that the ovary of an immature animal, when grafted into an ovariectomised adult, underwent rapid development, and became mature long before it would have done so in its original surroundings. Conversely, it was found that an adult ovary grafted into an immature animal lost both its histological and endocrine activity. Similar experiments, carried out subsequently by a number of workers, have adequately confirmed the accuracy of Foa's original observations, and have shown clearly that the functional age of the ovary is controlled by its environment. As Hammond concludes : " The age of puberty is determined by the nutritive state of the soma of the animal and not by age changes in the ovary itself."

Further proof that the ovary does not regulate its own activity comes from studying the facts of compensatory hypertrophy. It has been known for many years that removal of one ovary causes the remaining ovary to undergo hypertrophy to an extent sufficient to enable it to produce the same

number of mature follicles as did the two original ovaries. Even a small ovarian fragment will hypertrophy to produce the same result. The endocrine activity of the hypertrophied ovary or ovarian fragment is also equal to that found in the normal animal. This would point to there being some factor at work which limits the number of follicles maturing at one time, and Lipschütz was thus led to formulate "the law of follicular constancy," stating that the number of follicles maturing at one time is regulated, not by the amount of ovarian tissue, but by some factor outside the ovary. The type of reaction demonstrated by Foa, as well as the results of compensatory hypertrophy, have been explained by Hammond as an extension of Heape's theory of a "generative ferment." Heape suggested that there was a substance present in the body essential for growth and reproduction; in the young animal this substance was used up completely by the growth processes, and was available for the activation of the reproductive system only when growth ceased. In the mature animal the amount of this substance available limits the number of follicles which can mature at one time; and the restricted number will be matured from one ovary only, or even from an ovarian fragment. There is little doubt to-day that this hypothetical substance, or X substance, as some writers have since called it, can be identified with certain anterior pituitary substances which have been shown to form an external or somatic control of the ovary.

The experimental facts recorded above show that the time of the onset of puberty, and also the number of follicles ovulating, are both controlled from some source external to the ovary. Evidence is also available that the actual periodicity of the ovarian endocrine cycle is subject to somatic regulation. It was originally supposed that the alternating formation of Graafian follicles and corpora lutea resulted from some inherent property of the ovary, and that the endocrine activity of these alternating structures resulted in the cyclic changes in the accessory organs. It is now known, however, that the cyclic elaboration of the œstrin-producing hormone is independent of the cyclic maturation of Graafian follicles. After total obliteration of the follicular system in the mouse by exposure to X-rays, leaving the ovary devoid of cyclic structures, the periodicity of œstrous symptoms in the accessory organs is unaltered. This fact demonstrates, firstly, that the maturation of the follicle is not a necessary preliminary to the elaboration of œstrin, and, secondly, that the periodicity of follicular maturation does not control the periodicity of endocrine activity. The obvious deduction is that the synchronisation of œstrin production and follicular maturation results from

their both being due to a common stimulus, probably of extra ovarian origin.

Hypopituitarism

From the experiments discussed above, it is clear that the ovary does not regulate its own activity, and it is thus necessary to conclude that an outside somatic tissue is responsible for this regulation. The evidence suggests that some endocrine organ is the responsible factor.

It has been known for a very long time that the ovary and the other endocrine glands are interrelated to some extent. Thus it has been noted that tumours of the suprarenal cortex are connected with sex changes. Similarly, the atrophy of the thymus is held to be correlated with the onset of puberty, and hypo- and hyperthyroidism are associated with certain changes in the reproductive organs. Though there has been much discussion on this problem, it is only recently that a really close co-ordination between the ovary and any one particular gland has been demonstrable. Gradually, however, from clinical data and experimental evidence, attention has become focused on the pituitary body as the somatic organ controlling the ovarian changes.

Turning first to the clinical evidence, it was noted many years ago that disturbances of the pituitary body were accompanied by certain pathological conditions ; the most striking feature being a state of obesity, with marked derangement of sexual function and growth. Thus Frölich in 1901 described such a condition, in which he emphasised the marked atrophy of the reproductive organs. The description of this clinical picture, now well established as Frölich's syndrome, was followed by a large number of observations, which confirmed the earlier findings that disease of the pituitary body was closely associated with abnormalities of the ovary. Thus hypo- and hyperpituitarism have both been found in conjunction with infantilism of the sex organs. Whether this state arises through lack of development or through atrophy depends on whether the onset of the pituitary disease is pre- or post-pubertal.

This accumulation of clinical data led to animal experimental work designed to reproduce similar symptoms by hypophysectomy. The results obtained by different research workers in this field have been very varied, and have led to many conflicting conclusions as to the actual part played by the pituitary body. The reason for the contradictory nature of these results seems to be the intimate connection of the pituitary body with the brain, which makes it extremely likely that any

operative work in this region is liable to injure the brain, thus complicating the effects of removal of the pituitary body. Early work on ablation was completely unreliable for this reason, but Paulesco, in 1908, developed an operative technique which made advance along these lines possible. He showed that, while removal of the posterior lobe had no effect, removal of the anterior lobe produced adiposity and genital atrophy. Investigators who followed Paulesco paid much attention to the effect of hypophysectomy on the genitalia. Some of these considered that the genital disturbances following the operation were due to brain injury only, while others attributed the effect to the actual removal of the pituitary body. Aschner, one of the earliest workers to emphasise the importance of brain injury, concluded that lesions of the hypothalamus produced genital atrophy and obesity, but at the same time he admitted that the reproductive system did not develop normally in the absence of the pituitary gland. Camus and Roussy went farther, and considered that all the disturbances from hypophysectomy, including genital atrophy and loss of function, were due not to pituitary deficiency, but solely to injury of the brain. Bailey and Bremer also questioned the importance of the pituitary gland removal. They thought that though the anterior lobe controlled growth, the brain injuries were responsible for loss of genital function.

On the other hand, however, many investigators agreed with Paulesco that the removal of the anterior lobe of the pituitary body actually produced the changes in the sexual organs. Thus Cushing and his co-workers, in 1912, found that the condition produced by removal or partial removal of the anterior lobe was identical with the pathological condition described by Frölich. In addition they noted atrophy of the uterus, and disappearance of follicles from the ovary with persistence of the interstitial cells. To prove the effect was not due to brain injury, control animals were subjected to the same operative manipulations, save for the final step of removing the gland, and such animals showed no symptoms at all. Similar results have been obtained by Ascoli and Legnani, Bell, Dott, Reichert, and others.

There is no doubt that these somewhat conflicting results pointed to the possibility of a factor other than the pituitary body, namely, a nervous factor, being responsible for at least some of the changes in the genitalia. The latest work on hypophysectomy, however, has shown most convincingly that the anterior pituitary body influences not only growth but also the whole development and function of the ovary. Smith has supplied the required substantiation of the theory of hypophyseal control, with the very convincing results of his recent

work on replacement therapy in the hypophysectomised rat. This author, following up his work on frog larvæ, elaborated a technique for removing the hypophysis in rats. He obtained pronounced retrogression and atrophy of the reproductive system. Follicular growth ceased, and the ovary became filled with interstitial tissue. He then showed that implants of anterior lobe completely restored the atrophied ovary to its normal condition. This restoration indicated that the disturbance of the reproductive organ was due to the removal of the pituitary body and not to brain injury.

Ovary-stimulating Hormones of the Anterior Pituitary Body

The effect of the anterior pituitary body on the ovary was originally studied almost entirely by producing a condition of hypopituitarism—that is, by partial or complete removal of the gland. These extirpation experiments led to attempts to observe the ovary under a condition of hyperpituitarism, and as a result the decisive influence of the anterior lobe on the ovary was finally established. The results obtained from feeding experiments were not successful. Though Goetsch, in 1916, claimed to have stimulated growth and early sexual development, other workers, including Evans and Long, Frank, Sisson and Broyles, and Pearl, have since shown that oral administration has no effect on the development of maturity in animals. In view of this failure with feeding, Evans and Long in 1921 tried the parenteral administration of extracts of anterior lobes, with the result that giantism and certain sex disturbances were produced in the injected animals. Thus Evans found that a daily injection of a saline extract of ox anterior pituitaries caused the complete suppression of the œstrous cycle during the period of injection. In the ovary, all the larger Graafian follicles had become luteinised without the intermediate act of ovulation—in other words, they had formed corpora lutea atretica. This work was the first definite demonstration that there was a substance in the anterior lobe responsible for the production of luteal tissue in the ovary.

Subsequently, striking experiments were performed independently and almost simultaneously in Germany and America, the results of which appeared at first to be somewhat contradictory to those of Evans and Long. Zondek and Aschheim, convinced of some external control of the ovary, set out in a series of very complete and extensive researches to determine what endocrine organs, if any, had a stimulating action on the ovary. Using an intramuscular implantation technique, they found that only the anterior pituitary lobe would produce any

effect on the ovary, and this effect was not inhibitory in nature but decidedly excitory. Zondek and Aschheim found from these experiments that the implantation of a small piece of fresh pituitary of human or cow into a 6–8 grms. infantile mouse would produce full œstrous changes 80–100 hours after treatment—that is, the anterior pituitary substance was effective in bringing about precocious œstrus in the infantile mouse.

At about the same time Smith in America arrived, quite independently, at a somewhat similar conclusion. After failure to restore the atrophied sex organs of hypophysectomised rats by means of Evans's saline or alkali preparations of ox anterior lobe, he used daily implantations of small fragments of pituitary tissue. Success with this method led him to try the same treatment on the normal immature animal. He found that the daily implantation into an immature rat of the whole pituitary gland of an adult rapidly produced all the changes characteristic of sexual maturity. In the ovary, follicles matured and ovulated, true corpora lutea were formed, and the characteristic phenomena of œstrus appeared in the accessory organs.

It is evident from these results of Evans and Long on the one hand, and of Zondek and Aschheim and of Smith on the other, that the anterior pituitary body is capable of producing two distinct effects in the ovary : (*a*) luteinisation of the follicle and suppression of œstrus, and (*b*) ovulation with the accompanying œstrous changes ; in other words, of producing the two essential phases of the ovarian cycle. In both instances the treatment is without effect on the ovariectomised animal, no direct action being exerted on the accessory organs ; the active preparations first stimulate the ovary to produce the ovarian hormones, which in turn effect the required changes in the accessory organs.

Subsequently, Zondek and Aschheim showed that a substance or substances with a similar ovary-stimulating action could be obtained from the placenta and urine during human pregnancy. Whether these substances are of pituitary origin, or are identical with those of the anterior pituitary body, is as yet uncertain.

Responses to Ovary-stimulating Preparations

It was originally thought by most workers that Evans's alkali extract and the implants of anterior lobe as given by Zondek and Aschheim, and by Smith and Engle, were responsible for producing two quite separate and clear-cut effects on the ovary, the alkali extract producing solely luteinisation and the implants only maturation with subsequent ovulation.

On the other hand, it was fairly well agreed that urine of pregnancy extracts produced a mixed effect. Since then results of different workers using these extracts have accumulated, and though their observations seem somewhat contradictory, they tend to show that at present all available preparations of anterior lobe produce both effects in varying proportions.

Taking the results obtained from injection of alkali extracts, it seems that the luteinising effect does predominate very largely; only a few workers have pointed out other effects. Thus Putman says that, though he obtained luteinisation in the adult rat, the same extract in the immature rat brought on œstrus. He unfortunately gives no details of his experiments. Hill and Parkes found ova in the Fallopian tube after injecting immature mice with alkali extract, and also induced ovulation in the œstrous rabbit with a similar preparation. Wiesner and Crew indicate that the alkali extract had an enlarging influence on the follicles. Bellerby, on the other hand, says that these extracts have no œstrous-producing effects whatsoever in the immature animal.

Other extracts of anterior pituitary tissue have been shown to produce the two effects fairly equally, neither luteinisation nor maturation predominating. Bellerby, using a 1 per cent. acetic acid extract, obtained both ovulation and luteinisation. The sulphosalicylic acid extract of Wiesner and Crew gave both maturation and luteinisation, accompanied by cornification of the vagina. Hewitt reports that though an acetic acid extract had no effect on immature animals, a hydrochloric acid extract produced sexual maturity in immature mice. No histological details, however, are given.

The preparations made from urine of pregnancy also produce the two effects. Zondek and Aschheim found that three characteristic reactions occur in the ovary as a result of injections of urine of pregnancy:

(1) Maturation of follicles and ovulation, with production of œstrin and secondary œstrous symptoms.
(2) Production of blood follicles.
(3) Luteinisation to form atretic corpora lutea.

Many workers have confirmed these results. The significance of the blood follicles is not yet understood. Mirskaya and Crew obtained corpora lutea, corpora lutea atretica, and blood follicles after injection of urine of pregnancy, but failed to demonstrate that ovulation occurred. In contrast to most workers, Engle considers that urine of pregnancy preparations cause only follicular atresia, luteinisation, and formation of blood follicles, no ovulation taking place.

Finally, at the other extreme, are the implants which would appear to act mainly in producing maturation of follicles, with ovulation and the accompanying œstrous phenomena in the accessory organs. There are, however, definite reports that atretic corpora lutea are also produced, and sometimes even blood follicles. Smith and Engle still maintain their original view that implants produce ovulation only, the luteinisation occurring after ovulation to form true corpora lutea. They also say that they do not observe any blood follicles. This is quite contrary to the results of Zondek and Aschheim, who find not only ovulation, but also atretic corpora lutea and blood follicles. Their results have been confirmed by other workers, especially Fels, who emphasised the formation of atretic corpora lutea, which he said was more frequent than actual ovulation.

Correlation between the Morphological and Endocrine Response of the Ovary

The changes in the accessory organs resulting from activity of the œstrous and luteal hormones are well known in many animals, and the response of the accessory organs to ovarian stimulation is thus some guide to the type of endocrine activity induced in the ovary. The dual morphological reaction so often produced in the ovary by injection must tend to complicate the accompanying changes in the accessory organs, but usually the effect obtained follows the preponderating morphological change in the ovary.

It is necessary to qualify this statement by saying that in certain cases the reaction of the accessory organs has not the normal correlation with the ovarian structures produced. According to Wiesner and Crew, " extracts of anterior pituitary induce production of ovarian hormones without necessarily provoking the structural changes in the ovary, which normally precede, accompany, or follow their elaboration." Thus Wiesner and Crew obtained some injected animals in which cornification occurred in the vagina without definite follicular enlargement in the ovary. This would suggest that a higher threshold value of anterior pituitary substance is necessary for histological change than for endocrine activity. Wiesner and Crew emphasise that changes in the accessory organs may be a more accurate criterion of ovary-stimulating activity than is the ovarian morphological reaction.

Causation of the Dual Effect

The striking difference in effect produced on the ovary by the alkali extract of Evans and the implantations used by

other workers, made it seem probable that the anterior lobe contained two hormones—one responsible for luteinisation, the other for follicular maturation. The fact that these preparations are merely the extremes of a series of preparations, all of which produce the two effects in varying proportions, does not make this view any less reasonable. The dual effect on the ovary may, however, be accounted for by either of two hypotheses: it may be due to (*a*) two hormones present in varying amounts in the different preparations, or (*b*) one hormone working quantitatively.

The latter hypothesis is supported by certain differences in the mode of preparation and administration of the alkali extract and the implants. The alkali extract, made from ox pituitaries, has been neutralised by most workers before centrifuging, and thus the final extraction of the tissue is equivalent to a neutral or saline one, the only difference being that the initial treatment with sodium hydroxide breaks down the tissue, and allows a more complete extraction with the saline. Therefore the alkali extract is a highly concentrated preparation, and its luteinising effect might be due to the presence of one hormone in large amounts. The amount given per day to the adult mouse is roughly equal to 1 gramme of original tissue. The implants, on the other hand, are unaltered tissue, and the amount given per day is very minute, possibly as little as 5 mg. of original gland. The ovulation effect produced by these implants might be due to the same hormone, here present only in small amounts.

On the other hand, it might be assumed that alkali treatment destroys the follicle maturation hormone, but there seems to be little evidence for this supposition; in any case, no such theory could account for the comparative lack of luteinising action by implants. Another view of this difference, in the main effect produced by alkali extract and by implants, is that the substance responsible for maturation escapes readily from implants, whereas the second substance, responsible for luteinisation, becomes available less easily.

To test the quantitative hypothesis it was necessary to give the alkali extract in small doses and, conversely, to give implants in large doses. Several workers have carried out dilution experiments with this object in view. Zondek and Aschheim injected different amounts of an active sample of urine, and found that the same qualitative effect was obtained whatever the amount given. Wiesner and Crew similarly gave dilutions of alkali extract, using a range of doses from 0·2 to 0·0012 c.c. The effect of luteinisation in preponderance was found all through the series until a dose 0·0025 c.c. was reached, at which point no effect whatsoever was produced in

the ovary. They concluded, therefore, that there appeared to be no basis for the assumption of a quantitative action of one hormone. As far as can be seen from the literature on this subject, Courrier is the only worker who has obtained any positive evidence from quantitative experiments. He reports that injection of small doses of alkali extract in a cat produced ovulation, while larger doses produced atretic corpora lutea. Thus there would seem to be no conclusive evidence from experimental results in support of the quantitative hypothesis. Apart from this absence of positive evidence, there are other reasons why this hypothesis appears unsatisfactory as a working explanation of the mode of action of the anterior pituitary body. It seems unreasonable that one hormone should be able to produce both effects at the same time in one follicle, yet this is what appears to happen in many follicles in a treated ovary. Thus, many follicles which have become partially luteinised show at the same time every sign of having grown extensively. Again, on the quantitative hypothesis, it would be assumed that the addition of a small amount of hormone to that already present would only accentuate the previous effect produced. In pregnancy, however, where the anterior lobe is presumably concerned with maintaining the luteal phase, injection of a small amount of urine (Zondek and Aschheim) or implants (Engle and Mermod) does not increase the luteinising effect, but actually produces ovulation.

From the above reasoning and from the negative results obtained by the majority of workers from dilution experiments, the hypothesis of two hormones would seem to be the most probable. There is, in addition, a small amount of positive evidence that two hormones are present in the anterior lobe. This relates largely to the alleged preparation of active extracts from a source giving one type of activity only and to the partial chemical separation of the two principles.

Through their extensive researches on the urine of pregnant and non-pregnant women, Zondek and Aschheim have recently become convinced of the existence of two hormones. Previously, having shown that urine of pregnancy produces both maturation and luteinisation, they have now discovered that urine from non-pregnant women with tumours and certain endocrine disturbances, and urine from ovariectomised women may also show ovary-stimulating activity, but of the follicle-maturing type only. Again, Aschheim claims to have made an extract from the male hypophysis, causing nothing but luteinisation. Mirskaia states that it is possible to obtain from mouse placentæ an extract giving luteinisation without any maturation. The definite proof, however, that two substances are involved will be their chemical separation from an extract

producing the two effects. Zondek claims to have obtained occasional separation from urine of pregnancy by precipitation methods, but at present chemical evidence in support of the hypothesis of two hormones is completely unsatisfactory.

General Effect of Anterior Pituitary Preparations on the Normal Animal

In the immature animal the effect of prolonged injection of an extract predominantly causing luteinisation is, according to Evans, postponement of sexual maturity with retardation of follicular growth. The first œstrus usually occurs much later in treated animals, and afterwards only a few ovulations occur—in some animals œstrus is not observed. The follicles become luteinised, so that the ovary is filled with corpora lutea containing the remains of ova. If injection is carried on for a short time only, similar luteinisation of the follicles occurs ; immediate œstrous changes in the accessory organs do not take place. Wiesner and Crew state that the vagina shows changes characteristic of the luteal phase, but that the uterine changes are slight. It was also found that immature animals treated with alkali extract did not show the placentoma reaction.

Using their implantation method, Smith and Engle procured precocious œstrus after three implantations made on successive days into seventeen-day-old mice. Zondek and Aschheim state that one implant is sufficient to bring a mouse into œstrus one hundred hours after implantation. The latter authors, however, were using older mice, and Engle and Smith have shown that the required duration of treatment is inversely proportional to the age of the animal. The precocious œstrus produced has the usual accompanying symptoms. The vagina opens, and becomes cornified ; the uterus is distended and hyperæmic. The ovary is greatly increased in size ; a greater number of follicles are matured than usual, but the size of the mature follicle is not abnormally great. Ovulation occurs very readily, and thus large numbers of true corpora lutea are found.

Zondek and Aschheim failed to induce mating in treated mice, but Smith and Engle report that coitus occurred at nineteen days. Mirskaya and Crew obtained some evidence of mating during precocious œstrus, but pregnancy never followed. They further state that administration of œstrogenic pituitary preparations to the immature individual does not affect the pituitary of the animal, and thus maturity endures only so long as the administration is continued. The induction of precocious œstrus does not influence the subsequent onset

of puberty. Owing to the formation of corpora lutea, prolonged injection does not lead to persistent œstrus, but great hypertrophy of the accessory organs is produced.

In the adult mouse, the alkali extract suppresses the œstrous cycle completely during the period of injection and for several days after. The Graafian follicles, which have reached the stage of possessing an antrum, become completely luteinised without previously ovulating. The ovary thus comes to consist of a large mass of corpora lutea atretica, in which are embedded the remains of the ova. The effects of implants on the adult animal have been fully described by Smith and Engle. Growth of a large number of follicles takes place in the ovary, followed by super-ovulation, large numbers of ova being ovulated at once; correspondingly large numbers of corpora lutea are subsequently formed. Mating will take place, so that a large number of embryos may be present in the uterus and super-pregnancy may occur, at least for a short time. There is a marked increase in the weight of the ovary. In the mouse this is due to the excess formation of mature follicles and true corpora lutea. In the rat large cysts are formed as well.

The œstrous rabbit (which ovulates only after coitus) is a good test animal for ovary-stimulating preparations. Bellerby showed that ovulation could be produced without coitus by injection of preparations of ox anterior lobe. Friedman, and Hill and Parkes have obtained the same result, using urine of pregnancy. In the ferret the reaction produced differs somewhat from the normal response found in mice; luteinising extracts fail to produce any considerable growth of the follicular granulosa, but instead large cystic follicles, with swelling of the theca interna, are found.

The effect of implants on the pregnant animal has been investigated by Engle and Mermod, and Zondek and Aschheim. The former state that treatment with implants during the middle third of the period of gestation caused reabsorption or expulsion of the fœtuses. With later treatment many normal litters were born at term. Engle and Mermod suppose, however, that sufficiently large doses of hormone would induce abortion in every case, owing primarily to the production of œstrin causing a local disturbance on the uterus. Though ova were found in the Fallopian tubes, these authors consider that the fœtuses were dead before ovulation occurred. Zondek and Aschheim, on the other hand, found that while large doses caused abortion, smaller doses produced ovulation and another set of corpora lutea in the ovary without interfering with the fœtuses in the uterus. When implants were given earlier in pregnancy the same result followed, except that the vagina

was not affected by the œstrin produced. Fels came to the same conclusions as Zondek and Aschheim. He suggests that the non-occurrence of œstrous changes in the vagina is due to loss of sensitivity during pregnancy. Zondek also found that injection of urine of pregnancy induced ovulation in the pregnant mouse, but that greater amounts were required to cause abortion.

In contrast to this shortening of the period of gestation, the effect of injection of luteinising extracts would appear to be that of prolongation. According to Teel, this is due to delay in implantation of the egg. The fœtuses reach full term, but die in the uterus, and are expelled later. He concluded that their death was due to some failure of the birth mechanism brought about by the presence of the lutein tissue produced by injection.

Theoretical Implications of the Ovary-Pituitary Mechanism

It is necessary to conclude from the work described above that the anterior pituitary produces a substance, or more probably substances, exerting a very powerful influence on the ovary, and it is thus evident that the anterior pituitary body plays a very important—if not exclusive—rôle in regulating the ovarian cycle.

The fact that precocious maturity can be induced in the normal immature animal, by administration of anterior pituitary substance, would suggest that the first œstrus is brought about by some action of the pituitary body. But since pituitaries from immature animals are efficacious in producing precocious œstrus, it is difficult to understand how the mechanism is activated at puberty. The pituitary body is also presumably responsible for governing ovarian periodicity; but again, how this periodicity is actually brought about has yet to be shown. The site of the periodic mechanism has merely been transferred from the ovary to the pituitary body. The same questions which previously referred to the ovary must now be asked about the pituitary body, for while we have learnt in this transference what actual stimuli are responsible for the activation of the ovary, we still do not know the mechanism controlling the necessary alternation of these stimuli.

There appear to be no cyclic structures in the anterior pituitary body, yet the easiest supposition would be that the periodicity of the ovary is brought about by a periodic production of the stimulating substances by the anterior pituitary body. Smith and Engle "believe that the hypothesis of

the periodic liberation of the gonad-stimulating hormone of the pituitary may explain the periodic ripening of groups of follicles more satisfactorily than any previously advanced." Zondek and Aschheim suggest the alternate production of two hormones, one concerned with maturation, and the other with luteinisation, as the cause of the ovarian periodicity.

If such an endocrine cycle exists, it should be possible to demonstrate that pituitaries possess the active substances in varying amounts at different stages of the œstrous cycle. With the present inaccurate methods of assay, however, it is difficult to distinguish small differences in activity, though larger variations can be detected. Nevertheless, several workers, by comparing the weights of the immature ovaries after implantation of different pituitaries, have succeeded in demonstrating the existence of definite variations in the hormone content of the pituitary of the female and also of the pituitaries of the two sexes. Thus Evans and Simpson have shown that there is a difference in the amount of active substance present in the pituitaries of males and females. Contrary to what might be expected, pituitaries from adult males are more potent than pituitaries from females. Even the pituitary of the immature male appears to possess more active substance than that of the adult female.

The recent work of Smith and Engle showing that the phases of the œstrous cycle in the guinea-pig are correlated with fluctuations in the hormone content of the gland, is suggestive of a periodicity in the rate of elaboration of the active substances, and gives support to the hypothesis of an endocrine cycle in the anterior pituitary body. In pregnancy, on the other hand, when large quantities of the hormones appear to be secreted and increased amounts of hormones might therefore be expected in the gland, little change in the activity of the pituitary was observed by Evans and Simpson. Again, Engle showed that pituitaries from gonadectomised animals are more active than the pituitaries from normal animals. This work has recently been extended by Evans and Simpson, who state that whereas the gonadectomised male pituitary is roughly two and a half times as active as that of the normal male, the gonadectomised female pituitary is six times as effective as that of the normal female. They explain this increase of potency as due to a storage of the hormone, by what appear to be the basophil cells, rather than to a hypertrophy of the gland itself.

Other evidence of an endocrine cycle in the pituitary body may be derived from the study of variations in size and histological structure of the gland under different conditions. As the normal cell structure of the pituitary body is still in-

adequately known, the histological observations tend to be somewhat confused, and since there seems to be no satisfactory means of estimating the number of acidophil, basophil, and chromophobe cells in any one pituitary, the attempts at quantitative comparison of cells are necessarily somewhat inaccurate. The results obtained from size comparison of the pituitary are probably more dependable.

The most promising material might be expected from animals having a definite period of anœstrus, and Rasmussen has reported an increase in the number of basophil cells in the woodchuck at the end of the hibernation period, corresponding to the beginning of the breeding season. During pregnancy, most workers state that the pituitary body increases in size and weight, due to an increase in the number of cells present in the anterior lobe, though which type of cell is actually responsible for this enlargement is not by any means agreed upon. Erdheim and Stumme observed an increase in the number of acidophils due to a transformation of the chromophobes, with a related increase in weight of the pituitary body. Since their observations were made on human material with no method of control, the results are not very convincing. Aschheim also states that the chief cells undergo hypertrophy and hyperplasia to form the so-called pregnancy cells. On the other hand, Herring found that the pituitary body showed as much as 25 per cent. diminution in weight, which appeared to be due to a large decrease in the number and size of the acidophil cells. Allanson and Brambell also conclude there is no increase in size and weight of the pituitary in the rabbit during pregnancy.

After gonadectomy various workers have found corresponding changes in the pituitary body. Hatai states that ovariectomy has no effect on the weight of the gland, while Marassini agrees in that he found no histological change in the gland structure. Tandler and Grosz report that the pituitary hypertrophies, a result confirmed by Fischera, who found the hypertrophy to be correlated with increase in size and number of the acidophils. Cimorini also observes an increase in these cells. On the contrary, Addison found an increase of the basophil granules in the cells with great increase of the size of the cells and a corresponding decrease in size of the acidophils.

Finally, if the existence of some endocrine cycle in the pituitary body be assumed, it is necessary to suppose that the reproductive system can in turn react upon the anterior lobe to modify this cycle when necessary. Thus the postponement of the next œstrus by sterile copulation in the rat and mouse argues an influence exerted by this act on the pituitary body.

Since mechanical stimulation of the uterine cervix has the same effect as sterile copulation in producing pseudo-pregnancy, there would appear to be some nervous connection between the cervix and the pituitary body. Long and Evans have shown that cervical stimulation will activate the corpora lutea in a grafted ovary, making it quite clear that there is no direct nervous action on the corpora lutea. The intermediate point, where the nervous stimulus is changed to an endocrine one, must be the pituitary body. A similar conclusion may be drawn from our knowledge of the rabbit, where ovulation depends on copulation, which presumably influences the pituitary body to produce the substance required to induce ovulation. Again, implantation of the fertilised ovum causes the prolongation of the life of the corpus luteum, and this apparently involves prolongation of the luteinising stimulus from the anterior lobe.

The problem of how events in the accessory organs influence the endocrine cycle in the anterior pituitary body is of extreme interest and importance, and its solution may be invaluable in the elucidation of the nature of the periodic mechanism of that body.

FURTHER ADVANCES IN PHOTOGRAPHIC SCIENCE

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Dr. F. C. Toy, in an article published in Science Progress for October 1923 [1], explained the theory of the distribution of the latent photographic image in sensitive materials made from silver halides embedded in gelatine. In the present article it is intended to carry the story of latent image formation a little farther, giving in outline the results of more recent investigations and the main arguments of various hypotheses about the mechanism of light action when an exposure is made on a photographic plate or film.

First of all, however, let us summarise the earlier work described by Toy. The sensitive layer of a photographic plate consists of millions of tiny grains of silver halide, embedded in gelatine. The grains themselves are often crystalline in appearance and, so far as they have been examined, have a cubic lattice structure of the same type as sodium chloride. The grains having been formed in darkness or non-actinic light, are usually not reduced to metallic silver when treated with such chemical reducers as hydroquinone, pyrogallol, etc., known as photographic developers. Exposure to light, however, causes some of the grains to become vulnerable to developers. The greater the exposure, the greater the number of grains which are "developable," so that the variation in blackness of a negative is due to variation in the number of grains of spongy silver produced by reduction of silver bromide grains.

By 1923 the extreme sensitivity of modern photographic emulsions was fairly generally considered to be closely related to the presence of "sensitivity nuclei" on the surfaces of the halide grains. Toy and Svedberg [2] independently developed the idea that when light is absorbed by the sensitive material, these nuclei tend to be converted to "development centres." The presence of a single development centre on a grain of silver halide renders the grain capable of being completely reduced by a developer.

Since the sensitivity-nucleus-development-centre hypothesis

was suggested, much work has been done in the attempt to ascertain the chemical nature of the sensitivity-nuclei and to discover the mechanism of their action.

Colloidal silver has long been credited with the power of enhancing the sensitivity of photographic emulsions. Various colloidal substances besides silver are now known to possess this power, provided they are added in a suitable way to the emulsion during manufacture; gold, silver, and platinum have all been used [3]. The most notable discovery in this field was, however, made by S. E. Sheppard, of the Eastman Kodak Company [4]. It had been noted that certain gelatines when used for making emulsions always yielded very sensitive materials. After careful searching it was found that they contained minute traces of allyl-*iso*thiocyanate. This substance was suspected of being the sensitiser, and it was found that its addition to an emulsion made with a gelatine which ordinarily yielded comparatively insensitive material caused a great increase of sensitivity. The action of ammonia on allyl-*iso*thiocyanate is to form allyl-thiocarbamide (thiosinamine), and it was soon found that this substance sensitised very readily. The necessary condition for any considerable sensitisation was, however, that the emulsion should be slightly alkaline [5]. Now thiocarbamides form a series of complex salts with silver salts. Those complexes which contain the higher proportions of the thiocarbamide are generally soluble in water, those with a lower proportion of thiocarbamide are insoluble. A very dilute solution of a thiocarbamide in contact with silver bromide would therefore tend to form specks of insoluble complex on the surface of the halide. The complexes themselves are fairly stable in acid or neutral solution, but rapidly decompose in solutions which are at all alkaline, yielding silver sulphide. It was therefore natural to conclude that the sensitisation produced by thiocarbamides was due to the formation of specks of silver sulphide on the grain surfaces.

The amount of thiocarbamide necessary to produce a considerable sensitisation is of the order of 1 part to 300,000 parts of gelatine or about 1 part to 100,000 parts of silver bromide. Because of the small quantity of sensitiser, attempts to analyse emulsions for silver sulphide appear to have failed. For a like reason the accurate estimation of other nuclear sensitisers, such as silver, has not yet been very successful [6].

The failure to determine by direct analysis the nature and amount of the substances comprising the sensitivity nuclei also extends to the substances formed during the ordinary exposures necessary for the establishment of the latent image. It is true that when photographic materials are exposed long enough for a visible change to occur, it is possible to recognise

both metallic silver and free halogen, and to estimate the amount of the former. We cannot, however, assume on this ground alone that the latent image itself consists of free silver. Actually there are many facts which do support this view, though they do not prove its correctness.

Anxious to go beyond the limitations imposed by chemical analysis, several workers have set out to investigate the formation of the latent image by other methods. The new experiments have already been successful in showing how the sensitivity nuclei do *not* work, and this is in itself a great advance.

Toy, Egerton, and Vick investigated the external photoelectric effect in silver halides [7]. They found no relation between this effect and the photographic properties of the substances. In their experiments electrons were ejected from these substances only by light of shorter wave-length than 3,000 Å. Photographic sensitivity, at any rate with silver bromide, is, however, appreciable at 5,000 Å., so that there appeared very little likelihood of a direct relation between the external photoelectric effect and photographic sensitivity. Attention was therefore turned to the photo-*conductance* effect in silver halides. This is the occurrence of increased electrical conductance in substances when illuminated. It is shown by several substances [8]. Arrhenius, Scholl, Wilson, and Case had each in turn between 1895 and 1917 made experiments on it. Coblentz in 1922 published the results of more extensive work. Toy in 1926 summarised most of what was already known, and began experiments in order to find out whether the formation of the latent image is related to this photoconductance effect in the silver halides [9].

Even the earliest experiments of any importance showed a striking similarity between the two effects. As they were explored in the spectrum from the red end towards the violet they were found to begin to be appreciable at the same wavelength as that corresponding with the long wave-length edge of the main absorption band of the material under observation. As the wave-length of the light was decreased, both effects increased over a short spectral range, but as the violet region was reached divergences were found. Coblentz's results in particular showed a very striking disagreement between the photo-conductance effect in his specimens of silver halides and the spectral photographic sensitivity of the emulsified halides. The behaviour of silver bromide may be taken as typical. In Coblentz's experiments the photo-conductance in this substance rose sharply as the wave-length was decreased from 5,000 Å. and reached a maximum at about 4,600 Å. It then dropped almost to zero at about 4,200 Å. Now the sensi-

tivity of a photographic emulsion containing silver bromide as its only halide extends from about 5,000 Å. far into the ultra-violet. If therefore Coblentz's results were entirely due to an inherent property of silver bromide, there could be no hope of establishing identity between the mechanisms of latent image formation and the photo-conductance effect. Coblentz's experiments were, however, carried out on comparatively thick specimens of the halide, and Toy was able to show that by using thinner specimens the maximum at 4,600 Å. became smaller, whilst the relative effect towards the violet and ultra-violet became much greater. By using extremely thin layers of silver bromide he succeeded at last in getting rid of the maximum altogether, the effect increasing continuously towards the ultra-violet. The curve expressing the relation between the relative photo-conductance and wave-length approximated very closely indeed to the absorption curve for silver bromide, and also to the curve relating photographic sensitivity to wave-length for a single-grain layer of a very slow pure silver bromide emulsion, in which the light energy absorbed at any wave-length could be taken as proportional to the corresponding absorption coefficient. Thus the difficulty introduced by certain of Coblentz's results was shown to be due to the special conditions of his experiments, and not to an inherent property of silver bromide.

The reason for the maximum observed by Coblentz is not very well understood, but it was probably due in part to the fact that light of wave-length shorter than 4,600 Å. being so strongly absorbed in the front layers of the specimens that the bulk of the material was practically unilluminated. With light which is less strongly absorbed (*i.e.* of longer wave-length) the whole of the specimen could be affected.

Since a photograph may be taken in a very small fraction of a second, it follows that if latent image formation and photo-conductance involve the same primary mechanism, the rate at which the photo-conductance effect is established must be very rapid. Toy and Harrison have demonstrated that this is indeed true. They found that the photo-currents started within 0·001 second of the illumination, and were completely established within 0·03 second. Since this period was approximately that of the lag in the amplifier and galvanometer employed, it is probable that the effect reached its final value much more rapidly than this, and there seems no reason to doubt that it starts almost instantaneously. They were able also to observe these photo-currents with an intensity of ultra-violet light which was so small that an exposure for one-twenty-fifth of a second just produced a developable effect on a plate of H. and D. speed 500. The effect has thus been

observed under conditions of intensity occurring in making normal photographic exposures.

Finally, it was shown that the dark conductance of silver bromide is electrolytic, and is carried by certain silver ions. At the temperature of liquid air it decreases to about one-hundred-thousandth part of its value at room temperature. The photo-conductance is, however, probably largely, if not entirely, electronic [10].

As far as published results are concerned, it is evident that a very close similarity exists between the photo-conductance effect and the formation of the latent image. Further experiments are being carried out to extend the conditions under which the two effects may be compared. For example, Masaki found that in silver halides the absorption bands are displaced farther towards the red by raising the temperature [11]. The photographic spectral sensitivity follows a parallel course, and Masaki has just published a further paper in which he reports that the maximum point (compare what has already been said about Coblentz's work) in the photo-conductance curve for silver bromide is also shifted towards the red when the temperature is raised.

The existing data available when Toy commenced his study of the photo-conductance effect was sufficient not only to suggest the probable identity of the two mechanisms as already outlined, but also to indicate something about the actual way in which light acts on silver bromide. Toy summarised the recorded spectral regions for maximum photo-conductance for equal energy spectra. The substances mentioned in the summary were the chlorides, bromides, and iodides of thallium and silver, the iodides of lead and mercury, and solid iodine. The following important points were noted :

1. The spectral range for maximum effect changes very little with large variations in atomic weight of the metal, provided the same halogen is used.

2. There is in general a progressive shift in the position of the maximum effect from chlorides through bromides to iodides and solid iodine.

3. The average value for the wave-length of maximum effect for the iodide (4,900 Å.) is not far removed from that for solid iodine (5,400 Å.).

4. The absorption bands of chlorine and bromine vapours occur in the same order as do the spectral sensitivity regions of silver chloride and silver bromide for the photo-conductance effect ; the position of maximum absorption for chlorine being about 3,400 Å. and for bromine about 4,200 Å.

There seems little doubt from this summary that *the*

photo-sensitive units in the crystallised salts which have been considered are the halide ions. This is in accord with the idea put forward by Dauvillier in 1920 [12] and later by Fajans [13], and by Sheppard and Trivelli [14] that the first action of light in forming the latent image is to loosen electrons from the halide ions and to discharge neighbouring silver ions, thus forming free silver and free halogen. Toy suggests that the excited electrons in the halogen ions are momentarily in the free state, and so provide extra carriers of electricity within the crystal.

The magnetic properties of the silver halides may also furnish a great deal of information about the action of light on the silver halides. Garrison [15] has already found that there is an instantaneous change in the magnetic properties of these substances on illumination. Silver chloride is diamagnetic, but this property decreases on illumination. The bromide and iodide are paramagnetic, and become more so on illumination.

From what has now been said already, it seems that although sensitivity nuclei may consist of particles of foreign matter in the silver halide grains of an emulsion, yet the light action which produces the latent image is absorbed by the silver halide itself, and not by the substance of the nuclei. Direct experiments with sensitisers, such as thiosinamine, have sometimes yielded striking support for this argument. Sheppard actually sensitised an emulsion with thiosinamine, and found no change in the spectral range to which the emulsion responded, merely general sensitisation being observed [16]. Though this result is not always obtained, it is now generally believed that for a simple silver halide emulsion the first action of light is on the halide itself, and that the nuclei exert some subsidiary influence which favours developability. Up to the present we do not know how the nuclei operate. There are, however, several hypotheses, some of which are sufficiently interesting to be described here.

In 1925 Sheppard, Trivelli, and Loveland [17] suggested that the "development centres" described by Toy and Svedberg consisted of specks of some substance which were greater than a certain size. A speck of smaller size would not initiate the action of an applied developer. The sensitivity nuclei were supposed to be very small specks of impurity in the silver halide; these would not themselves initiate development, but would cause the silver set free by light absorbed in the halide to be collected round themselves, that is to say, a development centre was pictured as a speck made up of a core of some substance, not silver bromide, with a coating of silver formed by light action. Some nuclei would be larger than others, and would therefore require only a little

silver to be added to them in order that development centres might be formed. Smaller nuclei would require more silver and consequently greater light action before they would grow to the required critical size. In this way the "sensitivity" of the nuclei would be a function of their size. This hypothesis at the present time is very popular. It does not supply an explanation of the way in which the nucleus operates, but it does suggest that a development centre is a nucleus on which silver may be deposited from a supersaturated solution. This was called by Sheppard and his colleagues the "concentration speck hypothesis."

Several attempts have been made to explain how this concentration of silver at the nuclei takes place. It was suggested that the specks cause strains in the surface of the silver halide, and that for this reason photo-decomposition occurs mainly in the neighbourhood of the specks. Then, after it had been shown that silver sulphide is probably the substance of some of the specks, it was suggested by Hickmann that the liberated bromine attacks the nuclei of silver sulphide in such a way that silver is formed, the total amount of silver being greater than the amount actually derived from the photo-decomposition of the halide.

Though the reactions postulated by Hickman have been judged possible on thermochemical grounds, the unfortunate fact remains that when any attempt has been made to study the interaction between bromine and silver sulphide the formation of silver has never been observed.

More recently Trivelli [19] put forward a very ingenious hypothesis of the mechanism of the action of sensitivity nuclei. He assumed, not without supporting evidence [20], that the nuclei may sometimes consist of silver sulphide and metallic silver in contact with one another and embedded in silver bromide. A nucleus is considered to behave like a voltaic cell, the electrodes of which are silver and silver sulphide, while the electrolyte is silver bromide. The action of light on this cell is supposed to be twofold: the electromotive force is increased, and also the conductance of the electrolyte is increased. In the dark, the current flowing is supposed to be zero or at any rate very small. On exposure to light a current is quickly set up and causes silver to be deposited on the silver electrode of the nuclear cell. The quantity of the silver is thus increased until sufficient has accumulated to constitute a development centre.

Toy and Harrison raised strong objections to Trivelli's hypothesis [10]. A fraction of a second is a normal camera exposure, and in this time a latent image is produced in photographic material which, in the dark, would probably remain

undevelopable for years. According to the hypothesis this would correspond with electrolytic currents which in the light are hundreds of millions of times greater in the dark. For this increase of current to occur there would have to be either an enormous increase in the electromotive force of the cell or a corresponding increase in the conductance of the silver bromide. Now, the electromotive force of such a cell in the dark is probably not negligibly small, so that the hypothesis seems to require an electromotive force of great magnitude in light; the former alternative is therefore not attractive. The second alternative is equally unlikely, because it has been found that the electrolytic conductance of silver bromide is not perceptibly increased by light, the photo-conductance being due to *electronic* transfer of electricity. It is also well known that photographic materials are comparatively sensitive even at the temperature of boiling liquid air. If any part of the photo-current were electrolytic it would probably decrease rapidly with decrease of temperature. We should therefore expect that if the latent image originated in electrolytic currents the sensitivity of an emulsion would be almost negligible at very low temperatures. From these various aspects the hypothesis thus appears to be unsatisfactory.

Our knowledge of the sensitivity nuclei is as yet very meagre. It is now almost certain that the nuclei exist, and it is probable that silver sulphide is often present in them, but we do not yet know how they work, though there is strong evidence that the first action of light is on the silver bromide near the nuclei and not on the nuclei themselves.

The first action of light in forming the latent image is believed to be the photo-decomposition of the silver halide into its elements. The silver probably constitutes the latent image, and the halogen must in some way be prevented from recombining with the silver, and it is probable that gelatine itself combines with the liberated halogen, and so behaves as a preservative of the latent image. The amount of silver resulting from a given exposure must also be related to the readiness with which the halogen is removed, so that the investigator in the field of sensitivity must consider these secondary chemical reactions, as well as the primary action of light on the silver halide.

The idea of sensitising photographic emulsions by means of compounds which react readily with halogens is very old, but no valuable sensitisation of latent image production has yet been achieved in this way, though for the very great exposures necessary to cause the visible discoloration of the halide, considerable sensitisation has been observed. The failure to sensitise emulsions with halogen acceptors resulted in a fairly

widespread belief that the direct decomposition of silver bromide into its elements was not the fundamental chemical mechanism of latent image formation. This belief persisted for several years, but on account of the experiments which have been described and of others [21] which have led to somewhat similar conclusions, attention is now being turned once more to the reaction of bromine with gelatine. Progress is, however, very slow.

The factors which influence the sensitivity of emulsions are very numerous, and are not at all well understood. The disentanglement of these various factors is very laborious and difficult. The author [22] has spent a considerable time in exploring a single one of them, which is possibly related to the reaction between gelatine and bromine. A brief outline of the work is as follows.

The manufacture of photographic material may be divided into four stages: (1) precipitation and ripening of the silver halides in a sol of gelatine; (2) the removal of the soluble salts formed in the double decomposition of the salts used in the first stage; (3) digestion at a moderate temperature; and (4) coating the emulsion on to the base of film or glass and subsequent drying. The study now to be described concerns stage 3, the digestion of the emulsion. It was found that the free acidity of the emulsion during digestion exerts a powerful influence on sensitivity. In one experiment an emulsion digested at *p*H9 was nine times more sensitive than an exactly similar emulsion digested at *p*H5. The change of sensitivity appeared to be unaccompanied by any variation of grain size, nor was there any evidence of preferential sensitisation of some grains; as far as could be ascertained, each one had its sensitivity increased in the same proportion. Later it was found that the sensitivity of an emulsion which had been digested for some time was almost instantly lowered by the addition of acid. The original sensitivity was, however, restored just as quickly by adjusting the *p*H value of the emulsion to its original value. The ratio of the sensitivity of the emulsion immediately before adding acid to the sensitivity immediately after appeared to vary with the gelatine employed for making the emulsions, but was independent of considerable alterations in the method of preparing the grains themselves. This last result and several other facts suggested that the changes of sensitivity are related to the grain environment and not to the grains themselves.

Various hypotheses have been put forward to explain these results; some of them have already been tested, and have failed to pass the test of experiment. At the present time the most promising suggestion seems to be that the alkaline condition

favours the bromination of the gelatine and so causes a greater amount of free silver to be built up during a given exposure. The acid condition of gelatine on the other hand is supposed to be unfavourable, and allows considerable rebromination of the silver. Preliminary experiments [23] on the bromination of gelatin at different *p*H values support the idea. Apparent disagreement between the emulsion results and these later experiments is confined only to details, and cannot yet be pressed very much ; in the photographic emulsion the reactions occur between a very few molecules at the grain surfaces, the bromine liberated must react with gelatine, which is probably oriented by adsorption forces at the surface of the grains ; in the reaction mixtures used for the other experiments gelatine reacted with a dilute aqueous mixture of free bromine and hypobromite.

The problem of the latent image is of course one of great interest in itself, offering great scope to investigators who love to unravel mysteries. It is, however, more than a problem to engage the interest of philosophers ; its solution is urgently needed by the photographic industry. Sensitive emulsions are now made in hundreds of different ways to suit a great variety of needs ; their manufacture is, however, largely based on empirical knowledge. Every new grade of material represents a great number of trials. If we but knew in precise chemical terms what is needed to obtain a given photographic result much of the work of trial and error would be avoided.

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POPULAR SCIENCE

THE ROMANCE OF SCIENCE IN BYGONE LONDON

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Although London possesses no public building that recalls to mind so notable an experiment as that performed by Galileo from the leaning tower of Pisa, yet natural philosophers of former days have followed his example in our metropolis with no less success, though with less enduring fame. The Tower, the Monument, Westminster Abbey, and St. Paul's Cathedral, to mention no others at present, have each served as improvised laboratories, and even Father Thames has carried argosies along the stream of scientific discovery. If we begin our tale at the Tower and work in a westerly direction, we shall move more or less parallel with the chronological development of the City itself. Among the many interesting prisoners in that roomy fortress, who have participated in experimental research, was the illustrious Sir Walter Ralegh. When forcibly deterred from venturesome explorations for King and country, he was nevertheless allowed opportunities for exercising his active mind in chemical investigations and allied pursuits. He had private quarters in the Bloody Tower, and a "still house" in the Tower gardens, in which he concocted his Great Cordial or Elixir, whose virtues as a remedy for fleshly ills were so extolled, that those at Court who held him in disfavour were not averse to taking his medicine during their bodily indispositions. A sort of Invisible College was really in existence at this time within the Tower, preceding the more renowned one commenced in Oxford by Boyle, Petty, and others. With Ralegh was another distinguished political prisoner, the ninth Earl of Northumberland. On account of his dilettantism in occult science with a crystal sphere, he was known as the "Wizard Earl." A third member of the same coterie was Thomas Harriot, free to come and go as friend and tutor to Ralegh and Earl Percy. Harriot as a mathematician popularised the study of Algebra in England, and on Ralegh's recommendation was employed in surveying the province of North Carolina.

He taught the nobleman the elements of Astronomy, and constructed for him a sun-dial on the south face of the Martin Tower. Subsequently, Northumberland allowed his congenial tutor a pension for life, and gave him a place of retreat in his old age at Sion House, Isleworth.

The popular idea of many visitors to the Tower is that it has been mainly a prison and a place for chopping off people's heads. In reality, it has given accommodation to the Mint, as it now provides protection for the Crown jewels. It has even found house room for a Royal menagerie. For a long time it has been a storehouse of arms and ordnance, and afforded residence for Government officials engaged in national surveys. As a matter of fact, it was a Surveyor-General of Ordnance, Sir Jonas Moore (1615–81), who established a private observatory in the north-east turret of the Tower and invited the Reverend John Flamsteed to take charge of it; a modest beginning that culminated in Flamsteed's election to the position of First Astronomer Royal at Greenwich. By a lucky coincidence, Moore's temporary observatory was close to a tidal river, and Flamsteed was able to compile tables showing the connection between the "southings" of the moon and the time of high tide at the adjacent wharf. When Charles II heard that the study of astronomy could help the art of navigation, he became interested in the establishment of a National Observatory, and Flamsteed was recommended to His Majesty for the post of Astronomer Royal.

From our point of view, the most resourceful department in the Tower has been that connected with ordnance and surveying; for again we select from among its officers the name of Robert Porrett (1783–1868), whose father was storekeeper at the Tower. As assistant to an indulgent parent, the younger Porrett found plenty of time for scientific research in a laboratory of his own contrivance. In 1818 he discovered ferrocyanic acid, and showed by the electrolysis of its salts that the iron atom must be regarded as part of the acid radicle. He contributed several papers to the Chemical Society, of which he was an original member, and also demonstrated that the luminous portion of a candle flame is surrounded by a non-luminous mantle. Speaking of flames brings to remembrance the question of adequate illumination for night operations in trigonometrical surveying and the efficient equipment of lighthouses before the advent of the electric light. Argand oil-lamps, even with reflectors, were lacking in penetrative power, and the man who could invent an illuminant of surpassing brilliance was sure of fame if not of a fortune. Such an inventor appeared in the person of Thomas Drummond (1797–1840), who came from Edinburgh to Woolwich Academy

as a cadet, and subsequently was engaged on the national survey. While lodging in London he attended the lectures of Faraday and Brande at the Royal Institution, and hearing at one of them of the remarkable incandescence of heated lime, he set to work straight away, in the hope of devising a source of illumination to eclipse the comparatively feeble rays of the Argand lamp. In his earnest endeavour he succeeded completely, and had the satisfaction and honour of exhibiting his well-known Drummond lime-light before the leading men of science in the metropolis. Previous to its trial in the field it was tested in the vast armoury of the Tower. The common Argand burner and parabolic reflector of a British lighthouse were first exhibited, with considerable effect in the darkened room. Fresnel's lamp was next brought into play, dwindling the former with ease ; but when the gas began to play on the lime, a glare shone forth, overpowering its predecessors as the sun outshines the moon. A shout of triumph and admiration burst from all present. Sir John Herschel, who witnessed the demonstration, was unstinted in its praise.

Naturally places adjacent to such a commanding edifice as the Tower would share in its notoriety. Thus there are the Tower Bridge, Tower Hamlets, Tower subway, etc. ; and now our road lies up Tower Hill, where once resided the mathematician and surveyor, Robert Norwood (1590–1675). He published a book on Navigation called *The Seaman's Practice*, now so rare and priceless that the hands of bibliophiles itch with avarice at the mention of it. Like Dick Turpin, he is famous for an unusual journey to York ; but whereas the former fled on horseback, a fugitive from justice, Norwood walked all the way in easy stages. Starting from Tower Hill, with a Gunther chain, quadrant, and mariner's compass, he resolved to compute in a practical manner the length of a meridian of longitude. With the chain he estimated the actual distance traversed, with the compass he corrected deviations from the meridian, and by the help of the quadrant he found the difference in the altitude of the sun at the extremities of his journey. Another famous resident of Tower Hill, of the succeeding century, was William Sherard (1659–1728), physician and botanist. Having extensively travelled in the East, he settled here for some time, cultivating in an extensive garden the various specimens he had assiduously collected abroad. By his will he left £3,000 to found a professorship at Oxford on the condition that his friend Dillenius, whom he had brought from the Continent, should be chosen first professor. Our last reference to residents near the Tower concerns a Mr. Jackson, who kept a chemist's shop hereabouts towards the end of the eighteenth century. He utilised his knowledge of materia medica for the purpose of

brewing a kind of beer with drugs instead of with malt and hops. He offered to reveal his secret for a substantial sum to brewers—among whom was Dr. Johnson's friend, Mr. Thrale—only to dupe his credulous pupils, who could hardly complain in having a master who had robbed them, as they in turn hoped to defraud the public. Some years before his death Jackson kept by him a patent coffin, and proudly showed his acquaintances how well his final resting-place fitted him.

Our next halt will be at the Monument, a few minutes' walk from the Tower. Young folk look forward to the ascent of this lofty pillar, which involves a tiring climb up numerous steps. Nevertheless, on a fine day, their trouble is well repaid by the sight of the throbbing City below, with its acres of roofs and miles of surging streets. It is said that Robert Hooke (1635–1703), acting as curator to the Royal Society, which met in his day at Gresham College in Broad Street close by, used the Monument for an astronomical observatory. Finding, however, that traffic and wind rendered it unsteady for precise results, he sought a more rigid support elsewhere. A few years afterwards, another Fellow of the Royal Society, the Reverend William Derham (1657–1735), conducted the Torricellian experiment here with a portable barometer of his own construction. The alteration in the pressure of the atmosphere with height above sea-level was at that period still a matter of much interest, and just as Pascal had mounted the steeple of a church in Paris to investigate the variation of atmospheric density, so Derham followed suit by climbing the Monument for a similar purpose. According to his results a difference in altitude of 82 feet produced a fall in the column of mercury of $\frac{1}{10}$ inch. Derham was a frequent contributor to the *Philosophical Transactions*, his researches on the speed of sound being of European renown. It may be worth mentioning that he put the labours of his acoustical estimations to a practical use, trying to find the width of the river at Woolwich by shouting across the Thames. From the interval of time between voice and echo he was enabled to obtain a fair approximation of the width of the stream.

As an exercise in acute observation, it is worth noticing what ornaments or embellishments surmount buildings of any considerable height. It may be only a vane or a crowing cock. One English cathedral is topped by the figure of a donkey. Let sightseers take note in future! At the top of the Monument is an urn with burnished tongues of flame—a fantasy to commemorate the tragedy of the Great Fire. In this finishing touch, the architect, Sir Christopher Wren, builded better than he knew.

Periodically, lofty buildings are struck by lightning; but it

was first observed by a London mathematical instrument maker, Edward Nairne (1726–1806), that certain edifices in the City seemed immune from Vulcan's fiery darts. In the case of the Monument he attributed this freedom from danger to the projecting gilded flames; likewise, he remarked, similar means of protection are to be found in the antennæ and legs of the grasshopper above the Royal Exchange, as well as in the tongue and tail of the dragon on the steeple of Bow Church in Cheapside. During the evening of March 15, 1824, the Monument was illuminated from base to summit by gas compressed into portable cylinders; an innovation for the benefit of public consumers. This special rôle of lighthouse on the part of the Monument was to celebrate the laying of the foundations of the present-day London Bridge, which crosses the river near-by. Strange to say, it was in similar gas cylinders that Faraday first discovered the presence of an important new compound which he named Phene. By cooling the compressed gas in a freezing mixture, he obtained a liquid which, on being distilled, yielded the very valuable substance since known as Benzene.

Leaving now the odoriferous region of Billingsgate and wending our way along King William Street, we soon come to the Royal Exchange, the third building of that name to occupy this central position on the map of London. The second Royal Exchange, with its piazzas and superincumbent shops, was bounded on its east side by Sweeting's Alley, a thoroughfare that was built over when the last edifice was erected. In the eighteenth century, of which period we are at present speaking, clockmakers and instrument makers abounded in the vicinity. Edward Nairne, whom we have already mentioned, lived near at hand in Cornhill; David Quare, the inventor of the repeating watch, had a shop in Exchange Alley, and a few yards away, in Finch Lane, James Watt worked as a mechanic during his short stay in the metropolis. However, it is of another eminent horologer we wish to speak now; namely, John Ellicott, F.R.S. (1706–72), of Sweeting's Alley, who had the surprise of his life on the day that he happened to place two of his most accurate clocks side by side, each touching the rail that projected from a wall of his shop. In some mysterious way one of the timepieces persisted in stopping every two hours, although he set its heavy pendulum in motion with every encouragement for its usual span of running. In order to discover if the tramping of his customers' feet in any way affected the regularity of action, he placed the two clocks on the stone pavement under the piazzas of the Royal Exchange. This alteration of foundation restored the chronometers to their normal accuracy, so that evidently a steadier base made for better results. To determine if their

freakish behaviour could be repeated on the stone floor, he put between the pair of them a wooden rod which lightly made contact with the clock cases. Once again the instruments defaulted, and under certain arrangements could so arrest each other's movements that they stopped and started alternately. The origin of the mystery was thus solved. The wooden rail against which they had at first been placed had transmitted infinitesimal vibrations from the swinging pendulums and cases, allowing the clocks to affect one another sympathetically.

From the Exchange along Cheapside it is but a short distance to St. Paul's Cathedral, the dominating landmark of the capital, and the premier object of admiration to the average sightseer. When men, in the cause of science, perform experiments inside a sacred edifice, it might appear to other laymen as an act of sacrilege. Imagine a pail of water hauled up inside the dome of St. Paul's and unceremoniously upset in order that a few members of the Royal Society might witness what effect the resistance of the air would have on the descending bucketful of liquid. Would the fact that the water finally reached the floor like a shower of rain justify this act of indecorum? However, domes with whispering galleries seem to invite scientific investigations. The experiment just cited really had its commencement in Newton's guinea and feather exploit. Those who have seen a little bit of fluff keep pace with a coin as the two objects fall in an evacuated glass tube, must agree that modern conjurers should occasionally include this trick in their repertory. What wonder then that the resistance of the air to gravitating objects should be investigated by Newton's contemporaries? So it came about that Sir Martin Folkes, Edmund Halley, and Dr. Theophilus Desaguliers, respectively the President, Secretary, and Curator of the Royal Society, with George Graham acting as timekeeper, one day, in the reign of George I, entered the cathedral to explore those properties of the air which impede descending bodies with such curious results, and which we know now are involved in the buoyancy of that latest invention of man, the aeroplane. Among the objects let loose from the upper regions of the dome were balls of lead, of glass, spheres of pasteboard, and even pigs' bladders. We are told that all the leaden balls fell within a foot of one another, and made an impression on the floor-boards about one-third of their depth. Obviously the greatest difference in the times of descent occurred between the lead and the bladders, sometimes as much as an interval of nineteen seconds separating them. Another matter in which there would be some diversity of opinion was the exact height of the top of St. Paul's from a given horizontal plane. One person well known to students of Physics, General Roy—and who does not remember the text-

book partnership of Roy and Ramsden?—estimated the height of the cathedral both with the help of a barometer and a quadrant. He also performed similar determinations on another well-known landmark, the Pagoda in Kew Gardens; but that, by the way. In both instances he concluded that the Torricellian method was biased too much by fluctuating meteorological conditions of the air to be really dependable. Roy, in his trigonometrical surveys, used the tops of St. Paul's and Westminster Abbey and the summit of Shooter's Hill as peaks in his plan of triangulation. A subsequent attempt to calculate the altitude was made by the Reverend Francis Wollaston, the elder brother of Hyde Wollaston, interim President of the Royal Society. He called his apparatus "a Thermometrical Barometer." With it he found the boiling-point of water on a bookseller's counter in St. Paul's Churchyard, and then again in the golden gallery of the dome. From the difference in temperature of ebullition he computed the distance between the two vertical extremities.

Reference to the churchyard allows us to remind country visitors that the precincts of the cathedral have always been the scene of various business activities associated with a great city. Trade, vehicular traffic, and social amenities in neighbouring taverns centred around this ecclesiastical hive. Peter Dollond (1730–1820) the optician, had a shop in St. Paul's Churchyard; Cocker (1631–75), the arithmetical paragon, gave his address as Mathematician "over against St. Paul's Chain," and when Joseph Priestley (1733–1804) made his annual trips from Leeds and Birmingham, he lodged at Johnson's, the bookseller, in St. Paul's Churchyard. The pavement hawkers that offer picture post cards and the latest toy for sale are not, however, to be regarded as the lineal descendants of the rogues and cut-throats that haunted the dark alleys of St. Paul's in previous centuries. One tale of a local highway robbery in 1795 must not be omitted. It concerns Count Rumford, whose epoch-making experiments on the correlation of heat and mechanical energy have earned him enduring renown. After a stay of several years on the Continent, he returned to London, and on the way to his lodgings had to pass through the churchyard. Although it was on an early autumnal evening, his postchaise was held up at the pistol point, his watch and money taken, and what to him and posterity was infinitely worse, his trunk containing valuable instruments and manuscripts was stolen. In his own words he wrote, "By this cruel robbery I have been deprived of the fruits of the labours of my whole life, and lost all that I hold most valuable." One is forced to conjecture what his assailants did with the papers; perhaps they burnt them, perhaps cast

them into the Thames; or may one optimistically hope that Time will one day reveal them.

So far on our journey we have tried to draw romance from existing public buildings; but as we descend Ludgate Hill with the intention of reaching Westminster Abbey, via Fleet Street and the Strand, we may be permitted, in fancy, to visit one of the many coffee houses that prospered in this district. For preference we choose the London Coffee House, which once stood between St. Paul's and Ludgate Circus. With some hesitation we reconstruct a scene inside the tavern on a typical day of jollity and reunion. Benjamin Franklin is there exhibiting with gleeful satisfaction his Armonica, whose musical glasses sound the right note for a happy gathering; Joseph Priestley, the essence of good nature, hands round a newly printed *History of Electricity*, which he has recently compiled at the suggestion of the inventor of the lightning conductor; John Canton, the Spitalfields schoolmaster, is tapping the floor with the biggest poker he can find in order to demonstrate how strongly he can magnetise it thereby; while Emmanuel Swedenborg and Dr. Richard Price are discussing the question of religious toleration for Dissenters. But we must not stop long or often to reclothe scenes of bygone days in this district, or else Denis Papin, who lived in Water Lane, will want us to see the "digester" he has made for the King's laboratory, and in which he is preparing with superheated steam a feast of jellied gristle and bones as a surprise dish for the anniversary meeting of the Royal Society; and Thomas Tompion, the clockmaker at the corner of Whitefriars Street, will invite us to inspect his latest specimen of horological craftsmanship. We must keep to realities, and pursue the highway of Fleet Street and the Strand, the connecting link of the cities of London and Westminster. For remember, East is East even in this instance, and Westminster is West. As sightseers, however, we adopt an impartial attitude and halt midway at Somerset House, a building more imposing from the river than from the street. Apart from acting as a repository of wills and deeds, it has given accommodation to the Royal Academy, the Royal Society, and other distinguished societies, nowadays all sheltered under the roof of Burlington House in Piccadilly. Somerset Place is the name of the west terrace, and from this quarter Henry Cavendish (1731–1810) took samples of air to compare their composition with others obtained from rural South Kensington. As a result of his analyses, he could discover very little difference in the constitution of the atmosphere from day to day or from district to district. In Somerset Place also Jesse Ramsden (1735–1800) was given permission to test his standardised rods which Roy was to make use of, in

laying out a base line on Hounslow Heath for a national survey. By entering Somerset House other pages of its history can be re-read. During the tenancy of the Society, from 1780–1857, appropriate Government departments co-operated with committees drawn from Fellows of the Royal Society to discuss matters of national importance and of civic welfare. The results of such deliberations, in a practical form, were often exhibited to the public in the Navy Office in Somerset House.

Here were shown models of Sir Humphry Davy's copper-sheathed ships floating in tanks of sea water. Those provided with small strips of the more positive metal zinc showed no signs of corrosion ; while others covered with pure copper underwent deterioration and discoloration. Apparently metallic corrosion in sea water was to be a thing of the past, and thousands of pounds would be saved annually by the nation. Alas for the high hopes of Sir Humphry ; in practice marine shells and seaweeds adhered to the copper-plating and considerably hindered the progress of ships during a voyage. Davy took this failure very much to heart, and probably never completely recovered from the disappointment. In happy relief against this dark picture was another model ship fitted from top-mast to keel with a copper lightning conductor, the invention of Snow Harris, a Plymouth physician. This innovation stood up successfully to the tests imposed upon it by rigorous trials during thunderstorms. At first the British Government were chary of adopting the device, but when the Russian Navy was supplied with these protecting rods, and Harris was subsequently rewarded honourably by Continental Powers, our own Admiralty followed suit, and the Plymouth doctor was knighted and awarded a pension for life. At least one other research of national significance was begun here ; and that in the cellars of the tiers near the river. The question of standard weights and measures is of practical importance in all civilised countries. For example, what is the exact length of a simple pendulum beating seconds in the latitude of London ? This had been sought for many years by men like Bird and Kater ; but entire agreement was not reached, partly because the observations were carried out in unsuitable premises, where disturbing local tremors militated against precise results. In the deep underground cellars of Somerset House, however, jarring traffic and subterranean vibrations were absent. It was here the Reverend Richard Sheepshanks, F.R.S. (1794–1855), tried to improve on the labours of others, with the help of the most refined apparatus the Government could put at his disposal. While speaking of vibrating pendulums in this building, it is not straining matters too far, we trust, to state that Brook Taylor (1685–1731), who first treated

the subject of vibrating chords mathematically and with originality, spent his last days in Old Somerset House, which was pulled down to make room for the existing structure. The former mansion was built somewhat on the plan of Hampton Court, and had gardens and apartments for retired gentle-folk.

As a contemporary of Newton and the author of the celebrated theorem found in all works on the differential calculus, Taylor will always be remembered. On a spot situated on the other side of the road, and opposite Somerset House, stood a giant maypole, two centuries ago. Spring revellers sported round it for several years, until, its foundations becoming rotten, the lofty staff was a public danger. It was then that Sir Isaac Newton's offer of purchase was accepted, and the maypole was presented by him to the Reverend James Pound, Rector of Wanstead and uncle of Bradley, who discovered the aberration of light from the stars. The country parson was himself a devoted astronomer, and his enthusiasm infected his nephew with the laudatory consequences just mentioned. Wanstead Church steeple had served James Pound as a gnomon, and now the maypole was to act as a rigid support for the gear and tackle of his long-focus lenses. Obtaining accurate alignment in an aerial telescope with a field glass of 125 feet focal length was no easy task, but the Wanstead incumbent hoped for good results as a sequel to Newton's generosity.

So far we have hugged the banks of the river ever since we started our itinerary, and until we have reached Westminster Abbey we must not strike farther inland. Even so we have in mind a day's holiday on the Thames at a future date, when we hope to do a little beachcombing and angling in the cause of the history of science. The hand of Time, with the aid of the navvy's pick, has destroyed many historic houses along the Strand. Beaufort Buildings stood a little west of Savoy Hill. Here was once the workshop of James Sisson, who repaired for the University of Glasgow their model of Newcomen's steam-engine on which James Watt experimented in 1763, with consequences which were destined to revolutionise the construction of steam-engines for generations to come. On the other side of the road and nearer Charing Cross was the Lowther Arcade, with its neighbouring hall of scientific delights—the Adelaide Gallery. Here could be found all the latest inventions in working order, with men of real authority to explain their action and lecture on recent scientific discoveries. There were electric eels and galvanic coils to shock its patrons; lime-light lanterns to temporarily blind them; laughing gas to send them into hysterics, and buzzing propellers and steam-guns to render them partially deaf; with Wheatstone's magic lyre to send them home rejoicing.

Faraday visited the Gallery, and William Sturgeon (1783–1850), the shoemaker turned electrician, was on its staff of lecturers.

The fountains of Trafalgar Square, a short distance ahead of us, are about the only remaining sight to entertain us gratis on our journey towards the Abbey. The water which supplies the jets and basins is partly obtained from local artesian wells, a fact that appeared sufficiently interesting to Michael Faraday to form a subject for popular exposition before his fashionable audiences attending the Royal Institution. A short walk down Whitehall and we shall be in sight of Westminster Abbey. It may be news to some of our readers to hear that Foucault's famous pendulum experiment showing the rotation of the earth was satisfactorily performed in the nave of the Abbey soon after the French savant had exhibited it publicly at the Panthéon in Paris. Incidentally, you may see it in action nowadays at the Imperial Science and Art Museum, South Kensington. Frank Buckland, the son of the Dean of Westminster, saw Foucault's pendulum at the London Polytechnic, and being of an experimental turn of mind, he realised how well it would work in such a lofty building as the Abbey. His pioneering propensities also led him to experiment on animals with chloroform, when that anæsthetic was first brought into use. Having a private menagerie at the Deanery, he doped birds, beasts, and fish with it. The antics of his pet monkey before and during the operation make very good reading.

(*To be concluded.*)

ESSAY

CURIOUS BIRDS AND CURIOUS IDEAS (Douglas Gordon)

THERE can be no doubt that the surest test of a book lies not so much in its popularity when first produced as in the impression that it leaves upon the reader's mind. From the heterogeneous collection of literature that comes under one's eye, now and again there stands forth one—famous or little known—that impresses itself indelibly upon the memory. This is particularly the case with Nature books, of which "atmosphere" is essentially the key-note, the principal merit of such works lying in the author's ability to make them realistic. When reading a story of wild-wood life, one does not desire a mere recital of the measurements and habits of the animals concerned. One likes to hear the wind in the pines, to catch the fresh scent of the earth and the wild blossoms; to hear the rush of swift wings, or the light footfall over dry leaves and along the forest byways; and to experience something of the intimate passions and emotions that influence the lives of the shy wild people.

I wonder how many animal-lovers of the present day have ever come across a delightful little book entitled *Wood Magic*, which, read once more than a quarter of a century ago, still holds first place among numbers of its kind that have since come under my notice. How it would strike one now I cannot say, for it was a child's book, something of a fairy-tale, its charm lying in the curiously vivid pictures of wild life. For the purposes of the story, the birds and beasts were depicted as a more or less civilised community whose king was not the stereotyped eagle, or even the "royal" stag, but none other than an exceedingly cunning and rascally old magpie.

This selection now strikes one as being peculiarly apt, for if mind is indeed superior to matter, there is no bird better qualified to take prominence than the ubiquitous magpie. Alert, keen, and vivacious, he is the very life of the wood that he inhabits, and wherever encountered, whether wild or tame, he may be depended upon to make his mark. Even in captivity, his personality is arresting, and in the words of an old writer, "notwithstanding his pilfering, no one can contemplate his arch dark eye, full of meaning, his inquisitive-

ness, his familiarity, and hear his efforts at mimicry, without feeling an interest in him."

As a general rule, the more distinguished of our birds have been somewhat overworked lately, to the neglect of other and equally interesting species. Ornithologists never tire of writing about "rarities," or of discussing old controversial points worn threadbare by long argument, but rarely indeed does one come across an article dealing with the curious life-history of such birds as the magpie, or his next of kin, the jay. Yet no birds are more deserving of notice, being two of the most beautiful species that adorn our British avifauna. They are at least "birds of gay plumage," imparting a touch of almost tropical colour to the wild life of the countryside. Few southern forests can show anything more vivid or picturesque than the iridescent gleam as a jay or magpie flashes down a long shaft of sunlight, its rapidly winnowing wings "catching the rainbow" with every light quick movement. Either bird is unquestionably an ornament to the woodland, and in the jay's case the additional charm and interest that his presence imparts is not offset by the admittedly serious disadvantages that attach to the magpie as a neighbour. To all intents and purposes he is comparatively harmless. It is possible that he succumbs to temptation now and again, varying his customary diet with a course of fresh egg or even nestling. Strictly speaking, however, he is not a carnivorous bird, and though closely related to the formidable *Corvus* family, in his tastes and habits he more closely resembles the pigeons. Indeed, one might almost regard him as supplying a link between the crows and the doves, if one could imagine so curious a combination of the wolf and the lamb.

The *Corvidæ*, it should be observed, embrace species representing many characteristics of bird life. There is the grim raven, monarch of the mountain-fastness, the peerless aeronaut, in many cases as formidable as and certainly far more courageous than the eagle himself. As befits the "head of the clan," he sets a high standard of craftsmanship, building his wonderful nest either upon some precipitous ledge or high in a mighty tree, by way of example to his subordinates. There follows the carrion crow, a raven in miniature, a branch-builder, rapacious and solitary, confirmed pirate of the woods and fields. Closely allied to the preceding species comes the hooded crow, who prefers to nest upon the rocks, and evinces so marked a predilection for the seashore that one might be justified in considering him the marine as well as the one migratory representative of the family. Next in the scale stands the rook, omnivorous, civilised, gregarious, the misnamed "crow" of ancient literature; and combining the qualities of both the

crows and the jackdaws we have the magpie, most elaborate of branch-builders, equal to the "hoodie" in merciless craft, and even more inquisitive and vivacious than the jackdaw himself. The latter bird, neither essentially gregarious nor solitary, provides an interesting variation from the common rule by nesting in holes and buildings, in which habit he is emulated to a limited extent by his rare and more refined cousin, the chough. And last of his order, so far as strictly British birds are concerned, there comes the jay, most wary and elusive of all, yet capable of astonishing boldness, who builds his nest in bushes for choice, a lover of fruits, berries, and acorns, sharing with the thrushes a keen relish for succulent grubs ; a bird who has contrived to shed both the sombre plumage and the sinister character of his dusky relatives.

It is worthy of note that while the magpie is undeniably a harmful bird, being an incorrigible egg-thief and guilty at times of far more serious offences, such as attacking disabled animals, one rarely hears a definite accusation against the jay. Never within my experience has he been known to approach a chicken-run or to remove an egg from outlying nests which are constantly raided by magpies. In all wooded districts the magpie is the veritable *bête noir* of the poultry keeper, and I have yet to meet the farmer's wife who does not agitate for the black-and-white marauder's destruction. On the other hand, the jay is known to the country-people, not as the egg-thief and robber of the preserves and hen-coops, but as a bird which shares with the blue tit an incurable fondness for green peas.

For this dainty the jay's craving is insatiable, and to gratify so luxurious a taste he will risk a great deal. It happens, therefore, that in districts where peas are not grown as a field-crop the usually wary bird throws caution to the winds, and ventures boldly into cottage gardens wherever the inviting green rows catch his eye, and so formidable is his onslaught that it frequently brings him to grief.

The jay is, in truth, mainly a vegetarian, and the game-preserver who pays a price for the destruction of this beautiful and interesting bird is not only subsidising a campaign of vandalism, but is wasting his money. Jays, indeed, are useful rather than harmful in carefully preserved coverts, since of all sentinels they are the most alert and quick-sighted. No enemy, whether human or furred, can move in the woods unchallenged. True, they care nothing for the game-chicks, but they care a great deal for the safety of their own broods, which in effect amounts to the same thing, and the warning screech of these jealous guardians is the surest indication that some disturber of the peace, usually in the shape of fox, cat, or stoat, is upon the prowl.

Their curiosity, moreover, is boundless, and any unusual object is safe to attract their attention. Gamekeepers occasionally turn this weakness to account, luring the birds within gunshot by the simple expedient of tethering a white ferret in some open spot in the plantations and lying in ambush near-by, knowing well enough that every jay in the neighbourhood will flock to the spot the moment that the strange animal is discovered and announced. This tendency, no doubt, has led to that curious habit of baiting owls which appears to be common to most members of the crow family. When a jay, poking about in the inquisitive way peculiar to himself, discovers an owl ensconced in some dark ivy-screened crotch, his excitement knows no bounds, and he gives vent to frenzied screams which every jay or magpie within half a mile hears and comprehends. The birds assemble with inconceivable rapidity, and before the owl is well awake he finds himself beset by a flurry of screeching, hectoring jays and magpies, whose discord so jars upon his sensitive ears that he is compelled to take flight, the jays following with all the clamour and enthusiasm of a fox-hunt, resuming the wordy war the moment the owl finds another perch. I have rarely seen them come to actual grips, and one sometimes wonders whether instinctive hostility or sheer love of mischief on the part of the aggressors is the motive. The whole affair suggests comedy rather than tragedy, but it must be remembered that one seldom has a chance of witnessing the actual end, or, rather, what would be the end were the birds left undisturbed. The presence of the human observer invariably breaks up the show sooner or later. It is at least possible that such an affair might develop into yet another case of "fascination," and that a jay or magpie rather than the owl would be the eventual sufferer. They employ precisely similar tactics against a fox, and personally I am inclined to believe that the wily beast turns this curious habit to account, allowing the birds to work themselves up to such a pitch of frenzied excitement that caution is at length thrown to the winds and one or more pays the penalty.

The scream of a jay in the early-summer woods may be taken as a sure indication of danger, since the wary bird uplifts his voice for no other reason at this time of year. Usually so talkative, he becomes curiously silent during the nesting season, and upon this account frequently escapes observation, the whereabouts of his nest only being revealed when the hungry young birds betray the closely guarded secret. There are few nests more difficult to find. Inconspicuous in itself, its site is cunningly chosen, being seldom placed at a great height above ground, but in some thick-growing shrub, such as a hawthorn or a holly-bush, outlying thickets or hedge-

rows being frequently preferred. The nest bears little resemblance to the elaborate structure which other members of the genus consider essential for the comfort or safety of their broods, being in many cases as scanty and almost as primitive as that of the wood-pigeon, which latter it somewhat resembles both in position and general appearance. Indeed, the old nest of a jay may only be distinguished from that of a pigeon by its slightly deeper " basin," and the scanty lining of fine roots, or occasionally hair, which is the jay's concession to the high standard of workmanship attained by his relatives. When they nest in a wood, jays usually select a larch plantation, failing which a birch-tree comes nearest to their notion of the " ideal home." There is no rule, however, for the keynote of the bird's character is originality.

It is probably owing to his extreme wariness during the breeding season that the jay has been able to hold his own in this country. Less numerous by far than the magpie, he is not only more easily killed by human methods, but the bird appears to suffer more from the attacks of natural enemies. A remarkable encounter between a jay and a sparrowhawk was recently described to me by an eye-witness. The meeting —neither sought nor desired by the jay—was a matter of seconds only, within which brief period the unfortunate Beau was " down and out," and would certainly have provided the hawk with at least one meal had the latter been allowed opportunity to reap the full fruits of victory. His nest again is liable to attack by robbers, to whose activities the casual observer gives little thought. Only this summer I found the nest of a jay high in a huge and half-rotten alder overhanging the River Taw, and from its unusual and somewhat perilous position considered it safe, so far as the egg-hunting boy was concerned. And so, indeed, it proved to be ; but there are more assiduous climbers even than boys, and a prowling stoat—the worst enemy to brooding birds—discovered and found prompt use for the entire clutch. The larder of a stoat, of which I once obtained a photograph, contained an extraordinary number of eggshells, wood-pigeons having been the principal sufferers. Jackdaws upon a neighbouring cliff had not escaped, however, and one of the most interesting relics was the unmistakable shell of a kestrel's egg. In the case of the jay's clutch just described, I imagine that the difficulty of transport must have necessitated a meal upon the spot, since the sucked and empty shells remained in the nest.

Not the least interesting point in connection with the jay's history is its alleged habit of mimicry. It is said to imitate, not only the notes of other birds, but even such sounds as the barking of a dog and many other animal noises. Upon this

matter, however, one can speak from hearsay only. Almost any individual of the crow family becomes an apt mimic when in captivity, but one must be allowed a certain amount of scepticism with regard to these and other species in the wild state. For my own part I have yet to be convinced that any wild bird ever *deliberately* imitated another's cry. Confined in a darkened cage, with other sounds excluded, a captive may easily be induced to imitate any note or sequence of notes, if these are repeated at close quarters with sufficient frequency. That is a matter of patience and concentration. Cut off from the influences which inspire natural vocal expression, the suppressed instincts of the bird find a ready outlet in the artificial means provided. In the wild state, however, it is difficult to imagine that the jay, or any bird, would experience the slightest inducement to utter notes other than its own. The circumstances, again, are so different. The ear of the caged bird inevitably becomes attuned to notes repeated constantly within its hearing. The wild bird, on the contrary, has for its auditorium the boundless expanses of earth and sky, a hundred wild voices are continually in its ears, from the symphonies of wind and water to the minute song of the midge. There is no individual sound upon which its senses would concentrate with sufficient intensity for future reproduction. One seldom obtains a clear impression of any particular voice in a crowded room amidst the buzz of general conversation, and it can scarcely be otherwise with wild creatures among the innumerable sounds of the countryside.

Again, one has only to imagine the inevitable consequence were the habit of mimicry at all common amongst even a limited number of birds. Such birds within a comparatively short space of time would certainly lose all individuality in the matter of voice. From constantly copying alien notes they would acquire a more or less nondescript call or song, comprising selections from the musical score of numberless other species. Not even an avian ear could distinguish the song of either friend or enemy, and chaos would be the consequence. As it is, there are many bird notes so similar that they are scarcely distinguishable from one another. Again, there are birds whose cries are numerous and varied, while a certain family likeness naturally exists between the voices of many kindred species. It is not surprising that the rook and the jackdaw should occasionally utter very similar notes, and in this, I think, lies the secret of mimicry among wild birds. It is by accident rather than design that one "imitates" another, though it is quite possible that birds which possess a natural tendency in that direction may now and again catch, and to some extent reproduce, an outstanding and familiar note.

There arises another point that is worthy of consideration. How big a part has reputation played in establishing instances of apparently deliberate mimicry? A fieldfare, for example, whom nobody dreams of charging with such levity, possesses a note curiously like one of the magpie's many articulations. That is dismissed—and quite rightly—as pure coincidence. On the other hand, when the starling—a renowned mimic—in his turn utters a very similar call, he is generally assumed to be exercising his peculiar power. The case of the starling is perhaps the most interesting of all and the most debatable. Competent ornithologists seriously claim that the bird—itself an indifferent musician—with deliberate intent appropriates and faithfully renders the strains of such accomplished performers as the blackcap or the wood-lark. Recorded instances are many, and there can be no doubt that observers report to the best of their ability things that they have seen and heard. It must always be remembered, however, that no field of observation allows wider scope for the possibility of mistake. Nothing is easier than to see one bird and to hear another. In such cases, moreover, a sound rule applies: any bird capable of singing has a song of its own, and is therefore under no necessity to borrow that of another.

It frequently strikes one as curious that in the present age of "discovery," while much useful knowledge is obtained upon the ways of wild creatures, so many old and more or less discredited ideas have been revived and accepted with enthusiasm. Mr. Gerald Gould recently informed us that "science is the continuous discovery of its own mistakes," and one wonders whether this statement will not prove true in connection with a great deal in modern Nature study. The keenness of the present-day observer knows no bounds, and for this, one would venture to suggest, sufficient allowance is not always made. The more cynical amongst us might go so far as to hint that the old adage "seeing is believing" has been reversed, and that, nowadays, to believe is to see. This may be considered somewhat severe. At the same time, nobody who knows human nature can deny that a keen desire to see or hear lends a wonderful aid to the senses. Another point seldom taken into account is the great difficulty attached to the task of obtaining perfectly correct impressions, and the slightest error upon the part of some eminent authority may give rise to an entirely incorrect idea.

As I have frequently remarked in this connection, it is far from being my intention to discredit the testimony of any observer, and this point cannot be emphasised too strongly. At the same time, discrepancies in the most reliable evidence give pause for thought. Take the stock example of the

woodcock. We are now assured, upon the testimony of unimpeachable witnesses, that this bird actually carries its young —a proceeding discussed and declared "improbable" by as old an authority as Gilbert White. Why the woodcock alone among birds should adopt this singular course, why it should be necessary, and whether it is a frequent or rare occurrence are questions among many which arise upon this interesting point. Let it suffice, however, that according to many observers, the process takes place, and it might be well to quote the impressions of two or three witnesses.

The late Richard Kearton informs us:

"We have seen a woodcock carrying her young between her legs, supported by the toes and claws of both feet, whilst flying through the air in the Isle of Mull."

G. M. A. Hewett, again, says, concerning the woodcock:

"People have written a great deal as to the way in which they carry their young. There may be many ways, but I am sure of one, for I could have twice touched them with my stick."

He proceeds to describe an occasion upon which he watched a female bird attempting to lift a young one "neither with beak nor feet, but with her wing." Then, he adds:

"Some years afterwards I disturbed quite a tiny one, and the mother again came and took it under her wing and flew off with it almost from under my feet, and I saw its head peep out where her wing joins her body."

Another naturalist, a Devonshire gentleman, unknown to literature, but none the less a shrewd and conscientious observer, once described to me the case of a woodcock which passed over his head in the twilight, carrying its chick in its beak. In each case the observer describes what he distinctly saw, but one cannot fail to remark that in no two instances is the account the same. In the first case, the chick is held between the legs; in the second, under the wing; while in the third instance, the long beak serves as carrier. This is worth consideration. Admitting the proceeding to be habitual, it seems curious that no prescribed method should exist. A hawk or an eagle carries its prey in its talons; when a rook carries food or building material, the beak is employed; and I have yet to see a dog transporting anything by any other means than in its jaws. Either the woodcock is the most original and versatile of birds, or two of the three observers mentioned were mistaken.

Possibly some readers may remember the case of a wolf

which escaped from a menagerie in Northumberland about twenty-five years ago. For a considerable time it harried sheep in the district until eventually killed by a train in a tunnel. Its death did not become known for some days, during which period even the stolid North-country farmers continued to see wolves prowling about their fields to such an extent that the question was seriously raised as to whether one wolf or many had been at large in the district.

This provides a characteristic example of seeing what one expects to see, and therein, one ventures to think, lies the explanation of many puzzling questions in Natural History study. The gamekeeper who has never heard of wild mimics would be surprised if, hearing the call of a crow and stalking the bird, he shot not a crow but a jay, and I suspect that the most confirmed supporter of the theory would be almost equally astonished, if, hearing the wail of a buzzard overhead, he looked up and saw a raven. The jay, however, tries no such tricks upon the countryman, who knows him too well. He reserves his performances for people who expect and appreciate them, even as the conventional ghost appears only to the confirmed ghost-seer.

In their ideas for the protection of their nests, jays and magpies differ curiously. That both birds live in fear of molestation or robbery is obvious, but their methods of defence are diametrically opposite, and while the jay appears to depend mainly upon secrecy and close guardianship, the magpie believes in fortification. An ideal site for a magpie's nest is deep in the prickly heart of a blackthorn thicket, the actual bush selected being one that not infrequently defies negotiation, except at considerable cost to clothes and skin. The bird is also peculiarly partial to an overgrown hedgerow of thorn or holly, and failing such a stronghold, it falls back upon height, as offering the greatest assurance of safety, usually building in the crown of some tall, willowy sapling, or among the topmost twigs of a lofty beech. In the woods it is also fond of larch trees, but, whether by chance or otherwise, I have never yet seen the great nest in a Scotch fir, which is a tree most favoured by other branch-builders. Not satisfied, however, with the strength of the position, the magpie invariably fortifies its nest with that marvellous prickly dome which is one of the great wonders of avian craftsmanship. How the bird constructs so strong and intricate a device with no other "tools" than its own beak and claws is a question more easily asked than answered. Feathered artisans, when nest-building, are supposed to achieve the beautifully symmetrical effect of the interior by means of their breasts, but the deft manipulation of stiff and formidable thorns offers no such plausible solution,

and in viewing such workmanship, one realises how far more remarkable is truth than fiction, in Natural History as in all else.

Why it should be necessary for the magpie alone among birds to construct such a fortress is another interesting question. Why, one wonders, should *his* nest need such elaborate defence any more than that of the crow, which otherwise it closely resembles, with the slight difference that the crow lines it with animal, the magpie with vegetable, substance? Is the formidable character of the crow a sufficient defence? It should be remarked that the nest of a crow rarely indeed comes to grief through natural means. One wonders whether it is against the crow himself that the magpie builds his thorn entanglement, or it may be on account of the fierce animosity that exists between the black-and-white architect and the common woodland hawks with whom he is perpetually at strife. Here, however, the question of cause and effect arises. Does the magpie build against the hawks on account of their propensity to annex his nest, or is that partiality for his nest merely due to a realisation of its obvious strategic advantages? The nest of a crow is seldom in such demand, though it is occasionally appropriated by other birds. There is always perhaps the uncomfortable possibility that the grim owner may return and reclaim possession, for the crow is not averse to repairing an old nursery. In the case of a magpie's nest, however, there is no such fear, since he builds a new home each year, the discarded nest being at a premium, particularly in country where the birds are not numerous. Kestrels, sparrowhawks, tawny owls, great-tits, and squirrels are among the creatures that appreciate its value, and the kestrel at any rate is not above making a bid for the desirable establishment when new, a desperate fight for possession being the consequence.

There is a peculiar magpie custom about which very little appears to be known. On certain spring evenings—I cannot say how often it happens—all the magpies within a considerable distance assemble in some favourite spinney, and there hold the strangest and noisiest conference one could ever wish to attend. Upon the first occasion when I had the good fortune to witness such a gathering, I was lying in wait for wood-pigeons in a larch planting. One or two magpies usually roosted there, but upon that evening I should hesitate to guess at the number that came in. The wood seemed full of them, and the noise they made baffles description. In the midst of it all some mischievous impulse prompted me to imitate the alarm-note, the effect of which was astonishing. There was complete silence for a moment, then a tremendous clatter of wings as every bird appeared to change places with its neigh-

bour—a sort of general post—followed by an outcry compared with which the previous clamour was nothing, and during the disturbance, which lasted for several minutes, I walked about in plain view without a bird taking the slightest notice of me. Before that night I had never heard of the "magpie conference," and not until years afterwards, when a fellow sportsman mentioned a similar experience, did I venture to say much about it. Upon the latter occasion, my informant shot no less than thirteen of the birds (with twelve cartridges) before the remainder took to flight.

The magpie is in every sense a more formidable bird than his gay cousin the jay. In the course of ages he has acquired much of the superstitious atmosphere that invests the raven. In Cornwall particularly, the worst of luck is expected by the countryman across whose path a single magpie flies. The jay appears to have escaped this sinister reputation, probably on account of his more retiring habits. There is no commoner sight than a party of magpies making merry not far from the roadside, or a single bird flitting on light swift wings between the woodland and the open fields. To encounter a jay in open country is a comparatively rare occurrence, however, and, when seen, he is usually "hugging" some hedgerow as though anxious to escape observation. So unfamiliar an object is he outside his own beloved woodland that even the countryman looks twice at him as a rule to be sure of his identity, and it is not difficult to understand the reason why a bird so shy has figured but little in folk-lore, tradition, or fable.

NOTES

Obituary: Waldemar Mordecai Wolff Haffkine, C.I.E. (R. R.)

On October 27, 1930, one of the greatest men of this and of the previous century died in France (?). He was a Pole, but had been working at the Pasteur Institute when he was a young man. During his studies there he became convinced that a method could be found for preventing cholera by means of an appropriate inoculation, and offered himself to the Indian Government in order to continue his studies on a practical scale in India. The Government accepted his services on a fixed salary, and I remember his arrival there in 1893. He soon elaborated a vaccine against cholera, and began to use it on numbers of natives and others. While his work on cholera was in progress, plague broke out in Bombay in 1896, and he turned his attention also to that disease, and produced a similar vaccine against it, which is still used there and elsewhere. His vaccines against both these diseases must have saved an enormous number of lives. But, not only this, his whole work stimulated the scientific investigation of sickness of all kinds, especially in India, which had been very slack in following up the work of Pasteur and Koch, and other investigators. Early in the present century (1902), however, a serious accident occurred at Mulkowal in India. His prophylactic against plague was put on the market, while supplies of it were also distributed to medical men and to Governments. Each phial containing the prophylactic was accompanied by exact instructions as to the dosage, and the precautions which had to be taken. Unfortunately, these instructions were not exactly obeyed when the prophylactic was used at Mulkowal, and the consequence was that it became infected with tetanus germs. Nevertheless, it was used on a number of people (I write only from memory). Nearly all these became infected with tetanus (lockjaw) and died. There was a tremendous revulsion against the prophylactic, and many British officials, especially members of the Government medical services who were rather jealous of Haffkine, took the opportunity to attribute the accidents to faulty technique in the preparation of the prophylactic, and Haffkine was actually blamed for this.

He returned to England in 1908, and sought the help of the profession here in order to rebutt the charge. Dr. W. Simpson (now Sir William) and myself, among many others, took up the cudgels on his behalf, and finally succeeded in getting him restored to his position in India, where he continued his previous work, though much disheartened by the treatment meted out to him. The whole incident was an example of the way in which scientific investigation was treated in India in those days. Those who were working for science received short shrift, and were looked upon as "enemies of the people," rather than their benefactors. Sir William Simpson and others have written able obituary notices of Haffkine, but there is a tendency to minimise the Mulkowal affair. Personally, however, I think that it should not be minimised or even forgotten. The same spirit has led to many other administrative errors, and is still tending in the same way. For instance, we hear much of Round Table Conferences, but not much of the fact that the Indian authorities spend vast sums on the *treatment* of malaria, while they often will not afford any money for its *prevention.*

The Atomic Weight of Dvi-Manganese (J. G. F. Druce, M.Sc.)

A full account of the researches describing the isolation of dvi-manganese, the element of atomic number 75, has already been given in SCIENCE PROGRESS (Jan. 1930, xxiv, 480; April 1926, xx, 590), and whilst the chemistry and physics of the element have not yet been fully investigated, a careful determination of its atomic weight has recently been carried out by Hönigschmid and Sachtleben (*Zeitschr. anorg. Chemie*, 1930, cxci, 309). These investigators converted silver perrhenate (per-dvi-manganate), $AgDO_4$, into silver bromide by means of hydrobromic acid. Altogether, 51·82860 gr. of $AgDO_4$ gave 27·17309 gr. AgBr, from which it was calculated that the atomic weight of dvi-manganese is 186·31 ± 0·02.

In the Periodic Classification, the element is placed between tungsten (At. Wt. 184·0) and osmium (At. Wt. 190·9), and since its discovery in 1925 several attempts have been made to estimate the atomic weight of the new element. The first of these was made by F. H. Loring (*Chem. News*, 1926, cxxxii, 410) who, from careful observations and calculations, gave an interpolated value of 187. Washburn (*Journ. Amer. Chem. Soc.*, 1926, xlviii, 2351) arrived at the high figure of 187·4, by a modification of Harkins's and Williams's scheme for calculating atomic weights.

The first actual determination of the atomic weight of dvi-manganese was made by Ida Noddack (*Zeitschr. Elektrochemie*, 1928, xxxiv, 629). The metal was heated in oxygen

at 300° C., so that it combined to form the volatile white heptoxide. By this method the high value of 188·71 ± 0·25 was obtained.

The results obtained by Hönigschmid and Sachtleben will undoubtedly be accepted as the most accurate.

Experiments have recently been made by Tropsch and Kassler (*Ber.*, 1930, lxiii, 2149) upon the catalytic properties of the element, and it has been found to assist in the hydrogenation of the ethylenic double bond and in the reduction of carbon monoxide to methane. Its catalytic effect was soon lost, however, unless it was admixed with copper.

Researches on Herrings (M. V. Lebour, D.Sc.)

A detailed investigation into the composition of the herring catches in the southern North Sea is contained in the *Ministry of Agriculture and Fisheries Fishery Investigations*, Series II, Vol. XI, No. 7, 1929: "Investigations into the Age, Length, and Maturity of the Herrings of the Southern North Sea. Part III, The Composition of the Catches from 1923 to 1928," by William C. Hodgson, M.Sc., Assistant Naturalist on the Staff of the Ministry of Agriculture and Fisheries, with a "Section on the Hydrography of the Area," by J. R. Lumby, Assistant Naturalist on the Staff of the Ministry of Agriculture and Fisheries (His Majesty's Stationery Office. Price 5*s.* net. London: Adastral House, Kingsway, W.C.2, etc.). These systematic studies by the Ministry on the East Coast herrings began in 1923, and a preliminary report was published in 1925. Continuous data relating to age and length for the main autumn herring fishery for the six years up to and including the season 1928–9 are now available. The ages were estimated in the usual way by counting the annual rings on the scales, the lengths by the newer method used by Wollaston and Hodgson, details of which can be found in their paper in the *Journal du Conseil*, Vol. IV, No. 2, 1929.

The East Anglian fishery is made up of a stock of fish many of which return to the ground year after year, a stock which is certainly receiving additions in the form of new year classes (a year class including all fish with the same number of growth zones on their scales). The records show that the stock of herrings in the western part of the southern North Sea may be considered to be a definite unit in the herring population of the North Sea as a whole. The age determinations point to a return each year of the old stock of fish, and from reasons given it may be inferred that there have been no outstanding additions of fish from other areas, where the length at a given age was different from that of the southern North Sea, nor any notice-

able migration from the stock. The stock itself appears to be self-contained, for it has been shown that the composition of a year class remained constant throughout the period of its participation in the fishery; and also that it was the same for both northern and southern samples.

The whitebait along the East Coast was studied, the proportion of herrings to sprats varying greatly. In some parts of the Thames Estuary the catch consisted entirely of young herrings. In the samples it was found that there were three groups, the commonest group being about 7–8 cms. in length, which is derived from a winter spawning during December and February.

The study of the vertebræ has given inconsistent results, the average number in samples of similar age composition varying much in different areas. The author is inclined to the view that this is mainly due to conditions of temperature at the time the fish were hatched, and the growth of these herrings is identical with that found in other areas along the coast.

The main bulk of the herrings are between three and ten years of age, and it may be said that the effective life in the fishery is about eight years. Winter-, autumn-, and summer-spawned herrings all enter into the composition of the year classes. The two big year classes, 1904 and 1924, have both contained large proportions of all three broods, the lesser year classes being chiefly composed of one dominant brood. In 1928 the 1924 year class was so abundant that it constituted 41 per cent. of the samples examined, and this is the greatest hope for the future. Herrings with the same number of growth rings in their scales may vary in age owing to the time of birth, therefore fishes of the same apparent age may vary in length. The 1924 brood contained a large proportion of fish which were 12–13 cms. long when they formed the first winter ring on their scales, the more usual type in the shoals showing a first growth zone of about 8–10 cms. The presence of the 1924 brood herrings raises the average length of the year class, and it is safe to assume that there will be a good supply of large herrings in the autumn fisheries of the next four or five years.

It is urged that it is important to discover what are the conditions necessary to bring about good productivity of the various spawnings, and it is suggested that workers in other countries which co-operate with the International Council for the Exploration of the Sea should find out the composition of the stock of herrings with regard to broods in their own particular area so that a rough idea may be obtained of the intensity of any spawning season for the whole of the North Sea.

A Critical Review of Literature on Amplifiers for Radio Reception (S. K. Lewer)

Radio Research Special Report No. 9 (London, H.M. Stationery Office. Price 5*s*. net), which contains a review and an extensive bibliography of the more recent literature on the design of radio receivers, has been prepared at the National Physical Laboratory on behalf of the Radio Research Board. Its object is to indicate to those studying the properties of receivers the lines along which future research may be profitably undertaken. To bring out the relative importance of the different sections, the review is essentially critical, and all the papers cited have been examined from this point of view. The bibliography is believed to be practically complete from 1916 to April 1929.

The Review is divided into four sections, each of which is subdivided, and a critical essay based on the study of the literature of each subsection is included. The four sections are: I, Radio Frequency Amplifiers; II, Rectification; III, Audio Frequency Amplifiers; IV, Measurements. Antenna design and loud-speakers have not been included, as these subjects are considered to open up large fields of literature which are not relevant to amplifier design. All the sections have been compiled by H. A. Thomas, with the exception of Section II on Rectification, which has been compiled by F. M. Colebrook. The subdivisions of the sections on Radio and Audio Frequency Amplifiers are grouped under the following headings: Circuit Arrangements and General Properties, Analysis of Amplification, Transformers, Retroaction and Stabilisers, and Analysis of Distortion. The section on Rectification includes the general theory, rectifiers other than thermionic valves, and valve rectification, while the last section deals with the constants of valves relevant to the design of amplifiers, the performance of rectifiers, radio and audio frequency amplifiers, distortion in receivers, and the overall performance of receivers. The abstracts given have been made by reference to the original paper in every case. Cross-references are given where necessary. A list of periodicals consulted and an author index are included.

There has been an urgent need for a Review such as this, for the development of all branches of valve technique has been rapid, and large numbers of important papers have been published in numerous journals. All the information of interest and importance is collected here in a most concise and lucid manner. As a reference book and guide its usefulness cannot be over-estimated.

The Measurement of Minute Electric Currents (S. K. Lewer)

The normal method of measuring extremely small currents is by some form of electrometer having a sensitivity of about 2,500 mm. per volt. A device having a sensitivity 100 times as great as this has recently been developed by the General Electric Co. of Schenectady, New York, and is described by B. J. Thomson in *Electronics*, September 1930. The device enables currents of the order of 10^{-17} ampere to be measured. It consists of a thermionic valve of the ordinary type modified to eliminate or minimise certain deleterious effects. In an ordinary valve the grid current even under the best conditions is seldom less than 10^{-10} ampere, and shows unpredictable variations of about 1 per cent. or more. Consequently a current of less than 10^{-12} ampere could not be indicated by such a tube. In this experimental tube the grid current has been reduced to 10^{-15} ampere, thereby enabling currents of 10^{-17} ampere to be measured.

The principle employed is not new. The current to be measured is passed through a resistance of about 4×10^{11} ohms, and the potential difference between its ends is applied to the grid and filament of the valve. The resulting change in grid potential gives rise to a change in anode current which is measured by a galvanometer. The chief problem was to reduce the grid current as far as possible, and with this in view, the sources of grid current were examined separately. Electrons reaching the grid directly from the filament, the main source of current, were prevented by applying a negative grid bias of 3·0 volts. This current was reduced to a negligible value. Ions, formed by collision of electrons and gas molecules, even with very thorough evacuation were responsible for 10^{-13} ampere. The pressure in the tube was then 10^{-13} atmosphere. This current was still further reduced by keeping all voltages below the ionisation potentials of any gases present. A low-temperature filament was used to eliminate completely the thermionic emission from a hot grid—heated by radiation from the filament, and to reduce the photoelectric emission from the grid. To reduce leakage, the grid was mounted on quartz beads protected from contamination by a glass skirt. Emission due to soft X-rays formed by bombardment of the anode became negligible when the anode potential was reduced to 6 volts. Positive ions emitted from the filament were prevented from reaching the grid by the use of a space-charge grid at a positive potential. Only when all these precautions have been taken is it possible to measure currents as small as 10^{-17} ampere. A current of this magnitude corresponds to a flow of about 63 electrons per second. The tube is finding many

practical applications, including spectroscopic and thermal measurements of stars.

Two-way Television (S. K. Lewer)

An experimental and demonstration system of two-way television was set up last April by the Bell Telephone Laboratories in New York. This constitutes a distinct advance in the development of television. Nearly four years ago moving images were successfully transmitted by wire and by radio. In 1928 this was extended to include subjects in broad daylight in place of studio lighting, and in 1929 television in colour was introduced. The latest development to be demonstrated is two-way television coupled with telephonic communication. A detailed description of the methods and apparatus employed is contained in three articles in the *Bell System Technical Journal* for July 1930. The articles describe the systems for image transmission, synchronisation, and sound transmission respectively.

The two-way television system is the same in principle as that used by the Bell Laboratories in 1927. In realising the duplication of apparatus, a number of characteristic special problems have arisen. In addition to solving these problems, various refinements have been introduced into all parts of the apparatus. The greatest improvement has been in the received image. The early designs used a scanning disc and a set of photoelectric cells at the transmitter, and a similar disc and a neon lamp at the receiver, but in the new apparatus there are two discs at each end, and a bank of photoelectric cells and a neon lamp at each end. The number of holes in the scanning discs has been increased from fifty to seventy-two. This provides just twice the number of image elements, and therefore a greater degree of definition. The frequency range of the transmission band is doubled by this change, and the communication channel must consequently have extremely good characteristics. A frequency band 40,000 cycles wide is required for each of the two television circuits. Compared with a band of only 2,500 cycles width, which is sufficient to reproduce intelligible speech, this serves to indicate the nature of some of the difficulties encountered in the transmission of high-quality television images.

In any television system, the accurate synchronisation of the moving parts is of the utmost importance. The scanning discs in this system rotate at eighteen revolutions per second, and are synchronised by a valve oscillator at one end of the line. A frequency of 1,275 cycles per second is found to be most useful: it serves to hold the picture at the receiving

end inside its frame within a small fraction of the picture width. A phase shifter is added so that the operator at the receiver can momentarily increase or decrease the control frequency in order to bring the picture into frame.

The intensely bright light required for transmission has tended to dazzle the eyes, thus rendering the rather faint neon tube image difficult to see. By using blue light this dazzle has been largely overcome, for the eye is relatively insensitive to blue light, while the photoelectric cells are particularly sensitive to this colour. The glaring effect is still further reduced by illuminating the booth with orange light. An additional small lamp throws orange light on to the wall surrounding the viewing frame. The scanning light is perceived by the user of the apparatus merely as a blue spot of light lying above the incoming image. The neon lamp used for reproducing the image has been considerably improved. The early tube was very simple, but the latest tubes are water-cooled, and can therefore operate at much higher intensities. A little hydrogen added to the neon improves the characteristics in many ways. The gradual absorption of the hydrogen is provided for by a small side reservoir.

The acoustic characteristics of the system are extremely good, though there have been no new developments on this side. The microphones are of the type used in broadcasting, and are situated about 2 feet in front of the speakers' heads. The ordinary telephone receiver is replaced by a loud-speaker of the moving-coil type with a piston diaphragm. The telephonic devices do not therefore interfere with the view of the speaker. Careful choice of the positions of the loud-speaker and the microphone and of the materials used in constructing the inner surfaces of the booth is necessary in order to prevent "singing," the feed-back of acoustic energy from loud-speaker to microphone. The attenuation is such that the speaker's voice seems to come from a distance of 10 to 12 feet. This acoustic illusion of distance is in harmony with the visual appearance of the television image.

The primary object in developing and installing the two-way television system has been to obtain information on the value of the addition of sight to sound in person-to-person communication over the telephone, and to learn the nature of the problems involved. The addition of television to telephonic communication appears to be of considerable value from many points of view. Although the present apparatus is complex and bulky, very costly to install and operate, and requires the services of trained engineers, its future can be regarded with justifiable optimism.

Our Politicians (R. R.)

The noted journalist, traveller, and lecturer, Sir John Foster Frazer, F.R.G.S., F.J.I., made the following remarks about the treatment which Sir Ronald Ross has received from British Politicians. "Governments," he said, "have always been willing to give huge sums to Field-Marshals and Admirals who have done a lot of killing; but they are meanness itself when it is a matter of rewarding a man who has saved millions of lives. Frequent appeals have been made that Parliament do something for Sir Ronald Ross. It takes no notice, and so the hat has to be sent round. I call that a national shame." Personally, I have long concluded that all our politicians of all parties are rather a miserable lot. As a matter of fact, nearly all real advances in almost any subject are made, not by politicians, but by private persons.

Sluggish Advances (R. R.)

Now that anti-malaria work is slowly beginning to be taken up in most malarious parts of the world, one is surprised to see, in an excellent article by B. C. Basu, M.Sc., in the *Indian Medical Gazette* for last July, a statement to the effect that in Bhagalpur in Bihar, India, "there is no previous detailed record of the *Anopheles* fauna, nor of the malaria of the place." Surely such neglect for thirty years is rather disgraceful to the sort of people who are supposed to govern India, as it must mean a considerable loss of life in the district referred to. It is not enough that a few enthusiasts should labour, perhaps for years, over difficult points of science, if ruling authorities ignore their findings for so long. Probably the general badness of British education and the game-playing and idling propensities of the British public are responsible. Their only excuse for the neglect is that other nations cannot be acquitted of a similar one, as a broad general rule.

Amentia (W. R. Le Fanu)

A most valuable collection of records and publications on Amentia has been presented to the Library of the Royal College of Surgeons of England by Dr. Thomas Brushfield, who was formerly in charge of the Fountain Hospital for Imbeciles at Tooting, under the Metropolitan Asylums Board. The records comprise the detailed histories which Dr. Brushfield compiled of all the children who came under his charge from his appointment in 1914 till his retirement in 1927, with photographs of the children taken on admission and after treatment. The histories are classified by the types of amentia

—mongolism, cretinism, microcephaly, etc.—and Dr. Brushfield has preserved elaborate details, not only of each child's physical condition and family history, but of the mental tests employed in each case, and of the progress of such as were fit to attend school. All these records are fully indexed, and numerous tables have been drawn up summarising and analysing the material from various aspects. These histories and statistics, covering so long a period from the time when the particular study of mentally deficient children was only beginning, are all the more valuable because they have not been continued on any such scale as was undertaken by Dr. Brushfield. He has presented them to the College Library in Lincoln's Inn Fields, so that they may be readily accessible for any research-worker in this subject, for whom they ought to prove of inestimable value, as there is no comparable collection of similar material available.

But besides his first-hand records, Dr. Brushfield has also presented his very large collection of papers and cuttings on amentia, gathered from all over the world. These will be kept together in connection with Dr. Brushfield's own records, and the donor has prepared a full index to them. The papers cover not merely the varieties of amentia, but whatever may possibly bear on the subject, and there are numerous entries in the index under such headings as Birth, Encephalitis, Endocrines, Skulls, etc. To complete the usefulness of the collection, Dr. Brushfield has compiled a bibliography of books and articles issued up to the present, of which there is no copy in his collection, on all the subjects covered by his index. He further very generously proposes to keep this bibliography up-to-date and to incorporate into the collection whatever publications he may continue to collect.

It is most sincerely to be hoped that there may be workers who will be glad to avail themselves of this carefully prepared material. May it also be hoped that some of them will care to continue the collecting of this special literature which Dr. Brushfield has so industriously and disinterestedly begun ?

Imperial Botanical Conference, 1930

This Conference was held on August 15, 1930, at the Imperial College of Science and Technology, South Kensington, London, the President, Dr. A. W. Hill, C.M.G., F.R.S., being in the Chair. In addition to a number of British botanists, there were present delegates from fourteen overseas Dominions or Colonies and from four overseas Universities or Research Institutes.

The findings of the various sub-committees set up by the

Executive Committee of the 1924 Imperial Conference to deal with a series of Resolutions of that Conference were announced. The President then invited discussion on the question of a subsequent Imperial Conference. Various members having expressed their views, the following Resolution was put from the Chair :

"That an Imperial Botanical Conference take place in England in 1935 shortly before the International Botanical Congress which is to be held in that year in Holland." This Resolution was carried unanimously.

The following Interim-Committee was appointed: the Director of Kew (Convener); the Keeper of Botany, Natural History Museum; the Professors of Botany at Oxford and Cambridge; a Professor of Botany of the University of London (to be nominated by the Chairman of the Board of Studies of the University); one representative of the Colonial Office, and one representative of the Dominion Office. It was further resolved that this Committee summon a meeting of British botanists in the near future for the purpose of appointing an Executive Committee for the said Conference.

Notes and News

His Majesty the King has approved the award of Royal Medals by the President and Council of the Royal Society to Prof. O. W. Richardson for his work on thermionics and spectroscopy and Prof. J. E. Marr for his work on the zoning of the palæozoic rocks. The President and Council of the Society have also made the following awards: Copley Medal to Sir William Bragg, for his work on radioactivity and crystallography; Rumford Medal to Prof. Peter Debye, of Leipzig, for his work on specific heats and X-ray spectroscopy; Davy Medal to Prof. R. Robinson, for his researches in the domain of organic chemistry; Darwin Medal to Prof. J. Schmidt, of Copenhagen, for his genetic studies in animals and plants, and his extended oceanographical expeditions; Hughes Medal to Sir C. V. Raman, of Calcutta, for his discoveries in optics.

At the anniversary meeting of the Society on December 1, Sir F. Gowland Hopkins was elected President; Sir Henry Lyons, Treasurer; Drs. H. H. Dale and F. E. Smith, Secretaries; Lord Rayleigh, Foreign Secretary. The other members of the Council for the year 1931 are: Prof. E. V. Appleton, Prof. G. Barger, Prof. A. E. Boycott, Prof. E. P. Cathcart, Sir Alfred Ewing, Prof. E. S. Goodrich, Prof. G. H. Hardy, Sir Harold Hartley, Sir Thomas Lewis, Dr. W. H. Mills, Prof. E. A. Milne, Dr. A. B. Rendle, Prof. R. V. Southwell,

Prof. G. I. Taylor, Prof. D. M. S. Watson, and Prof. W. W. Watts.

The Nobel Prize for Physics for the year 1930 has been awarded to Prof. Sir C. V. Raman.

We have noted with regret the announcement of the death of the following well-known men of science during the past quarter: Dr. D. Adamson, past-president of the Institution of Mechanical Engineers; Edward Allen, Baron Brotherton, industrial chemist; M. Paul Appell, of the University of Paris, mathematician; F. Cajori, physicist and professor of the history of mathematics in the University of California; Prof. H. B. Dixon, of Manchester; Dr. J. V. Elsden, geologist; Dr. J. W. Evans, F.R.S., past-president of the Geological Society; Dr. Lewis Evans; G. Fano, physiologist, of Florence; M. A. Giblett, meteorologist; Prof. A. Gullstrand, of Uppsala, professor of physiological and physical optics; Rev. J. G. Hagan, S.J. director of Vatican Observatory; Mr. H. R. H. Hall, keeper of Egyptian and Assyrian Antiquities, British Museum; Dr. E. B. Knobel, astronomer; J. A. Le Bel, For. Mem. R.S., chemist; Prof. W. D. Matthew, F.R.S., professor of palæontology in the University of California; Prof. E. R. Matthews, municipal engineer; Prof. J. F. Pompeckj, professor of geology and palæontology in the University of Berlin; Lieut-Col. V. C. Richmond, designer of the R101; A. E. Seaton, marine engineer; Prof. H. H. Turner, at Stockholm, where he was attending the meeting of the International Union of Geodesy and Geophysics; H. E. Soper, statistician; Capt. J. T. Ainslie Walker, expert on disinfectants; Sir William Walker, lately Chief Inspector of Mines, Home Office; Sir Francis Watts, principal, Tropical Agricultural College, Trinidad; Dr. H. W. Wiley, food chemist; Dr. E. H. Wilson, botanist.

Sir E. A. Sharpey-Schafer has been elected President of the Royal Society of Edinburgh.

By royal decree Senatore G. Marconi has been appointed at once Member and President of the Italian Royal Academy. He succeeds Senatore Tomaso Tittoni.

The Horace Brown Memorial Medal of the Institute of Brewing has been awarded to Dr. E. S. Beaven for his work on barley.

Mr. E. J. Butler, C.I.E., D.Sc., F.R.S., Mr. Kenneth Lee, LL.D., and Mr. N. V. Sidgwick, Sc.D., F.R.S., have been appointed members of the Advisory Council to the Committee of the Privy Council for Scientific and Industrial Research. The following members of the Advisory Council have retired on completion of their terms of office: Prof. V. H. Blackman, Prof. F. G. Donnan, and Prof. F. A. Lindemann.

Father Stein, S.J., has been appointed to succeed Father Hagen as director of the Vatican Astronomical Observatory.

The Alvarenga Prize of the College of Physicians, Philadelphia, has been awarded to Dr. H. H. Harris, University College, London, for an essay entitled, "Cod-liver Oil and the Vitamins in Relation to Bone Growth and Rickets."

The British Photographic Research Association has been disbanded by agreement between the Department of Scientific and Industrial Research and the manufacturers concerned in the Association.

The British Non-Ferrous Metals Research Association has leased a factory in Euston Street, N.W.1, where it is proposed to centralise its offices and provide accommodation for research laboratories and workshops. Dr. D. H. Ingall has been appointed Assistant Director, and Mr. G. L. Bailey, M.Sc., of the Metallurgy Research Department, Woolwich, Development Officer in succession to Mr. S. J. Nightingale.

In connection with the fourth general assembly of the International Union of Geodesy and Geophysics, held at Stockholm last August, an exhibition was arranged at the Museum of the Swedish Geological Survey to illustrate the importance of modern geophysical survey methods in mine prospecting. The work is carried out by a chain of associated companies, the English link being the Geophysical Company of London Wall (Chairman, Dr. John W. Evans, F.R.S.). Successful surveys by gravimetric electric and magnetic methods have been carried out in all parts of the world, notably in Texas, Sweden, and at Buchan's Mine, Newfoundland.

The 21st Annual Exhibition organised by the Physical and Optical Societies will be held at the Imperial College of Science on January 6, 7, and 8.

The new lecture theatre and library erected by the Royal Geographical Society in commemoration of its centenary this year were opened on October 21 by the Duke of York.

The U.S. Congress has allocated a sum of 6·5 million dollars for the extension of the United States National Museum. With this allocation the floor area of the Museum, now 9·5 acres, will be doubled.

The centenary meeting of the British Association will be inaugurated at the Wesleyan Central Hall, Westminster, on September 23 next. The presidential address will conclude the meeting on the evening of September 29. The sections of the Association will meet in various institutions near the University of London in South Kensington. The meeting of the Association in 1932 will be held at York, in 1933 at Leicester, and in 1934 at Aberdeen. In connection with the centenary meeting, efforts will be made to raise an endowment fund of

£40,000 to put the finances of the Association on an adequate and permanent basis.

In *Nature* (October 18) Prof. R. W. Boyle and Mr. D. Froman describe the remarkable results obtained when the Kundt's tube experiment is carried out with tubes filled with clear liquids in which stationary waves of ultrasonic frequency are set up by means of quartz oscillators. The nodes in the liquid are marked by curtains of bubbles produced by ultrasonic cavitation. The wave-length can therefore be measured and the phase velocity calculated. By varying the frequency of oscillation the data for a velocity-frequency curve is obtained. This curve resembles the selective absorption curve in optics. It shows a gradual decrease of velocity, as the frequency rises to that corresponding to the "absorption band," then the velocity rises suddenly, *e.g.* to twice its normal value, this rise being followed by a fall at first rapid and then more gradual as the frequency is increased. The effect is attributed to the vibration of the walls of the tube.

It has long been known, as the result of marking experiments, that small plaice transported from the over-crowded grounds on the Continental coast to the Dogger Bank grow at a much faster rate, and the International Council for the Exploration of the Sea has therefore appointed a Committee to study the financial aspects of the question, and to decide, if possible, whether the transplantation of, say, one million plaice can reasonably be expected to yield a commercial profit. Another Committee has been appointed to deal with research in the herring industry, the English representatives being particularly concerned with the study of the herring in the Southern North Sea.

The Cambridge University Press announces the publication shortly of a new work, *Radiations from Radioactive Substances*, by Sir Ernest Rutherford, J. Chadwick, and C. D. Ellis, to take the place of *Radioactive Substances and their Radiations*, published in 1913, and now for some years out of print. G. Bell & Sons have in the press a reprint of the fourth edition of Newton's *Opticks*. It is really amazing that a book of such outstanding interest and importance should have been obtainable for so long only in its original type. Bell's are also publishing *Faraday's Diary* for the Royal Institution. This work exists only in its original manuscript at the Royal Institution. It contains the full record of Faraday's experimental work, and includes many observations, of which there is no record in his printed papers. The *Diary* will occupy about six or eight volumes, and it is hoped to have some of them ready for the Faraday Centenary celebrations in September next. Prof. Bridgman's *The Physics of High Pressure*, which

is to appear in Bell's "International Textbooks of Exact Science," is now in the press.

The Eighth Annual Report of the Safety in Mines Research Board, which summarises the work of the Board during the year 1929, can be obtained from H.M. Stationery Office, price 1*s*. Progress reports of research are printed in an appendix. At Buxton researches classed under eight heads are being supervised by Prof. R. V. Wheeler. At the Imperial College of Science, Prof. S. M. Dixon has continued to direct work on pit props and wire ropes; other researches are in progress at the Universities of Birmingham, Leeds, Manchester, and Sheffield. Experiments on the effects of dust inhalation are being carried out for the Health Advisory Committee at St. Bartholmew's Hospital. The Board is in active co-operation with Comité Central des Houillères de France and with the U.S. Bureau of Mines.

The Committee of the Board engaged in studying methods for preventing accidents from falls of ground in coal-mines has issued its Report on the North of England coalfields, the last of six reports dealing with the coalfields of Great Britain. These reports are all published by H.M. Stationery Office, Adastral House, London, W.C.2, at the nominal price of 2*d*. net, to bring them within the reach of all classes of mine workers. The present report, which is freely illustrated with sketches and plans, describes the usual methods of working, which include a good deal of board and pillar work and adaptations of board and pillar to allow for the pillars to be extracted by longwall machine mining. It gives an account of colliery practice as regards the support of the workings, calls attention to the best methods, and makes suggestions where improvement seems possible. Besides giving full descriptions of methods of work in which steel supports are being used, it deals with many other matters affecting the occurrence of accidents from falls of ground. The Committee concludes with the remark that the best safeguard against accidents is close and systematic support under all conditions, whatever the method of work.

Appended to the Report are some examples of common types of accidents, with notes as to ways in which they might have been avoided, a preliminary report on some comparative tests of rectangular and half-round planks, a report on tests on wood baulks reinforced with old wire rope, a record of the variation of roof pressure on an hydraulic chock when a longwall machine was undercutting the coal along a face, and an extensive glossary of the mining terms used in connection with the methods of working and timbering in the North of England coalfields.

The *Report of the National Physical Laboratory* for the year 1929 (H.M. Stationery Office, price 11*s*. net) contains the report of the Executive Committee, of which Sir Ernest Rutherford is chairman, and detailed reports from the Superintendents of the several departments (Physics, Electricity, Metrology, Engineering, Aerodynamics, Metallurgy, and Froude Tank), the whole forming a most readable account of the activities of an institution which excludes no part of pure or applied physics from its purview. Only the finances of the Laboratory are omitted, and this is rather surprising in a document so complete in other respects as to give the names of the porters and labourers employed.

The routine tests included the examination of 621,303 clinical thermometers (an increase of 20 per cent.); 10,250 ordinary thermometers (25 per cent. increase); 2,702 telescopes; 98 standard cells; 7,423 glass measuring vessels; 1,331 weights; 174 watches; and 17,295 taximeters and taximeter gear-boxes. Among the watches was a 2-day deck watch made by the Zenith Watch Co., Le Locle, Switzerland, which obtained 97·3 marks out of 100 possible. This is the best performance yet recorded at the Laboratory. The best pocket watch, by Paul Ditisheim, gained 89·6 marks, another by Rotherham's of Coventry with 89·1 marks being a good second.

An accurate determination of the melting-point of palladium was made in connection with the recently established International Temperature Scale. The measures of this temperature, made by the N.P.L. (1,555° C. ± 2° C.), the Bureau of Standards, the Riechsanstalt, and the G.E.C. Nela Research Laboratory, now agree within 4° C. High frequencies are more easily measured than high temperatures, but even so the agreement among the determinations of the frequency of a portable quartz oscillator by the chief standardising laboratories is quite remarkable, as the following figures will show: France, 199,955; Italy, 199,954; Germany, 199,965; England, 199,961; U.S.A., 199,966!

Among the new instruments constructed during the year was a primary standard barometer. It is hoped that the height of the mercury column in this barometer may be measured against a line standard to an accuracy of ± 0·001 mm.

Among the many researches in pure physics carried out by members of the laboratory staff, we may mention a determination of the thermal conductivity of a single bismuth crystal when placed in a magnetic field. When the heat is flowing parallel to the trigonal axis, the application of a magnetic field of 11 kilogauss perpendicular to this axis produces a change in the thermal conductivity of 16 per cent.

The Froude tank has been much in demand for commercial

tests; the staff has been increased, and the tank operated during the evenings and week-ends. Even so, orders have had to be declined, and in consequence a scheme for the construction of a second tank has been prepared.

Vol. V, Part III, of the *Indian Journal of Physics* contains a very valuable paper by S. Bhagavantam, entitled, "The Raman Effect: Its Significance in Physics and Chemistry," with a complete bibliography of the papers dealing with the effect published up to the end of June 1930 – 350 references in all. In addition, this issue of the *Journal* contains Sir C. V. Raman's Annual Report for 1929 of the work and affairs of the Indian Association for the Cultivation of Science—a document which includes a properly audited balance sheet showing all the assets and liabilities of the Association.

Twenty years ago Mr. Murray published Sir Ronald Ross's verses connected with his researches on malaria. Sir R. Ross is republishing these verses (Harrison & Sons, Ltd., St. Martin's Lane, W.C.2) under the name of "In Exile," and at the same time is bringing out another book called *Lyra Modulata*, containing twelve poems in regular but unusual metrical schemes. The former book was the one so highly approved of by Mr. Masefield, the Poet Laureate, and by Mr. Cloudesley Brereton, and the latter consists of picked verses.

CORRESPONDENCE

To the Editor of Science Progress

PARTHENOGENESIS

From F. A. Bather

Sir,—In addition to the instances of parthenogenesis in Orthoptera mentioned by Prof. Nabours in your October number (p. 327), the following is likely to escape notice, as it appears in the *Report of the Colombo Museum for* 1929. The Assistant in Systematic Entomology (name not given) there writes :

" *Euantissa pulchra.*—A small, ant-like, black larval Mantis taken in the Museum compound was reared to maturity and proved to be of this species. After attaining maturity it was kept alive for some time, and produced several cothecæ, from which, strange to say, two larvæ hatched. As there was no possibility of the mother's having been fertilised (she was kept throughout in a small glass-bottomed pill-box), this would appear to be a clear instance of parthenogenesis in this species. The larvæ, unfortunately, died in early infancy, having refused to feed on the small flies (Drosophila) provided."

Yours faithfully,
F. A. Bather.

46 Marryat Road,
London, S.W.19.
October 8, 1930.

HEREDITY IN MAN

I

From Prof. R. Ruggles Gates

Dear Sir,—My attention has been directed to an anonymous review of my book, *Heredity in Man*, which appeared in your July number (p. 160). One must conclude from the internal evidence that the writer, who signs himself as β_{10}, is a biometrician of the old school, who still, thirty years after the event, fails to recognise the fundamental importance of the Mendelian laws in the study of heredity.

Experimental biologists, who have confirmed Mendel's principles thousands of times in plants and animals (and in innumerable cases the evidence is equally clear and indisputable in man himself), are hardly likely to be lured back to the biometric method in its old pre-Mendelian sense. On such a basis much of the study of human heredity is simply a quagmire of pseudo-accuracy based on erroneous biological premisses. The experimental science of genetics, based on co-ordinated investigations of the external characters and chromosomes of organisms, has built up an immense and secure body of knowledge whose roots have deepened with the development of biology itself. An attack upon the methods and conclusions of this science from a mathematical point of view, in the name of "accuracy," is nothing less than a pathetic anachronism.

I do not mean to imply by this that biometrics has no place in the modern study of heredity. We know in fact that mathematical treatment is being introduced to an increasing extent into certain analyses of the results of Mendelian experiments; and there remains the field of quantitative inheritance, where measurements are necessary, and where the phenotypic characters involved cannot be determined by simple inspection. Here mathematical analysis is a valuable weapon, although progress in the interpretation of such cases must necessarily lag behind the field where direct observation plays the decisive part. But the biometrician who sets out to investigate heredity, forgetting or ignoring the Mendelian results, is like a ship without a compass sailing without charts for an unknown port.

Any geneticist would therefore regard your reviewer's criticisms as, for the most part, captious and trivial. To reply to them individually would require considerable space, and I shall therefore content myself by referring briefly to two only, especially as the answers to the others would be similar. "β_{10}" quotes from my book (p. 160), "The frequency of syndactyly is probably not more than 1 : 1,000, and of polydactyly slightly higher," and asks on what these estimates are based. They are quoted from the papers dealing with the inheritance of these conditions, where they were obviously meant as nothing more than rough estimates of the order of frequency of their occurrence. Even though the order of frequency should prove to be less than 1 : 10,000 in certain populations, this ratio would still serve to indicate roughly the kind of frequency which might be expected and to compare it with a frequency of say 1 : 100,000 or 1 : 1,000,000. The answer to several other criticisms is in similar terms.

As a matter of a different kind, take the quotation from

my book (p. 56), "There is greater variability between the offspring of fleshy parents than of slender parents," with the remark, "Surely a very doubtful statement requiring the detailed evidence on which it is based." This statement, as the context shows, is taken from a paper of Davenport on the inheritance of body build, and is not stated as a proved fact, but as a conclusion reached by an investigator of this subject. Anyone wishing to see the evidence on which it is based is there referred to the original paper. Every biologist is aware that final proof or even indubitable evidence of the general truth of a point of this kind is very difficult to get in the present state of knowledge. But that is no sufficient reason for failing to refer to such tentative conclusions as have already been reached.

My book was, in fact, an attempt to express the present state of knowledge on the subject of heredity in man, and judging from numerous reviews in scientific journals, biologists appear to be agreed that it was not unsuccessful in achieving that aim. In the innumerable cases where the inheritance of a Mendelian abnormality is concerned, not even the most captious critic can quibble seriously about the facts and their explanation. The bulk of the book (except the last chapter, which is on racial crossing) is devoted to such cases, although an account is also given of other less readily interpretable subjects, such as inheritance of stature or the inheritance element in cancer.

Finally, it is necessary to point out one of the serious errors into which followers of the Karl Pearson school of biometry are led when they pin their faith to biometric methods while ignoring the biological facts of Mendelian inheritance. In several places in my book (*e.g.*, p. 77) I pointed out that the same defects, such as retinitis pigmentosa, may be dominant in certain pedigrees and recessive in inheritance in others. (Microphthalmia may be dominant, recessive, or sex-linked in different pedigrees.) This fundamental difference in manner of inheritance is nevertheless ignored by the Pearson school, all pedigrees of the condition being lumped together. Thus a "ratio" of normals to abnormals is obtained which is obviously misleading, and can have no biological significance whatsoever. When the biometrical school make such fundamental mistakes through shunning Mendelism, geneticists can hardly be expected to take their protestations of "accuracy" too seriously.

R. Ruggles Gates.

King's College,
Strand, London, W.C.2.
Oct. 23, 1930.

II

From β_{10}

DEAR SIR,—Prof. Gates's letter complaining of my review of his book—*Heredity in Man*—illustrates what constitutes the fundamental source of my criticism of the work, if it is to be judged as a scientific contribution to the literature on the subject. He says of my review, "One must conclude from the *internal* evidence that the writer fails to recognise the fundamental importance of the Mendelian laws in the study of inheritance." I maintain that the internal evidence warrants no such conclusion.

Biometricians have never failed to recognise the importance of Mendelism; they have many a time complained of the faulty interpretation of statistical material at the hands of Mendelians, and have repeatedly demonstrated that the material provided does not warrant the conclusions drawn from it; which is quite a different matter.

I am content to leave the detailed complaints in my review to the judgment of the reader, and to restrain my impulse to add to them; the student who will refer, as an example, to the work from which the author takes his statement [1] regarding the offspring of fleshy parents will find ample justification for my criticism of it; it is not, however, one such unreliable statement which I criticise, but the multiplication of them.

I would suggest that an author whose avowed purpose is to express the present *state of knowledge* on the subject of heredity in man should not refer to *tentative conclusions* which have been reached without indicating to the reader that these are not established facts.

β_{10}.

[1] *Proc. National Academy of Sciences*, vol. ix, pp. 226–30, Washington, 1923.

ESSAY-REVIEW

DARWIN AND DARWINISM. By Prof. F. J. Cole, F.R.S. Being a review of **The History of Biological Theories**, by Emanuel Rádl, translated and adapted from the German by E. J. Hatfield. [Pp. xii + 408.] (Oxford: University Press, 1930. Price 17*s*. 6*d*. net.)

It is difficult to frame an estimate of this work which will be accepted as just by the majority of interested readers. The so-called General Biologist, who is satisfied with speculative results only, and does not concern himself with the working literature of Biology, will probably regard it favourably as a serious and even important piece of criticism, but the specialist, who expects the history of Science to be recorded with detachment and scholarship, will think otherwise. For example, we cannot imagine any Geologist or Palæontologist reading Chapter xviii without indignation. The implication that the incompleteness of the palæontological record is a kind of wheeze to disguise the poverty of the results obtained ignores the most evident facts, not only of Palæontology, but of Geology itself. In most cases the sedimentary rocks represent but an infinitesimal proportion of the time spent in making them, and thousands of feet of strata over vast areas may either have failed to be deposited or have been subsequently removed. Hence these rocks would not give us a complete record, even if organic remains had been continuously preserved in them. It is doubtful whether Prof. Rádl has fully grasped the bearings of phylogeny as they appear to the Palæontologist.

The central theme of the work is Darwin and Darwinism, and its object is to illustrate the shortcomings and errors of the Darwinian system. Darwin himself comes in for severe and almost contemptuous criticism, and that so negligible an intellect should have produced such a profound effect on the thought of the world is an anachronism to which Prof. Rádl might have devoted a little attention. We are told that if Natural Selection does not occur, Darwinism must be abandoned. This is assuming that Natural Selection, and not the Principle of Evolution, is Darwinism—a confusion of thought which obliterates the part played by Darwin in establishing the fact, because his explanation thereof is not acceptable. Erasmus Darwin, according to Prof. Rádl, was richer in

original ideas than Charles, whose theories were original but not profound, and his arguments were monotonous and embodied little searching of the mind. " Darwin is dead." The Historian who has closely studied the early literature of Biology could produce dozens of Erasmus Darwins of equal importance and originality, but he would find it difficult to discover another Charles. Erasmus Darwin is known to the average Biologist as the grandfather of Charles, and but for that relationship he would, like others of his rank, have long since been lost in the fog of antiquity. There are, in fact, many passages in Rádl's history which make us suspect that Darwin must be one of his cherished aversions. The harmless and rhetorical reference to gravitation in the peroration of the " Origin " is perverted into a suggestion by Darwin that his idea must be compared with Newton's discovery of gravitation. Such a comparison is neither stated nor implied by Darwin, and is entirely foreign to his attitude of mind. Even Down House is dragged in to share the eclipse of its owner, and the peaceful home, which "formerly" attracted pilgrims from all parts of the world, is degraded to the level of a " girls' boarding school." Prof. Rádl can at least correct the latter point in his next edition. Down House is now held in trust for the scientific world by the British Association, and during the last year or more, when it has been open to the public, this famous house has been visited by thousands of interested persons, including a number of distinguished Biologists.

Three general observations are suggested by a perusal of Prof. Rádl's work: (1) In spite of its deference to topical research, an undercurrent of hostility to Science and a leaning towards formal Philosophy are reflected in many passages. It is difficult, in the face of such a bias, to produce a just history of Biological Theories. Thus Prof. Rádl accepts Evolution with ill-concealed regret—the theory not having " received any clinching scientific proof." (2) We are not convinced that the author has sufficiently explored the original authorities of the subject, a necessity which, it must be conceded, would be extremely laborious where so large a field is covered. He prefers textbooks and treatises of a general and critical nature, but ignores the material on which they claim to be founded. That it is possible to produce an historical treatise dealing with general principles which at the same time is based on a first-hand knowledge of the literature has been abundantly demonstrated by Miall, Russell, and more recently by Daudin. (3) Prof. Rádl fails to realise that the progress of Science may be compared with a succession of waves. Each begins as an unobtrusive but significant disturbance which rapidly gathers momentum until it acquires a velocity which

threatens to carry all before it. *But it finally recedes*, leaving behind a valid deposit, small out of all proportion to the stress and agitation which produced it. The fact is that the importance of any new movement in Science (and also in Medicine) is always ludicrously over-estimated, and when this is realised, as sooner or later it must be, the tendency is to swing to the other extreme, and to deny that it has any importance at all. But the *historian* must not be deceived by all this. He must not be so conscious of the turmoil that its modest contribution to knowledge escapes him, which is Prof. Rádl's particular weakness. The young Physiologist, obsessed by his own preoccupations, may be pardoned when he claims that morphology, cytology, descriptive embryology, the neurone theory, etc., etc., are dead, but such statements in the historian are inexcusable. A change of fashion in research is not so much a change of conviction as a growth of conviction. Thus morphology is not less *important* than it was before, but only less *popular* as a subject for research.

Prof. Rádl is evidently sympathetic towards the contention that Biology must submit to a critical overhaul of its scientific concepts by Philosophy. This contention assumes, not very civilly, first, that the Biologist is incapable of doing this for himself, and second, that a Philosopher with only a casual and second-hand knowledge of Biology can deduce general laws for Science by the exercise of that precious but treacherous faculty known as intuitive reason. To neither of these assumptions does the study of the history of Biology give the slightest support. On the contrary, the intrusion of Philosophy into biological theory has rarely been helpful, even when it has not actively clogged the wheels of progress. The Biologist must be trusted to deduce his own principles—a task difficult enough at the best, but impossible in one who has not a working knowledge of the phenomena.

We regret we have only space to refer to a few of the topics in respect of which Prof. Rádl's presentment of the case cannot be accepted. The work is marred by a number of errors in minor matters which, however, cannot be dealt with here. The chapter on "Spontaneous Generation" is perfunctory and beside the point. The author says that "Pasteur solved the problem—at least in the form propounded by Pouchet." Pasteur and Pouchet obtained results which appeared at the time to be mutually exclusive, and each observer accused the other of error. We know now that Pasteur was not wholly right or Pouchet wholly wrong. It would be possible to repeat some of Pasteur's experiments, and produce results of which Pouchet would have thoroughly approved. The amazing feature in this comedy is that neither investigator personally

encountered both types of phenomena, and it was only when they supervened in the experiments *of the same worker* (*i.e.* Tyndall) that it became necessary to dismiss charges of error, and to discover the cause, unknown to Pasteur and Pouchet, which lay at the root of these discordant and bewildering experiments. It was John Tyndall who rendered this great service to Biology. His researches are only less important than those of Pasteur, and in some respects transcend them. Tyndall's name therefore cannot in justice be omitted in any discussion of the doctrine of Spontaneous Generation. Rádl's views on Mimicry are inscrutable. He includes under this term Convergence, Protective Resemblance, the odour of the Stink-Fungus, and Mimicry properly so-called. He admits that not all these cases would be regarded by Darwinians [or by anyone else] as examples of Mimicry, and adds the extraordinary misstatement that no attempt has been made "to analyse the widely divergent phenomena included under this head, no attempt to deduce the laws of Mimicry." The work of Poulton and his collaborators has been overlooked here. The statement that Erasmus Darwin suggested the idea of "Mimicry" can only be accepted if for "Mimicry" we read "Protective Resemblance." Haeckel is criticised for introducing new terms, "the great majority of which were still-born," "as if every new word represented a new thought!" A number of Haeckel's terms have survived, and many do represent a new thought. Cœlomata and Gastrula (1872) and Metazoa (1873) may be mentioned as particularly striking examples. The fact that extravagant claims have been made for Haeckel's Biogenetic Law has forced Rádl and others into the belief, equally extravagant, that there is no truth in it whatever. "How could anyone," he says, "hope to read the past by studying what is happening to the embryos of to-day? If such a thing were possible we ought, from the study of modern Philology, to be able to guess at the contents of the lost manuscripts of the ancient world." The illustration is unfortunate. The study of modern Philology throws considerable light on the history of Language, and even on the people who employ it, and the same claim may be made for the study of living organisms. Prof. Rádl's attitude towards the Chromosome Theory is characteristic of his point of view generally. This theory may be described as a complex and pretentious edifice which conceals, it may be, a small kernel of truth. That it does not "help in the understanding of the whole phenomenon of sex" should not surprise any student of the History of Biology. The phenomenon of sex is a big business, and its comprehension will occupy many decades of human research. To pour contempt on the early stages of that progress simply because they

do not, and cannot, give us the complete story shows no small lack of judgment on the part of the critic. "But the men of science," says Rádl, "can only tell us of centrosomes and chromosomes; their exalted wisdom has revealed nothing more. Tant pis pour elle!" Or, in the words of the old philosophy, man was not made to contemplate midges, and anything small is of necessity contemptible.

In thus dwelling on certain defects, as they appear to us, in Prof. Rádl's work, it must not be assumed that its distinguished author has altogether failed in his task. Far from it. His provocative book is one which must be read and carefully studied by all who are interested in the history of Biology, but it expresses in the main the restricted outlook of the partisan rather than the exhaustive and considered judgment of the assessor, and it cannot therefore be regarded as a faithful guide to the history of Biological Theories. We have not compared the translation with the original text, but the English form, considered by itself, is admirable.

REVIEWS

MATHEMATICS

Advanced Mathematics for Students of Physics and Engineering. By D. HUMPHREY, B.A., B Sc. [Part 1, pp. viii + 120 ; Part 2, pp. 175] (Oxford University Press: London, Humphrey Milford, 1929. Price, Part 1, 6*s* ; Part 2, 7*s*. ; or in one volume, 12*s*. 6*d*)

MR. HUMPHREY has compiled a useful mathematical compendium for the students whose interest in mathematics depends upon its practical use to them. They will find here a great deal of the material that they need arranged in a convenient form, and illustrated by well-chosen worked-out examples and by unworked examples to which answers are given. The first part of his work includes chapters on series, complex numbers, some portions of the Calculus, of which an elementary knowledge is assumed, and the elements of Differential Equations. The second part deals with more advanced Calculus, more Differential Equations, including solution in series, Vectors, Three-dimensional Co-ordinate Geometry, Fourier Series, Determinants, Finite Differences and Least Squares. The treatment of most of these topics is necessarily brief, but the selection of matter is good, and the book can be recommended to students to whom the logic of mathematics is not of interest except in so far as it affects its practical working.

Unfortunately, it is necessary to accompany this recommendation with a good deal of criticism, for Mr. Humphrey is guilty at times of slipshod and even of false statements. He begins very badly in Chapter I, on Series. On p. 8 he defines a series in the words usually applied to a "sequence," although he uses the word " series " in the usual sense of an added sequence throughout the rest of the chapter. On p. 9 he commits himself to the statement, no longer true even for engineers, that only convergent series are of use in practice. On p. 11 he states d'Alembert's test wrongly, and uses the words " the limit of $\frac{u_{n+1}}{u_n}$ approaches unity "—a most misleading phrase. On p. 10 he says, " We shall consider the convergency of series consisting of positive terms only," and on p. 12 he proceeds to deal with series some of whose terms are negative. The list of unfortunate slips could be lengthened, though not in proportion to the number of his pages. On p. 64 of Part 2 we find, " For practical purposes Simpson's ordinary rule is quite as accurate as Weddle's." What does the author mean by " quite as accurate," and why will he encourage the student in loose thinking on anything of such practical importance as the theory of approximation ? Again, in his last chapter, he allows the reader to suppose that every directed quantity is a vector, and that the law of vector addition is in some way divinely bestowed.

The chapter on Complex Numbers breathes the usual faintly metaphysical atmosphere that makes the more independent student regard mathematics as a " wangle," and this atmosphere is to be found also in some other parts of the book. On the other hand, the treatment of Differential Operators is better than in many books of more mathematical pretensions.

It is a pity that the author must await the possibility of a second edition

before he can think again. A little careful revision and the addition of a few pages devoted to a frank discussion of difficulties would suffice to make his book as good as any of its class. It is already better than many.

R. C. J. H.

Cambridge Five-Figure Tables. By F. G. Hall, M.A., and E. K. Rideal, M.A. [Pp. 8 + 76.] (Cambridge University Press, 1929. Price 3*s*. 6*d*.)

This new book of tables contains little more than common logarithms and the values of sines and tangents with their logarithms. After this, it is surprising to be able to add that the authors have been quite original, and have based the arrangement of the tables upon an entirely new idea. The logarithms of all whole numbers from 1 to 9,999 are tabulated in groups of ten, and no differences are given. Instead, the tables are arranged in the reverse of the usual order, so that of the logarithms of two successive numbers that of the smaller appears underneath that of the larger. This enables differences to be read off at a glance. The same arrangement is used in the trigonometric tables ; the interval here used is one minute.

It is unfortunate that the last entry in each column has not been repeated at the head of the next column. To interpolate, for example, between 61° 59′ and 62°, it is necessary to make use of entries on different pages of the book.

Circular measure and exponentials share a single page ; natural logarithms and hyperbolic functions are absent. A brief list of physical constants, a table of atomic weights, and three pages of standard differentials and integrals complete the book.

Only experience can show how useful the authors' new method may prove to be. We hope that, if it should prove popular, they will give us some more extensive tables on the same plan, and that other authors will be stimulated to attempt still other plans to the refreshment of our tired eyes, so long sickened of the sight of columns in which the content is so regrettably unchangeable and the form unchanging.

R. C. J. H.

The Quarterly Journal of Mathematics, Oxford Series. Vol. 1, No. 1. [Pp. 76.] (Oxford : Clarendon Press ; London : Humphrey Milford, 1930. Price 7*s*. 6*d*. ; Annual Subscription 27*s*. 6*d*.)

This revival of the *Quarterly*, under the editorship of a group of distinguished Oxford mathematicians, is designed by them to take the place of both the old *Quarterly* and the *Messenger*. The number of English mathematical journals is small, and the yearly output of research steadily increases, so that the editors should find no shortage of matter. The first number indicates that they intend to set a high standard of quality.

We have no doubt that mathematicians generally will welcome the new venture, and wish it a life no less long and distinguished than its predecessor's.

R. C. J. H.

Mechanics of the Gyroscope. By Richard F. Deimel, M.A. [Pp. ix + 192.] (New York : The Macmillan Company, 1929. Price 17*s*.)

Prof. Deimel's book is one of a series (The Engineering Science Series) of volumes addressed to engineers, and must be judged from the engineering rather than the mathematical standpoint. The author attempts to give, in less than 200 pages, an account of rotational dynamics starting from first principles and leading up to such difficult applications as the gyroscopic

compass and the Schlick and Sperry stabilisers for ships. In this rapid progress he is guided by the educational theory that it is best to deal rather briefly with fundamental principles, "and then to get a real insight into them as a by-product of working at the harder parts." This theory would be very hotly contested, but as it would, on the average, find engineers in its favour and mathematicians in opposition, the author must be allowed to have adopted the method most suited to his readers. Nevertheless, when we find in his preface a statement that even a single particle may be regarded as a gyroscope, we cannot but suspect that his method may sometimes obscure principles even from himself.

He has, however, given a very useful account of gyroscopic phenomena and their applications. Of these, the gyro-compass claims most attention, nearly a quarter of the book being devoted to it. Many readers will find this section the most interesting and valuable. The Sperry compass is dealt with very fully, but features occurring in other makes of instrument are also discussed. The author wisely does not attempt to describe all the structural features of these instruments, but his diagrams give a very clear idea of their working. The theory of their principal motions is given.

The author has a clear style, and an engineer with some mathematical ability should be able to learn a great deal from his book. The topics with which it deals are steadily becoming of more practical importance, while textbooks on the Theory of Machines either ignore them or touch upon them very lightly. Prof. Deimel is thus to be thanked for a useful piece of work. That a mathematician is not quite happy with his methods is perhaps hardly a criticism of a book not intended for mathematical readers.

R. C. J. H.

Leçons sur l'Hydrodynamique. By HENRI VILLAT. [Pp. iii + 296.] (Paris: Gauthier-Villars et Cie., 1929. Price 50 frs.)

PROF. VILLAT is, to judge by his writings, a mathematician first and a physicist hardly at all. He deals with the analytical problems of hydrodynamics with a care and lucidity altogether admirable; but one does not go to his book to learn what relations, if any, exist between analysis and the known phenomena of fluid motion. The mathematician intent upon further progress in the solution of the intractable equations of fluid motion will find here a most stimulating review of a limited number of the paths along which research is proceeding and, provided that he does not mistake the book for an attempt at a comprehensive treatise, will have little but praise for the author's work.

Nearly 100 pages are first devoted to the two-dimensional motion of a perfect fluid, treated by the method of the complex variable. The conformal representation of a simply connected region upon a circle and of a doubly connected region upon an annulus are discussed, and some free streamline motions are dealt with. Finding the use of elliptic functions necessary, the author devotes a chapter to developing their theory, and succeeds in giving a remarkably inclusive account of their properties in no more than fourteen pages.

Most of the remainder of the book is devoted to an exposition of the recent work of Oseen and his school. What characteristics has a fluid motion derived from the general viscous motion by letting the viscosity tend to zero? This was the question at the base of Oseen's researches, and his answer to it was embodied in the set of integral equations which the author here establishes and of which he makes some applications. The general case of three-dimensional motion is first treated, and the special features of the two-dimensional case are then discussed. Returning to the three-dimensional case, the author then examines the problems of trans-

lational motion of a disk and of a hemisphere. In the last chapter he deals with the motion of a plate in a semi-infinite fluid.

Prof. Villat is especially to be thanked for the later portions of his book. He has presented in a concise and clear manner a great deal of matter not otherwise to be obtained except from the original memoirs. His account will be of great value to the increasing body of applied mathematicians who are finding in the problems of hydrodynamics a fascinating and fruitful field of work. R. C. J. H.

Introduction to the Theory of Fourier's Series and Integrals. By H. S. Carslaw, Sc.D., LL.D., F.R.S.E. [Pp. xiii + 368.] (London: Macmillan & Co., 1930. Price 20s.)

Prof. Carslaw's book, of which this is the third edition, is too well known and appreciated to need recommendations. It has for long been, and still is, one of the best introductions to the theory of infinite series and integrals, while, as a treatise on Fourier Series, it is probably more rigorous and more comprehensive than any other that adopts the same point of view. In assessing the present value of the book to mathematicians it is, however, very necessary to define this point of view with reference to modern standards. Since the appearance of the first edition, twenty-four years have elapsed, and in that period ideas on the subject of Trigonometric Series have undergone something of a revolution. Lebesgue Integrals and summable series, from being whispers among the elect, have become topics of undergraduate lectures, and Dirichlet's treatment of the Fourier convergence problem is regarded by the modern analyst as hopelessly outdated. Prof. Carslaw's position may be defined by saying that he adheres to the older methods of treatment, while bringing to their aid the spirit and many of the refinements of the new. Of Lebesgue Integrals he treats only in an Appendix, new to this edition; the excellent account that he there gives of them leaves no doubt of the ability with which he could have presented the newer theories had he chosen to do so. That he has not so chosen must be taken to mean that he addresses his book not to the analyst but to the growing body of mathematicians who, though interested mainly in the applications of their subject, are not content with slipshod methods of proof. Indeed, it may be said that the book will now be reaching, almost for the first time, those for whom it was originally written. Its publication in 1906 as the first part of a treatise on the Conduction of Heat was little less than an act of faith. At that date much English mathematical teaching and nearly all English text-books were innocent of rigour. Bromwich's *Infinite Series* and Hardy's *Pure Mathematics* had not been written; Whittaker's *Analysis* was still in its first edition, and was only beginning to make its influence felt. Prof. Carslaw's book was thus a pioneering work which, after a quarter of a century, may hope to appeal to the class of readers for which it was intended and which it has done much to create.

The new edition contains, in addition to the Appendix already mentioned, a short Appendix on Numerical Fourier Analysis. Among a number of additions to the text may be noted a section dealing with the Gibbs Phenomenon.

The publishers are to be congratulated on the very considerable reduction in price with, at the same time, no falling off in the quality of their production.

R. C. J. H.

Leçons sur la Théorie des Tourbillons. By Henri Villat. [Pp. 300.] (Paris: Gauthier-Villars et Cie, 1930. Price 65 frs.)

As in his recent *Leçons sur l'Hydrodynamique*, Prof. Villat here treats interestingly of those parts of his subject that interest himself and makes

no attempt at comprehensiveness. His liking for analysis and, in particular, for the complex variable leads him to devote over half of his book to two-dimensional problems. These he takes up after two preliminary chapters on the fundamental theory of vortex motion, and he is very soon dealing with the inevitable topic of the Kármàn vortex street and its application to the theory of fluid resistance. He then turns to some related problems, such as the motion of vortices in a fluid limited by parallel walls. A chapter on the applications of conformal transformation concludes this part of the discussion.

The author then deals with the cylindrical vortex, Kirchhoff's elliptical vortex, and Hill's spherical vortex before passing to the theory of vortex rings, which he dismisses rather briefly, considering their great interest. This brings us to p. 221, or nearly three-quarters of the way through the book. With the exception of a brief final chapter on vortices in a viscous fluid, the rest of the pages are devoted to Prof. Lichtenstein's general theorem on the motion of vortex configurations. This is a subject of considerable difficulty, and the author's careful exposition is very valuable. The difficult and important problems of vortex motion in three dimensions have scarcely begun to be solved except in special cases. Their very abstract and general treatment here will scarcely appeal to those who want specific and practical results, but the work of Lichtenstein is fundamental, and will form the basis for further advances. Prof. Villat is at his best when he writes of such abstract matters. He has the clarity of style so happily characteristic of the French mathematical school, and analytical difficulties are always in his hands reduced to their minimum without being in any way shirked. This is an admirable characteristic, and will gain for his book a considerable measure of popularity among those interested in its subject.

R. C. J. H.

The Theory of the Gyroscopic Compass and its Deviations. By A. L. Rawlings, Ph.D., B.Sc. [Pp. x + 191.] (London: Macmillan & Co., 1929. Price 10*s.* 6*d.*)

Dr. Rawlings's book, written as it is by one with a long practical experience of the instrument, is one that nobody interested in the Gyroscopic Compass can afford to neglect; but it suffers to some extent from confusion of aim. As his preface makes clear, the author hopes to make his work intelligible to engineers with only a rudimentary knowledge of mathematics; at the same time he attempts to present a very complete mathematical theory of the compass and its deviations, *i.e.* to carry out a considerable piece of mathematical research. The result will not be wholly satisfactory either to engineers or to mathematicians; the engineers will find much of the analysis difficult; the mathematicians will find it inadequate. This is not to assert that the author's results are incorrect. His instinct and his knowledge of the instrument may have led him to right conclusions. But everybody who has had experience of Rigid Dynamics will know how dangerous are the short cut, the apparently harmless assumption, and the approximation made at too early a stage. It is hardly to be expected that anyone but an experienced mathematician should be able to handle the general differential equations of so complicated a dynamical system, and Dr. Rawlings is to be congratulated upon a very courageous attempt to solve his problems in a more elementary fashion, but, until the general method has been carried out, it is impossible to feel quite comfortable with some of his results.

Clear descriptions of the various forms of compass in use to-day are given, with excellent photographs and diagrams, and every feature of the compass is discussed in a way that makes the book of value quite apart from its mathematical portions.

The author should not, however, use a textbook for the purpose of carrying on his private quarrels with regard to patents. This is a waste of the reader's time and of the publisher's paper, and is, besides, in the very worst of taste.

R. C. J. H.

Elementary Theory of Finite Groups. By L. C. MATHEWSON, Ph.D. [Pp. x + 165.] (Boston: Houghton Mifflin Company, 1930. Price $2.50.)

THE recent writings of Prof. H. Weyl and others have done much to extend the interest in the theory of groups to physicists, chemists, and to various other cultured non-mathematicians. Moreover, a large number of professional mathematicians have been deterred from entering this subject by the numerous difficulties met at the threshold thereof in various extensive treatises thereon. The present little volume seems to be very well adapted to provide a more elementary textbook on this subject than has hitherto existed in English. Its ten chapter headings are as follows: Examples of Groups and Some Definitions, Permutation and Permutation Groups, Transitivity and Primitivity, Some Groups of Movements and Other Special Groups, Some General Properties of Groups, On Abelian Groups, On Abstract Definitions and Some Well-known Groups, The Group of Isomorphisms and the Composition-Series, On Groups of Linear Substitutions, and Some Applications of Group Theory in Other Fields.

The presentation is largely by propositions and proofs and aims to offer the reader numerous units which can be easily assimilated. The author states in the preface that most of the chapters have been tried out once or more in the classroom. A considerable number of graded exercises are provided, including about fifty which are called "additional exercises," and are not necessary to understand the rest of the book, but aim to furnish material for those who desire to go more deeply into the questions under consideration. Fortunately, a considerable number of references are given to places where the reader can find additional information relating to the various subjects treated. The author does not confine himself to the material implied by the title of his book, as he refers also to groups of infinite order, and even includes a note on Lie's Theory. The theorem credited to Netto on p. 20 was given much earlier by Cauchy (*Exercises d'Analyse*, t. 3, 1844, p. 221), but the number of such errors is very small, and the statements are usually clear and to the point, so that the beginner in this field finds here but little that presents unnecessary difficulties. In fact, the author has here provided in a very skilful manner a simple introduction to a large number of useful modern mathematical concepts which are continually commanding a wider interest.

G. A. MILLER.

Theory of Functionals and of Integral and Integro-differential Equations. By VITO VOLTERRA, For. Memb. R.S. Edited by LUIGI FANTAPIÉ. Authorised translation by Miss M. LONG. [Pp. xiv + 226.] (London: Blackie & Son, 1930. Price 25*s*. net.)

THE present writer, having no previous knowledge of the Theory of Functionals, approached the subject with some diffidence. But he felt that his impressions of the subject, although only those of a student, would definitely be formed from Prof. Volterra's book, and so the reader would be given some idea of its merits.

A functional is a generalisation of a function. Now generalisation for the sake of mere generalisation may be rather a barren pastime. But when one is forced to realise that the theories of analytic functions, integral equations, and the calculus of variations are first cousins, that problems concerning

hyperspaces, the abstract theory of aggregates, integro-differential equations, and most of the fundamental problems of modern physics can be expressed concisely in the language of the Calculus of Functionals, one is tempted to believe the assertion of the publishers that the subject has "innumerable" applications.

The simplest description of a functional is that it is a function of a *non-enumerable* infinity of variables. An expression like $\underset{n=\infty}{(\lim)} f(x_1, x_2, \dots x_n)$ is a function of an *enumerable* infinity of variables. For example, the sum of the series $a_0 + a_1x + \dots + a_n x^n + \dots$ may be regarded as a function of the *enumerable* infinity of variables $x, a_0, a_1, \dots a_n, \dots$. But expressions like $\int_a^b x(t)dt$ or $\overline{\underset{a<t<b}{\text{bound}}} \frac{dx}{dt}$ depend in general on the values taken by $x(t)$ for the *non-enumerable* infinity of values of t between a and b. A functional is usually restricted to some "field of definition," such as the class of functions which are integrable or differentiable; the "elements" may also be abstract aggregates, instead of ordinary functions. The "distance" between two elements may be defined in an arbitrary manner, for instance as $\overline{\underset{a<t<b}{\text{bound}}} \left| x_1(t) - x_2(t) \right|$, where $x_1(t)$ and $x_2(t)$ are two elements. "Continuity" of functionals may then be defined in an obvious way, and the operations of differentiation and integration also have their analogues.

An important class of functionals is that of functionals which are *continuous* in a field of *continuous* functions, for these can be expressed as limits of functions of a finite number of variables, *i.e.* as functions of an enumerable infinity of variables. Another important class is that of the "linear functionals." A functional $F[y(t)]$ of the element $y(t)$ is said to be linear if $F[\lambda y(t) + \mu y(t)] = \lambda F[y(t)] + \mu F[y(t)]$. For instance $\int_a^b \kappa(t)y(t)dt$ is a linear functional of $y(t)$. Thus the problem of solving a linear integral equation is the direct analogue of the problem of solving a set of linear algebraic equations. Functionals of the second and higher orders may also be defined, and one is then led to "functional power series," whose sums define "analytic functionals." Differentiation and the problems of maxima and minima lead to the Calculus of Variations, and so on apparently without end.

The editor and translator have obviously worked hard; but the style is somewhat irritating in places. There are ambiguous statements, while definitions and hypotheses of theorems are not always clearly stated. But the book should prove interesting to any serious mathematician and also to mathematical physicists. It can be used as a textbook or as a hunting-ground for new problems for research. Finally, there is a bibliography giving references to over 600 papers, more than fifty of which are by the author. It is noticeable that less than a dozen of these are British, though some are American.

L. S. BOSANQUET.

PHYSICS

Introduction to Physical Optics. By JOHN KELLOCK ROBERTSON, F.R.S.C., Professor of Physics, Queen's University, Kingston, Canada. [Pp. vii + 422, with 6 plates and 223 figures in the text.] (London: Chapman & Hall, Ltd., 1930. Price 20s. net.)

HERE is quite the best account of the elements of physical optics which the reviewer has yet seen. The mode of treatment suggests the standard of a pass degree, but the inclusion of several chapters devoted to "modern" work adds much to the interest and value of the book.

The first six chapters contain an account of reflection, refraction, and the behaviour of mirrors, lenses, etc., based on wave methods. The usual formulæ (*e.g.* for "thick" lenses) are deduced, and their application made clear by numerical examples. In Chapter VII is an account of the production, detection, and character of spectra, including an introduction to spectral series and an explanation of the usual P.D.S. notation. This is followed by two chapters on interference. These, particularly Chapter XI, which deals with Hooke's fringes, Haidinger's fringes, and the interferometers, are of outstanding merit. A treatment of diffraction and polarisation, notable for the simplicity of the mathematics, takes us to the end of Chapter XV, and the concluding chapters deal with the electromagnetic theory, the quantum theory, the reconciliation of the two theories by wave mechanics, and the problem of the ether. There are numerous problems (with answers) at the ends of the chapters and an index.

Adverse criticism of the author's work is possible on only a very few minor points. For example, the proof of the relation between the magnifying power of a telescope and the diameter of the eye ring (pp. 104–5) is a little involved, and does not bring out clearly the fact that the eye ring is the image of the object glass formed by the eye lens. The statement (p. 118) that the expression $D = (n - 1)A$ for prisms of small angle is restricted to minimum deviation is hardly correct. The deduction of the expression for the resolving power of a grating (p. 248) might be simplified, and the reference to "Lord Rayleigh" on p. 255 is obscure, in that it is not clear to which of the illustrious bearers of that title the author refers. Finally, Fig. 196, which purports to show the arrangement of the apparatus for the production of fringes in "convergent" polarised light, is quite inadequate.

The circulation of the book in this country is likely to be restricted by its relatively high price. The copyright is held by the Van Nostrand Company of the United States, and the book was printed in that country. The printing is adequate but not irreproachable, the lettering on the diagrams is too small, while some of the diagrams are unnecessarily large (*e.g.* Fig. 203). The binding is good, and the plates contain photographs which will quite definitely help the student to understand the text. We should like to see a new edition, containing additional matter of the kind generally studied under the heading Geometrical Optics, at two-thirds the present price.

D. O. W.

The Measurement of Hydrogen-ion Concentration. By JULIUS GRANT, Ph.D., M.Sc. (Lond.), A.I.C. [Pp. viii + 159, with 24 figures.] (London: Longmans, Green & Co., 1930. Price 9*s.* net.)

THIS book should be very useful to the worker in an industrial laboratory, for whom it is primarily written. The theoretical part is intentionally short, as no attempt is made to supplant the standard works on the subject. Details of practical methods are clearly and fully given, making the book a valuable laboratory manual.

P. TOOKEY KERRIDGE.

X-ray Crystallography. By R. W. JAMES, M.A., B.Sc. Methuen's Monographs on Physical Subjects. [Pp. vii + 88, with 29 diagrams.] (London: Methuen & Co., 1930. Price 2*s.* 6*d.* net.)

IT is often said that good stuff lies in small compass; at any rate, the proverb is true enough when applied to the present neat little volume of a neat little series. It should not take the intelligent reader more than a few hours to digest the scientific contents of *X-ray Crystallography*, but it will be time well spent, and he or she will have acquired a brief but well-illuminated

glimpse of one of the most fruitful and fascinating subjects of our time. Here is a simple opportunity for getting the gist of the rather overwhelming literature of X-ray crystal-analysis, and the reader may rest assured that he (or she) is sitting at the feet of an expert on the subject—especially in the field of the *intensity* of X-ray reflexion by crystals. We wish only that Mr. James had not chosen the doubtful title of *X-ray Crystallography*.

W. T. ASTBURY.

Structure of Line Spectra. By L. PAULING and S. GOUDSMIT. (International Series in Physics.) [Pp. x + 263.] (London: McGraw-Hill Publishing Co., 1930. Price 17*s.* 6*d.* net.)

THIS book presents an exposition of the atom in terms of a vector model and a theoretical interpretation of line spectra. It is not a book dealing with specific spectra, but with the general principles underlying all spectra. The authors state that they have kept in mind readers entirely unacquainted with the subject. With the standard of this book in mind we are inclined to the opinion that they are certainly very complimentary to their readers. The book is not an easy one to read, for the very good reason, we think, that the subject itself is far from easy. Theoretical developments have taken place so rapidly in the last few years that the experimental spectroscopist has frequently felt bewildered at the prospect. This book brings the theoretical position of the present time before us within a reasonable compass (some 258 pages). Various chapters deal with atomic theories and models, and in particular with the hydrogen atom. The interpretation of the latter in terms of the quantum mechanics is given. Three chapters are devoted to the term values, the fine structure, and the vector model of one-valence-electron atoms. Following chapters treat the two-electron and the many-electron atom. There are also chapters on intensity and polarisation of lines, on the Pauli exclusion principle, on X-ray spectra, on hyperfine structure and nuclear moment, and sundry magnetic phenomena.

We thoroughly recommend the book to spectroscopists as being a good and comprehensive account of the theoretical position by two competent authorities. It is as far as possible up-to-date and, moreover, gives many detailed references to original papers for those who have specialised interests. For its size it is probably the best book at present written on the subject, and it will doubtless meet with well-deserved success.

We may add that the production of this book by the McGraw-Hill Publishing Co., Ltd., maintains their tradition of high quality.

R. C. JOHNSON.

Adsorption und Kapillarkondensation. By ERICH HÜCKEL. [Pp. vii + 308, with 34 figures.] (Leipzig: Akademische Verlagsgesellschaft M.B.H., 1928. Price M.18.)

THE volume on adsorption contributed by Dr. Hückel to the series *Kolloidforschung in Einzeldarstellungen*, edited by Prof. Zsigmondy, is one of the most important volumes in a notable series. In the compass of three hundred pages the author discusses very fully the adsorption of gases and vapours at solid surfaces, theories of surface tension, and the "Kapillar-kondensation" of vapours in porous bodies. Thermodynamic theory is consistently applied, and, as might be expected, one section of the book deals in some detail with the electrical interpretation of adsorption forces.

Compact in form, easy to handle, and containing a full bibliography, the volume should be on the shelves of every physical chemist interested in this branch of his subject.

It is with mild surprise that one notes, on page 229, a diagram exhibiting the march with temperature of free and of total surface energy, in which the

graph for the last-mentioned quantity exhibits the doubtful maximum indicated by Whittaker. If the variation of free energy (γ) with absolute temperature is represented by

$$\gamma = A\left(\frac{\theta_c - \theta}{\theta_c}\right)^{\omega}$$

and the relation between total energy (λ) and free energy is

$$\gamma = \lambda + \theta\frac{\delta}{\delta\theta}$$

then, as A. W. Porter pointed out some time ago, no such maximum exists.

A. F.

An Introduction to Advanced Heat. By Ivor B. Hart, O.B.E., Ph.D., B.Sc. [Pp. vii + 336.] (London: G. Bell & Sons, 1928. Price 7*s*. net.)

If, in the matter of authorship there is one truth which is more important than another, it is that a would-be author must set his mind to *grapple* with his subject, and, in all those aspects with which he proposes to deal, must make the subject peculiarly his own. It is so fatally easy to compile, so difficult to think.

The dangers attendant on the most industrious compilation are signally illustrated in the volume before us. The scope of the book is that of the syllabus for a good pass degree, and the author has been at considerable pains to introduce adequate discussions of new experimental methods in place of those antiquated methods of measurement which have appeared for so many generations in orthodox textbooks.

No subject has suffered more in the general school curriculum from overloading with out-of-date descriptions than has the subject of heat, and the author is to be congratulated on a well-planned and comprehensive effort to overcome this defect, and to give adequate accounts of modern methods for the measurement of temperature, expansion, specific and latent heats, and thermal conductivities.

It is when we come to examine the volume in detail that we find such traces of haste in compilation and lack of constructive thought as make the book an unreliable guide. Thus, the method of calibration of a mercury-in-glass thermometer given on page 2 is out of date. On page 9 the author gives a diagram of a simple Wheatstone's bridge, and ingenuously remarks that "we can always measure the resistance S of anything if we know P, Q, and R." He then (p. 10) proceeds to describe the platinum thermometer and its method of calibration, giving the fixed points for standardisation as 0°, 100°, and − 273° C. (and actually writes of taking a third reading *at* this temperature !). It is difficult to think that the author has any adequate conception of what is meant by platinum thermometry. In the next section (p. 11), dealing with the thermo-couple, the author describes a circuit consisting of junctions A and B and a galvanometer, and continues: "Suppose we heat one junction so that the temperature at A is greater than the temperature at B. We find that the galvanometer registers a 'kick.'" Comment is superfluous.

The author's views concerning modern atomic theory are fantastic and misleading in the extreme. He tells us (p. 49) that "the atom in its normal state consists of a comparatively large nucleus to which is attached a positive charge of electricity, surrounded by a number of more minute particles, to each of which is attached a negative charge of electricity. The nucleus is spoken of as the proton. . . ." The scholar who reads this as his first introduction to modern atomistics will find himself with much to unlearn.

On page 56, à propos of elementary kinetic theory, we read that, in the equation $PV = \frac{1}{3} Mu^2$, "u is the square root of the mean square velocity and not the square of the mean velocity. The two are very different." Naturally! On page 94 J is given in *ergs*.

The discussion of reversibility (p. 110 *seq.*) is not at all adequate, and the inadequacy leads to another serious error, in that the terms *adiabatic* and *isentropic* are taken as equivalent (p. 127) and a definition of entropy is formed on this equivalence.

The calculation for the determination of the ratio C_P/C_V from the results of Clément and Desormes' experiment (p. 102) is very much facilitated by a simple process of approximation which the author does not consider.

The involved calculation of the efficiency of a reversible engine (p. 117), whose working substance is a perfect gas, may be avoided by the use of the equation $Tv^{\gamma-1} =$ const., deduced on page 100.

The author quotes (p. 119), without any critical comment, the incomplete statement of the second law made by Lord Kelvin. It would have been very helpful to the student had he discussed this statement critically, and compared it with the very careful enunciation of the second law due to Planck.

The author is not always happy in the sign of his differential coefficients, as is instanced in his account of the conduction of heat along a bar (p. 274), where he makes the heat flow up the temperature gradient.

It is specially unfortunate that the section on thermodynamics should be disfigured by these errors, as the author has made a praiseworthy effort to show the student that the Carnot cycle is but one among many important thermodynamical cycles.

Dr. Hart has cast his net widely, and has brought to light the work of a large number of obscure physicists. Bridgeman, Chappius, Clapyron, Depretz, Henry V, Van der Waal's, and Van t'Hoff are names hitherto unknown to the reviewer.

With drastic revision the book may be made to serve a useful purpose. As it stands, it must be used with very great caution.

A. F.

The Mechanism of Nature. By E. N. DA C. ANDRADE, D.Sc., Ph.D. [Pp. xi + 170.] (London: G. Bell & Sons, 1930. Price 6s. net.)

THE happy times when the fundamental truths of science could be expounded in terms of everyday mechanical notions are gone beyond recall, and he who would read the story to-day with any profit to himself must be something of a philosopher, whether his technical knowledge be small or great.

It is questionable whether there is, among the people at large, as much enthusiasm for scientific knowledge, popularly explained, as existed in the seventies, when, week after week, Manchester folk queued up to hear lectures by such masters of their art as Tyndall or Roscoe, and filled the Free Trade Hall to overflowing. But the demand does exist, and Prof. Andrade has set himself to meet it in the volume under review.

The book is far from being elementary—no book which deals with fundamental physical truths in any adequate manner can to-day be described as elementary—but Prof. Andrade's skill in exposition, innate scholarliness of mind, and aptness of illustration, analogy and comment have combined to make the path as smooth as may be, and to produce a book that is almost perfect in its kind.

Here the reader will find the latest—or almost the latest—views on Heat and Energy, Light and Radiations, the Quantum theory and the Atom expounded with admirable clearness, and interspersed with fascinating thumbnail sketches of some of the newest applications of physical science to industrial needs.

It is almost *too* easy. The author has indeed served up to us a most refined repast, and, while the reviewer echoes his hope that readers may, with whetted appetite, seek for more substantial fare, he fears that they will find such provand but dull and heavy after the delicate flavours of Prof. Andrade's—

" Jellies soother than the creamy curd,
And lucent syrops tinct with cinnamon."

A. F.

Matter and Radiation, with particular reference to the detection and of the Infra-red Rays. By John Buckingham, M.A. [Pp. xii + 144.] (Oxford University Press, 1930. Price 7*s*. 6*d*. net.)

The appearance of this volume is very deceptive. Its binding and printing, and the badge it bears, lead one to expect a treatise based on expert knowledge, an intellectual feast. Unfortunately, a speedy disillusion awaits the reader, for it is really only another popular book on the structure of the atom in general and on the infra-red radiations in particular, written for those readers who have no previous knowledge of these subjects. It is not a particularly charming book to read, for the author lacks that deftness with simple analogy and that lightness of touch which are essential to make such a book appeal. Although the book is supposed to have special reference to the infra-red radiations, their treatment is confined almost entirely to a description of the ordinary methods of detection and to their uses in secret signalling and the distant control of apparatus.

L. F. B.

CHEMISTRY

A Comprehensive Treatise on Inorganic and Theoretical Chemistry. By J. W. Mellor, D.Sc., F.R.S. Vol. X. S, Se. [Pp. x + 958, with 217 diagrams.] (London: Longmans, Green & Co., 1930. Price 63*s*. net.)

More than ever sulphuric acid remains the life-blood of chemical industry, and thus of most other industries, and for this reason alone Vol. X of "Mellor" is at least as important as any of its predecessors, dealing as it does with the compounds of sulphur and selenium.

Not only is sulphur of immense value in peace and war in the form of sulphuric acid, but in its elementary state it is used to the extent of over 1,000,000 tons a year for the prevention of blight on vines and hops, and Vol. X is a mine of information on all that relates to sulphur, the oxides, oxy-acids, halides of sulphur, and the corresponding though more limited derivatives of selenium.

In accordance with his strict self-imposed terms of reference, Prof. Mellor has devoted very little space indeed to actual manufacturing processes, and one cannot but feel that an exception should have been made in the case of sulphuric acid, as in this case science and industry are so closely interwoven. One notable omission also is revealed by the absence of any reference to the important Bayer process for the manufacture of sulphur trioxide by heating together gypsum and clay, cement being formed as a by-product; at all events, there appears to be no mention of it in the index or the text.

The question of the structure of the sulphur molecule itself is gone into very fully, and the copious references indicate the continued interest taken in this element; the more complex oxy-acids of sulphur are also dealt with in great detail, and a valuable summary of the whole situation is given.

Whilst the chemistry of selenium is necessarily less important in terms of pounds, shillings, and pence—not to mention dollars—than that of sulphur, it possesses several valuable properties which render its study

exceedingly interesting; one has only to recall its photo-electric sensitivity as a case in point; this is fully discussed with the aid of diagrams. Again, as a catalyst, selenium and its compounds possess many remarkable and unexplained properties; for instance, in the preparation of certain important dyes of the anthraquinone series it was found quite by chance that the necessary reactions will only occur in the presence of a minute trace of selenious oxide, though the reason for this is still not clear.

Once again chemists must express their indebtedness to Prof. Mellor for his endless patience and industry, and must place yet another volume of "Mellor" on their book-shelves, where it will, without doubt, be often and gratefully consulted.
F. A. MASON.

Paint, Powder, and Patches: A Handbook of Make-up for Stage and Carnival. By H. STANLEY REDGROVE, B.Sc., and GILBERT A. FOAN. [Pp. xii + 170, illustrated.] (London: William Heinemann, 1930. Price 7s. 6d. net.)

SCIENCE seems indeed to be losing all sense of veneration when we find the devotees of Æsculapius and Hermes invading the very shrines of Bacchus and Thespis, making public the inner mysteries of the priesthood, and daring even to suggest improvements in the rites and ceremonies of Maquillage!

Our authors, however, approach the subject with all due respect, and, indeed, the seriousness and care with which they touch upon all aspects of make-up—whether for theatrical purposes or for the purpose of assisting Nature where her hand has momentarily failed—and the figures they quote for the annual consumption of cosmetics, make one realise that this branch of applied art is indeed one of the major industries of the world! We are told, for instance, that in the United States alone the fair sex spend daily over £1,000,000 on cosmetics of one sort and another, so the world figures must be terrific. It is an interesting speculation whether the consumption of lip-salve is inversely proportional to the intake of alcohol, but on this point the oracle is silent. Perhaps the great Matriarchal States of America, seeking fresh fields of endeavour, may pass a Nineteenth Amendment to prohibit the use of all cosmetics, and we may yet be thrilled by tales of the feminine counterpart of Prohibition with mysterious midnight rouge-running exploits, secret face-powder factories in lonely marshes, and "look-easies" where the more depraved may powder their noses ($10.00), tint their cheeks ($20.00), or incarnadine their lips ($100.00)!

The first part of this book deals with the Preparation of Theatrical Cosmetics, and covers such topics as colouring matters, dry and liquid make-up requisites, fats, waxes, grease-paints and powders, whilst the second part deals with the Practical Art of Making-up, and will be useful for actors, movie-performers, and amateur detectives; there is also a Bibliography and a good Index.

Whilst *Paint, Powder, and Patches* can perhaps hardly be regarded as a scientific textbook, the information collected together within its covers may well be of value to chemists called on to advise or warn upon the subject of cosmetics, for careful attention is paid to the possible dangers of poisoning involved in the use of some products, and the authors may feel well satisfied at having produced a concise and readable monograph on the subject.

F. A. MASON.

The Art and Principles of Chemistry. By HENRY E. ARMSTRONG. [Pp. xxxi + 276.] (London: Ernest Benn, 1927. Price 15s. net.)

BETWEEN the boards of a single volume of moderate size, Prof. Armstrong has collected a number of essays of very different scope and date. Two threads, running through the whole series, serve to give it unity—Prof.

Armstrong's devotions to Chemistry as an Art, if not as a Religion, and his very emphatically expressed views concerning electrolytic dissociation.

The first, and by far the longest essay in the book, gives its title to the volume. It is a reprint of an article which appeared in the thirteenth edition of the *Encyclopædia Britannica*, and in which the author endeavours to present his subject in such a manner as to bring it "within the compass of the general reader—the real users of the work—and so that it might serve to interest the junior student, the senior schoolboy, in fact, even the teacher." The nature of the remaining essays—the Dream of fair Hydrone, the Thirst of salted Water, the Corrosion of Iron and of other metals, Rhapsodies culled from the Thionic Epos, the Origin of Osmotic Effects, and Electrolytic Conduction, is indicated sufficiently by their titles, and by our knowledge of Prof. Armstrong.

The present reviewer, combining the wisdom of the sarpint with the innocence of the dove, does not propose to embark upon the task of discussing, critically or historically, Prof. Armstrong's views on the subject of electrolysis. They are well known. His reasons for his views are not so well known; and it is a matter for congratulation that his admirers and his critics—and a representative of the two classes is more than seldom to be found under one hat—may turn to this volume for a reasoned statement of his views expressed in that witty and forcible English which we are accustomed to look for over the initials H. E. A.

His essays are thoroughly stimulating, and never more vigorous than when, ionically speaking, he is most wrongheaded. It certainly cannot be said of Prof. Armstrong that his—

> ". . . trenchant blade, Toledo trusty,
> For want of fighting was grown rusty,
> And ate into itself for lack
> Of some body to hew or hack."

Long may he continue to wield it with undiminished vigour.

A. F.

Experimental Physical Chemistry. By DANIELS, MATHEWS and WILLIAMS. [Pp. xvi + 475, with 132 text-figures.] (London: McGraw-Hill Publishing Co. Price 17*s*. 6*d*.

IT is only necessary to glance at a book such as this to be struck by the greatness in the change that has taken place during the last fifteen years in the teaching of physical chemistry. Not only has there been an enormous increase in the scope of the subject-matter, but the method of presentation has been radically changed. Formerly, physical chemistry was merely a small addition to chemistry, in which physical measurements were used to supplement chemical methods. Now it is rapidly becoming the centre of physical science. The situation is somewhat perplexing, because physical chemistry, besides being a very important subject, is also a very large one. There is therefore a serious danger of the amount of material to be taught being so large that it is practically impossible for the student to absorb it fast enough. Where such a danger exists, there is no doubt that a well-designed course of practical work is of the utmost value. The course outlined in the first part of this book is undoubtedly well thought out, and those who use the book will have the benefit of the considerable experience of the writers. Besides a student's course the book contains, as a second part, a synopsis of experimental methods in the various branches of the subject, designed as much for reference afterwards as for use during the student course. A third part deals with "miscellaneous" operations—calibration, thermostats, thermometers, and glass blowing.

R. K. SCHOFIELD.

METALLURGY

The Scientific Fundamentals of Gravity Concentration. By JOSEF FINKEY, E.M. Translated into English by C. O. ANDERSON and M. H. GRIFFITTS. *Bulletin of the Missouri School of Mines*, Technical Series, November 1927. [Pp. 296, with 44 figures and 31 tables.] (Rolla, Missouri: School of Mines and Metallurgy, 1930. Price $1.00 net.)

THIS is a translation of Prof. Josef Finkey's treatise which gives an account of his research in connection with gravity concentration processes. The original manuscript, which was in Hungarian, was translated into German by Mr. Johann Pocsubay and published in 1924. The present English version, the translators of which are to be commended on the excellence of their work, was presumably ready for publication in 1929. It represents co-operative work between the United States Bureau of Mines and the Missouri School of Mines and Metallurgy of the University of Missouri.

The treatise is an examination of the physical principles underlying the action of particles in water in classifying, jigging, and table concentration. Naturally the subject is dealt with mathematically Prof. Finkey obtains many interesting results by the use of hyperbolic functions. Some of the equations obtained are complex, and in these cases the author has derived a simplified form suitable for practical purposes. In addition to the mathematical deductions there are observations on gravity concentration practice. Following an introduction the volume is divided into four chapters, namely: (1) The Principles of Mechanics of Gravity Concentration; (2) Preparation for Gravity Concentration; (3) Jigging; (4) Concentration on Tables.

In spite of the fact that the flotation process has been extensively applied for many minerals in recent years, gravity concentration is still of importance in a large number of cases of dressing. Excepting Rittinger's well-known textbook, published in 1867, most books on ore dressing are chiefly devoted to a description of machines and appliances, so that this present treatise should make an appeal to all those interested in gravity concentrations, and who are concerned with the design and improvement of machines used in this connection, and should certainly prove a means of stimulating research in this branch of the subject.

E. O. COURTMAN.

BOTANY

Practical Botany. By A. H. REGINALD BULLER, F.R.S., B.Sc. (Lond.), D.Sc. (Birm.), Ph.D. (Leip.), LL.D. (Manitoba), LL.D. (Saskatchewan), F.R.S.C. [Pp. vi + 275, with 2 diagrams in the text.] (London: Longmans, Green & Co., 1929. Price 6*s*.)

THE syllabus covered by this book is similar to that of an intermediate Botany course; the arrangement, however, is markedly different from the usual, as there are two lines of development instead of one, so that, for example, Protococcus is studied in the same practical manner as the Epidermis of a Monocotyledonous leaf, and Bacteria with the internal structure of the stem of a Dicotyledon. While this method undoubtedly gives the variety of interest it was intended to do, the discontinuity, in the end, must be disconcerting to the student. The instructions and descriptions are minute and exact—nothing seems to have been forgotten or omitted. This method leaves little or no initiative to the student and gives him no opportunity of learning from his mistakes, while it must inculcate great powers of carefulness and tidiness. It would be invaluable in large classes with comparatively few demonstrators, and also prove a boon to the demonstrator. The description of the microscope and its use is especially admirable, and the inclusion of derivations to many of the terms used is also another excellent feature of this book.

E. M. C.

The Useful and Ornamental Plants of Trinidad and Tobago. By W. G. FREEMAN, B.Sc., A.R.C.S., F.L.S., and R. O. WILLIAMS. [Pp. 192.] (Trinidad Government Printing Office, Port of Spain, 1928.

A DICTIONARY of the wild plants, native or introduced, of these islands, giving an account of their economic or ornamental qualities. The memoir, which is in its second revised edition, gives information on cultural methods suited to local conditions; English and local names are included. After the dictionary, lists are given of useful plants for timber, fruits, beverages, etc., and after this a systematic index and an alphabetical index to the families. A useful book in the West Indies and in the Tropics generally; in other parts of the world it will be found to supplement such works as: *The Treasury of Botany* and *Müller's Extra Tropical Plants.*

E. M. C.

Plant Biology. An Outline of the Principles underlying Plant Activity and Structure. By H. GODWIN, M.A., Ph.D. [Pp. ix + 265, with 67 figures.] (Cambridge, at the University Press, 1930. Price 8*s.* 6*d.*)

THE proper material for inclusion in the first M.B. course is often matter for discussion. This new book by Dr. Godwin solves the question in a modern way by laying stress on "the physiological point of view and the physico-chemical background of plant life." In accordance with this point of view, full attention is paid to the fungi and bacteria, but comparatively little to the higher plant, the method of reproduction of which has been omitted, and so short a description—a page in length—of secondary thickening provided that it is difficult to believe that this alone will give the student an idea of this process of which he finds it so difficult to form a mental picture. The structure of Fucus, on the other hand, receives detailed consideration, with an account of both the primary and the secondary growth.

The book is an extremely interesting one to read and packed full of information, but a strong doubt may be expressed as to whether the ordinary medical student, at this stage of his career, will have sufficient knowledge of physics, and especially of chemistry, to take full advantage of the chapters on Crystalloids and Colloids and on Organic Substances, or with the present arrangement of the book to follow the description of the metabolism of the plant beginning on page 48, and the very inportant chapter on Photosynthesis beginning on page 77, when the description of the structure of the leaf, including stomatal movement, is left to the last chapter beginning on page 241.

The book is of a good size, and very well printed; the illustrations are as a rule well chosen; a few, however, notably Fig. 34, page 171, seem to have been drawn in a manner not very suitable for reproduction on the slightly rough surface of the paper.

E. M. C.

Pathologie der Pflanzenzelle. Teil I. Pathologie des Protoplasmas. By ERNST KÜSTER, Prof. der Botanik an der Universität Giessen. [Pp. viii + 200, with 36 illustrations.] (Berlin: Gebrüder Borntraeger, 1929. Price M.15.)

As pointed out in the preface, a comprehensive report on the pathology of protoplasm involves a review of about eighty years' work; even though confined to the pathology of the protoplasm of the plant cell, the mass of literature to be dealt with is so great as to make it well-nigh impossible to deal with it in detail; as a consequence the author admits that he has produced a short introduction to the subject, rather than a monograph. The book is divided

into two chapters, dealing respectively with changes in form and changes in structure. Under the former heading are included plasmolysis, separation of the protoplast into detached fragments by plasmolytic contraction, by microdissection or by electrical means, and a description of the behaviour of protoplasm under cataphoresis and in the centrifuge. The second chapter deals with changes from the fluid state under the influence of light, temperature, and other conditions, vacuolation, and swelling. Many of the subjects are treated in a very cursory manner, which produces the effect of a somewhat disjointed narrative, this is perhaps inevitable in a work of this kind, but is amply compensated by the very valuable list of references covering twenty-five pages at the end of the book for which all interested in the subject will be duly grateful to the author.

P. H.

The Plant in Relation to Water: A Study of the Physiological Basis of Drought-resistance. By N. A. MAXIMOV, Professor in the Institute of Applied Botany, Leningrad. Authorised English translation, edited, with notes, by R. A. YAPP, Mason Professor of Botany in the University of Birmingham. [Pp. 451, with 46 figures.] (London: George Allen & Unwin, Ltd., Museum Street. Price 21*s*.)

THIS important work is divided into three main portions, the first dealing with water-absorption, the next with water-loss, and the last with water-balance and drought-resistance. These summaries of modern work do not claim to be exhaustive, but are critical, and are helpful in obtaining a general picture of the processes. It is the last part of the work that is of especial interest, for it brings together a very large amount of recent American and Russian work, the latter not easily accessible. It puts forward Prof. Maximov's view that many plants inhabiting dry situations are not drought-resisting but drought-escaping (*e.g.* ephemerals), and that the others are not found in such districts because they grow best there but because they can resist drought better than other plants. As regards the nature of drought-resistance, Prof. Maximov is of the opinion that structural modifications are of secondary importance; the really important point being the "capacity to endure without injury an intense loss of water."

The work of translation and editing has been admirably done. A bibliography of more recent work, an index, and special prefaces to the edition are provided, while Prof. F. E. Weiss has contributed a short biographical note.

E. M. C.

Further Illustrations of British Plants. By ROGER W. BUTCHER, B.Sc., F.L.S. Drawings by FLORENCE E. STRUDWICK, Nat. Sci. Tripos (Cantab.), M.A. (Dublin). [Pp. viii + 476.] (Ashford (Kent): L. Reeve & Co., Ltd., Lloyds Bank Buildings, Bank Street. Price 12*s*. net.)

IT is safe to say that there is no book better beloved (only a term of affection such as this is adequate) by British field-botanists than Bentham and Hooker's *Handbook*. Used in conjunction with Fitch's *Illustrations*, it has proved of inestimable service; and, in spite of certain defects, it possesses virtues which belong to no other work on British flowering plants and ferns which has ever been published.

In the conflict between "lumpers" and "splitters," scientific opinion in recent years has tended more and more to the side of the latter. Bentham, of course, was the lumper of lumpers. To a very large extent the consequent defects in his book were made good by Hooker's parenthetical additions. Additions have also been made in various editions to Fitch's *Illustrations*; but the book still remains very incomplete.

British field-botanists have dreamt of the day when a companion volume might be published, giving descriptions, and especially illustrations, of those species which Bentham relegated to the rank of mere varieties or even forms. Moreover, the flora of the British Isles has been enlarged in two other ways since Bentham's day. Certain alien plants, such, for example, as *Matricaria suaveolens*, *Sisymbrium altissimum*, and *Azolla filiculoides*, have found permanent homes in our country and become completely naturalised. Moreover, species new to Britain have been discovered, such as the well-known *Selinum carvifolia* of Cambridgeshire and Lincolnshire.

Well, here is the book of the British field-botanists' dreams, and a most satisfactory book it is, containing short, but adequate, descriptions and excellent drawings of 485 species or varieties belonging to one or other of the three categories mentioned above. Together with Bentham and Hooker's *Handbook* and Fitch's *Illustrations*, it constitutes a pretty complete and up-to-date guide to the flowers and ferns of the British Isles, if exception is made of those very variable and complex genera, *Rubus*, *Hieracium*, *Euphrasia*, and *Salix*, whose omission from the present work (save for three species of the last) has been absolutely necessitated by considerations of space.

The illustrations are much larger in size than those of Fitch's work, the majority of them occupying about three-quarters of a page each, and being, in many cases, half natural size. A 1-in. and 1-cm. scale is included on each page. Dissections considered useful for identification purposes are shown on each plate, the magnification being stated. Three new species of *Thalictrum* are described and figured.

There will, no doubt, be some divergences of opinion concerning the author's choice of what plants to include and what to omit; but, in general, this appears to have been a wise one, based on sound considerations. It is only possible to describe the book as a long-wanted one, an excellent one, and one which every British field-botanist will wish to possess.

H. S. Redgrove.

Hydrogen-ion Concentration in Plant Cells and Tissues. By James Small, D.Sc., Ph.C., F.L.S., M.R.I.A., F.R.S.E., Professor of Botany in the Queen's University of Belfast. [Pp. xii + 421, with 28 illustrations.] (Berlin: Gebrüder Borntraeger, 1929. Price 30 M.)

This volume, the second in the series of Protoplasma-Monographien, concerns itself in a very detailed manner with a critical account of the various methods of determining hydrogen-ion concentrations in plant cells and tissues, summarising the already enormous amount of facts that have been observed and discussing the meaning of these observations. The first chapter deals with the problems, and the last with problems restated, and it would seem that so far few important conclusions of a general nature have emerged. The work is provided with three appendices, a bibliography, and indices to plants and authors.

E. M. C.

The Spore Ornamentation of the Russulas. By Richard Crawshay, with Preface by Frederic Bataille, Ancien Vice-Président de la Société Mycologique de France. [Pp. 188, with 46 plates, 21 illustrations in the text, and 2 colour charts.] (London: Baillière, Tindall & Cox, 1930. Price 12*s.* 6*d.*)

This book is concerned with much more than we could gather from its title; some seventy pages are taken up with a description of the details of methods of examination of the spores under the high powers of the microscope and with dark-ground illumination, photo-micrography, etc.; a chapter

gives us a dichotomous key to the species of Russula, another chapter critical notes of some of the species. The drawings of the spores, showing their shape, characteristic protuberances, and sculpturing, are accompanied by a description giving the shape, colour, and average dimensions of the spore of each species. The colour changes which occur in some red Russulas during decline should have been figured from the Russulas themselves, and not from a colour plate of similar colourings in Primulas.

E. M. C.

ZOOLOGY

Heredity in Live Stock. By Christian Wriedt. [Pp. xi + 179, with 70 figures.] (London: Macmillan & Co., 1930. Price 7*s*. 6*d*. net.)

This book is intended mainly for the breeder of animal stock, and is of considerable use, for, as is pointed out in the introduction, the late Christian Wriedt was one who not only understood the problems confronting the breeder, but was also familiar with the scientific principles underlying them. It is also of use to the laboratory zoologist, for it places the practical problems before him, and shows how some of them have been solved. The book aims to put before the breeder the solid knowledge that has been won, and from this point of view perhaps too much space was given to the inheritance of milk production and lethal factors, problems still not satisfactorily solved. It is also a pity that it should include statements like the one to the effect that a double dose of the factor for frizzled feathers in fowls is lethal, for, when the breeder finds them to be untrue, he will naturally be inclined to look askance at some of the other statements that are more firmly established. Apart from these relatively small points the book has much to recommend it. The illustrations are well chosen and well reproduced. The text is concise, and makes interesting reading. It should be acceptable to a wide circle of readers.

C. H. O'D.

A Report on the Fishing Survey of Lakes Albert and Kioga. By E. B. Worthington, B.A., F.L.S. [Pp. 136, with 2 maps and 24 other illustrations.] (London: Crown Agents for the Colonies, 1929. Price 10*s*.)

The Report consists of four parts dealing with Lake Albert, Lake Kioga, the Victoria Nile, and a General section. This is followed by four appendices on the History, Physiography, and the Fishes of the two lakes, and a fifth on the Ecology and General Natural History. The Report and the first appendix are of importance from the local point of view, forming as they do the first account of these areas. They are clearly and concisely written, and will furnish a good basis upon which future and more intensive work can be built. Also they provide data for the understanding of the last appendix, which from its nature will have a wider interest. The general ecology and the food chains of the fishes are treated in an instructive manner, and deal with types of waters and conditions that have been but little studied. The survey lasted from March to July 1928, and the author is to be congratulated upon the amount of information he gathered during that time, and also upon the celerity with which the information was made available to the public in such an interesting way.

C. H. O'D.

The Coconut Moth in Fiji: A History of its Control by means of Parasites. By J. D. Tothill, assisted by T. H. C. Taylor and R. W. Paine. [Pp. vi + 269, with 12 coloured and 22 black-and-white plates, 1 map, 121 text-figures.] (London: The Imperial Institute of Entomology, 41 Queen's Gate, 1930. Price 31*s*. 6*d*. net.)

Recently a great deal has been talked about biological control. In some quarters the public has been led away by the over-enthusiasm of some of

its advocates and, on finding that every pest cannot be controlled by biological means, has consequently been tending to lose faith in it. This account of the control of the coconut moth, a Zygænid, by its parasites should serve to establish the fact that biological control does work with conspicuous success on occasion.

The outstanding feature of the book is its make-up: being bound in buckram, with twelve beautifully coloured plates, the result of employing two artists, twenty-two black-and-white plates, and over one hundred text-figures. Is this the level to which the Imperial Institute of Entomology aspires for all its publications on successful entomological campaigns? Dr. Tothill and Messrs. Taylor and Paine are to be congratulated on the successful issue of their work, and also on having their studies published so magnificently.

After a short introduction and notes of previous work, there is an account of the campaign instituted in January 1925. Thirty-three pages are sufficient to describe it. There follows a detailed account, running to p. 177, of the Levuana moth; its relationship and phylogeny, origin and natural habitat, food plants, its life-history, and its control prior to 1926 are described in great detail. The Tachinid fly, *Ptychomyia remota*, which was used as the chief controlling agent of the moth, is then discussed in twenty-five pages. Other allied Zygænids, with notes on their natural control, next receive attention. The book ends with accounts of the Zygænid parasite B (why publish the book before all the parasites had been identified?), *Trichogrammatoidea nana*, an egg parasite of Javan Zygænids, and the predaceous Clerid beetle *Callimerus arcufer*, three other insects which were also used in the control of the Levuana moth. There is, in addition, an annotated list of the more important literature on *Levuana iridescens* published in Fiji, and a short index.

In such a publication, where expense has not been spared, it is a pity to find a lack of proper accentuation on the more striking scientific discoveries. For instance, if one refers to *Tropical Agriculture* (7, No. 8, August 1930, pp. 215–19), one sees an account of a remarkably interesting piece of information that was obtained during the coconut moth campaign. This article describes the gap in the sequence of generations in *Artona catoxantha* which apparently is due to the inter-relationship between the dominant parasite and its host. In the book under review it is difficult to find any mention of this discovery and deduction. Again, the extra larval instars of the *Levuana* moth and the omission of an instar by *Callimerus arcufer* (not specially stressed) are of outstanding genetical interest. An afterthought of reading the book is that there are too many unimportant, almost notebook, remarks.

The coloured plates are very good. It may be pointed out that Figs. 14 and 15 are not very clear, and that the "means" given on p. 82 do not necessarily give a true indication of the numbers of eggs. The reference to Fig. 41 under text-fig. 115 is an obvious slip. Further, all the photographs are not worth publishing. After weighing the information one cannot help admiring the band of workers for their painstaking endeavours and for amassing such a quantity of valuable information: they set out with an object and carried it through very successfully. Perhaps in writing up the work they might have done more sifting of facts and co-opted the Government Entomologist of Fiji as part editor, so as to have been enabled to make more use of his knowledge.

H. F. B.

Animal Breeding. Second Edition, rewritten and reset. By LAURENCE M. WINTERS. [Pp. viii + 389, with 103 illustrations.] (New York: John Wiley & Sons; London: Chapman & Hall, 1930. Price 18*s*. 6*d*. net.)

A WELL-ARRANGED outline of the scientific foundations and the practical methods used in animal breeding is presented. A brief introduction on the

origins of domestic animals is followed by a section on reproduction and lactation. The latter is an excellent attempt to apply the results of scientific experiment to the practice of animal breeding, and in parts the results of quite recent research work are included: so often there is delay between the advance of research work and its inclusion in the textbook.

The greater part of the book deals with heredity and the particular ways in which the science of genetics can be applied to farm animals. While plant breeding is usually carried out by the specialist only, almost every farmer has to practise animal breeding himself; this book therefore fills a requirement which the usual textbook of genetics, based mainly on plant work, does not supply. The details of the Mendelian inheritance of many fancy points (colour, horns, etc.), mutations, and lethals are given. Genetic studies in the past have dealt more with superficial characters than with economic ones, the latter being mainly due to multiple factors. With quantitative characters, where a large number of factors are involved, Galton's concept of percentage of blood still forms a good generalisation of the facts, more especially in domestic animals such as cattle, where the chromosome number is large. The book can be read with interest also by the geneticist, for it gives facts obtained from the practice of animal breeding which lead one to believe that there is still more to be learned about the mechanism of variation and inheritance. For example, it is stated that mutations are, at present at least, more a matter of academic interest than of practical importance: waiting for mutations to turn up is a most unlikely road to success in building up a superior strain. By keeping his herd in good "condition," the breeder develops his animals, and so paves the way for selection, *i.e.* the best producers are then discernible and are retained. In the chapter on selection the results obtained by this method of directing evolution are given: the world's record yield for a cow was 18,004 lb. of milk per year in 1880, while by 1920 this had been raised to 37,381 lb.—more than double the quantity. He concludes that the master breeder, like the superior animals he produces, is first born and then developed.

Interesting chapters on inbreeding, fertility, abortion, and development, as well as an extensive bibliography, are included.

J. H.

Materialism and Vitalism in Biology. By Sir PERCY CHALMERS MITCHELL, C.B.E., F.R.S., D.Sc., LL.D. [Pp. 30.] (Oxford: at the Clarendon Press, 1930. Price 2*s*. net.)

SINCE the methods and purpose of science and philosophy are widely divergent it is probable that the controversy on the nature of life will continue to flourish. But while the vitalists are at pains to bring forward new theories, the materialist has changed no whit in his belief that the patient application of physico-chemical methods of investigation is the only means by which to overcome the incompleteness of our present knowledge and to arrive eventually at a fuller understanding of the nature of life. Sir Chalmers Mitchell, in his Herbert Spencer Lecture, brings forward convincing evidence that a materialistic monism is nowadays more, not less, credible than when Spencer wrote his *First Principles*. Indeed, we are more sceptical than Spencer, he maintains, since the advance of knowledge has made matter a more probable not a less probable substratum for life. This is in part due to a changed conception of matter. Instead of regarding it as inert material acted upon by external forces, we have learned to regard it as a centre of active energy. If we compare the contributions of Profs. Eddington and Hogben to a discussion on this subject, it would seem that the methods of biology and of physics are tending to change places, since biologists of the advanced experimental school use mathematical analysis and microscopical examination of unrelated parts, while physics is faced

with the necessity of taking into account the "individuality" of single electrons and the different electrons to account for their behaviour, *i.e.* the difficulty of reconciling the quantum theory with mathematically predictable facts.

Sir Chalmers Mitchell reviews the achievements of physiology during the time that has elapsed since Spencer, and points out that even more striking results can be predicted in the immediate future, since structure, the formerly neglected factor in chemistry, is bringing into harmony the organic and inorganic processes, and is making unnecessary the assumption of a principle peculiar to life. Neglect of this factor has so far led to failure in the synthesis of living matter.

The vitalists' views of purpose come in for the usual amused criticism accorded to them by upholders of the mechanistic view. The author points out that so much of the structure of living organisms has been shown to be the result of mechanistic principles. This is even true of the human brain, that stumbling-block to the acceptance of mechanistic principles, since every function of the human brain and nervous system is determined by its past history, and can be understood only by reference to the course of structural evolution, admittedly a material factor. Vitalists see "purpose" in the amazing adaptations of animals to their environment, but when studied dispassionately it is possible to find as many structures and as varied phenomena which seem entirely purposeless. "Structure determining function that turns out to be useful, structure determining function for which no purpose has been found, structure leading to destruction ; all are striking aspects of animals and plants."

In entering the arena as a protagonist of mechanism it is desirable to meet the vitalist on his own ground and deal with the problems of consciousness and voluntary activity. The feeling that physico-chemical interpretations could never prove adequate to deal with the properties of living matter was greatly determined by the traditional distinction which physiologists have drawn in the past between reflex and voluntary activity. The epoch-making work of Pavlov's school on the conditions under which new reflex systems can be brought into being has broken down the sharp distinction between reflex or automatic and voluntary or "conscious" behaviour, and has been justly claimed as "more far-reaching in its philosophical consequences than the evolutionary speculations of the nineteenth century." It is unfortunate that Sir Chalmers Mitchell has made no reference to this work.

The address is a lucid, calm, and convincing statement of the case for materialism, and ably upholds the author's claim that "Materialism has proved itself the best working hypothesis of science."

M. ALLANSON.

An Introduction to Physical Anthropology. By E. P. STIBBE, F.R.C.S., Senior Demonstrator in Anatomy, University College, London. [Pp. vii + 199, with 42 illustrations and 1 map in colour.] (London: Edward Arnold & Co., 1930. Price 12*s.* 6*d.* net.)

THIS book is designed with the dual purpose of providing the amateur with an introduction to the principles and technique of physical anthropology, and the student with a textbook based on the syllabus for the B.A. and B.Sc. degrees of the University of London. The book, on account of its size, will be particularly welcome, both to those who do not intend to go more deeply into the subject, and also to those who have felt the need of a compact introduction to the more voluminous books dealing with Physical Anthropology. Moreover it is up to date—an important quality, in view of the recent discoveries of fossil primates. Indeed, the most recent of these, Sinanthropus,

is mentioned, although particulars were not available at the time when the book went to press.

The author, being a member of that distinguished school of anthropologists centred around Prof. Elliot Smith, is able to write with the weight of authority, and from that particular viewpoint which is associated with the school to which he belongs. The book, excluding the introduction, is divided in a strictly orthodox manner into three parts, dealing respectively with the Zoological, Palæontological, and Ethnological aspects of the subject. The importance of practical work both in the museum and on living subjects is stressed throughout.

Early in the first part of the book the reader's attention is centred on the part played in the evolution of man and the primates by the brain and the sense organs. The sections dealing with the skeletons of man and the anthropoid apes are printed side by side on left- and right-hand sides of the pages respectively, thus rendering comparison easy, but detracting somewhat from the appearance of the text on account of the blank spaces that are necessary to bring corresponding sections opposite. The part dealing with the fossil primates and man is introduced with an account of the general principles of palæontology. The description of the living races of mankind is followed by instructions for measuring skulls and calculating the appropriate indices.

The author is to be congratulated on having produced a very useful little book and one which should prove popular. It is well written, well illustrated, and with good paper, printing, and binding.

F. W. R. B.

The Fauna of British India, including Ceylon and Burma. Cestoda, Vol. I. By T. Southwell, D.Sc., Lecturer in Helminthology, School of Tropical Medicine, Liverpool. [Pp. xxxi + 391, with 221 illustrations and 1 map.] (London: Taylor & Francis, 1930. Price 22*s*. 6*d*. net.)

This fine monograph deals systematically with all those Cestodes which have been described from India up to the present time. It is arranged according to a new classification proposed by the author and based on that of Pintner. Our knowledge of Indian Cestodes is still very imperfect, but the descriptions given in this book are as complete as is possible at the present time. Many of the species described are also figured. The amount which the author himself has contributed to our knowledge is indicated by the fact that he has named no less than four genera and over thirty species. The introduction contains a useful account of the technique of collecting and preserving Cestodes. A full bibliography is given at the beginning of the book. It will be found invaluable as a reference book for identification, and will be much appreciated by Helminthologists.

F. W. R. B.

The Development of Sex in Vertebrates. By F. W. Rogers Brambell, B.A., Ph.D., D.Sc. [Pp. 261 + xvi, with 24 plates, 25 text-figures, and 22 tables.] (London: Sidgwick & Jackson, Ltd., 1930. Price 12*s*. net.)

The scope of this book is more accurately defined in the author's preface: " This book is concerned with one of the most fruitful fields of recent research: the embryology, physiology, and histology of the gonads. . . . The author has attempted to treat the subject as it interested him, believing this to be the most likely way to interest others." This method has both its advantages and its limitations, and it follows that the bibliography at the end is more in the nature of a list of references.

The subject of sex and reproduction are much in vogue in the biological world of to-day, and the problems they present are being attacked by a

large number of workers from many different points of view. It follows that a considerable and scattered literature is rapidly accumulating, and this cannot well be followed save by the specialist. Consequently two needs arise: in the first place, the general zoologist needs an opportunity for becoming acquainted with the trend of modern work, and, in the second, one beginning research in the same field needs a comprehensive review of the subject to serve as a taking-off place. In both respects the present volume can be recommended, and the author is to be congratulated upon the manner in which he has handled the various topics in its fourteen chapters.

It is stated on p. 136 that in the Marsupials and Placental Mammals there has been confusion with regard to the rôles played by the cells of the theca interna and those of the membrana granulosa. This is true of the Eutheria, but it was pointed out sixteen years ago that in a number of species of Marsupials the cells of these two layers are clearly distinguishable, and their rôles in the formation of the corpus luteum can also be followed. Recent work with more refined technique has confirmed this finding and extended it to the Monotremata and Eutheria.

Apart from this the book seems free of errors and misprints, and provides interesting and useful reading. The illustrations in particular call for notice, for they are all well chosen and well reproduced. Indeed, the whole "get up" of the book is distinctly superior to those usually sold at the same price. The author has creditably performed a piece of work that will be of use to a number of readers in different fields.

C. H. O'D.

Human Biology and Racial Welfare. Edited by EDMUND V. COWDRY. [Pp. xviii + 612, with numerous illustrations.] (London: H. K. Lewis & Co., Ltd., 1930. Price 28*s*. net.)

THIS book contains twenty-five separate chapters which have been contributed by twenty-seven different authors, and it has in addition an introduction by yet another person. It is therefore difficult to review, since it has neither continuity of subject nor similarity of treatment and style. All the authors with the exception of three come from the United States, and as a result, examples are chosen mainly from that country. The various chapters deal with a wide range of subjects, from a discussion of the possibility of other inhabited worlds, through the relations of cells one to another, the reaction of food, to the questions of the purposive improvement of the human race and the intentional shaping of human opinion. They are gathered into one volume because they all deal with some aspect of biology in its very widest sense, and have some more or less close bearing on the biology of man.

It is rightly pointed out in the introduction that we all tend to become specialists in a somewhat restricted field, and this remark applies in particular to students of medicine. But to be good physicians it is essential to be good biologists, and so the book aims at putting before such specialists other aspects of biology than those usually studied in the medical course, but yet relating to man. Secondly, it aims at reaching the thinking man who wishes to know more of himself and his relationship to the rest of the world. These two aims are admirably provided for in the succession of essays, each written by an expert. The treatment of the subjects is remarkable, in that while it can be understood by the layman it is still informative and not boring to the specialist. In this we feel the book has achieved distinction.

From its widespread interests it is not a book that can be read as a continuous whole, but on the other hand it can be picked up from time to time and chapters selected at random with the certainty of finding interesting reading. The printing and general "get up" are creditable to editors and printers alike.

C. H. O'D.

Studies on the Structure and Development of Vertebrates. By EDWIN S. GOODRICH, F.R.S., Linacre Professor of Zoology and Comparative Anatomy in the University of Oxford. [Pp. xxx + 837, with 754 illustrations.] (London: Macmillan & Co., Ltd., 1930. Price 36*s.*)

THIS book, as its title states, deals with the morphology of vertebrates, or rather "with certain subjects and problems of special interest and importance" to vertebrate morphologists. It is intended for "advanced students and others engaged in teaching and research." Eight chapters, comprising rather more than half the book, are devoted to the skeleton. The remaining six chapters deal with "The Visceral Clefts and Gills," "The Vascular System and Heart," "The Air-Bladder and Lungs," "Subdivisions of the Cœlom, and Diaphragm," "The Excretory Organs and Genital Ducts," and "The Peripheral Nervous System and Sense Organs." Thus the central nervous system, the alimentary system, the reproductive and endocrine organs, etc., are not dealt with at all. The reason for this is explained in the Preface, where the author states: "My original intention was to cover the whole range of vertebrate morphology; but the preparation of this volume has taken so many years, that I thought it better to publish what is ready than to wait for the remainder, which might possibly never be completed." We feel sure that Prof. Goodrich's wisdom in publishing the book in its present form will be admitted, and that zoologists in this country and elsewhere will be immensely grateful to him for not withholding the present volume on the grounds of incompleteness. At the same time, it is impossible to peruse the present volume without hoping that the author may find time in the future to complete his original scheme, and so render a still more conspicuous service to the science he serves.

The book as it stands covers many aspects of vertebrate morphology which are inadequately dealt with in existing textbooks. The reviewer knows of no other book in English which covers so thoroughly the more modern work on many aspects of the subject. That part which deals with the skeleton is rendered especially valuable on account of the author's own contribution to the subject, and the section on the skull provides a unique account of the work of Broom and of Watson. The difficulty of keeping abreast of modern views on the homology of the bones of the skull in the different classes of vertebrates has been felt acutely by many zoologists who are not specialists in this aspect of their subject. They will appreciate particularly the clear and logical account of the skull in Chapter VI.

The whole book is characterised by the care expended on statements of facts, and by the balanced judgment exercised in drawing conclusions therefrom. This is indicated by the author's statement in the Preface that he has, so far as possible, "tried to verify statements of fact by personal observations"; yet this is an amazing achievement in view of the scope of the book.

The book is well and copiously illustrated. Rather more than 300 of the total 754 illustrations are new, and have been drawn by the author himself or from his sketches. Their quality is uniformly high, and the reproduction is excellent. Prof. Goodrich has produced a scholarly and finished volume.

F. W. R. B.

Aquatic Mammals: Their Adaptations to Life in the Water. By A. BRAZIER HOWELL. [Pp. xii + 338, with a frontispiece and 53 text-figures.] (Baltimore: Charles C. Thomas; London: Ballière, Tindall & Cox, 1930. Price 22*s.* 6*d.* net.)

IT is widely recognised that whales and porpoises are modified for an aquatic existence, and they are often cited as forming with Ichthyosaurs and sharks a striking example of convergence. This is generally done in quite a superficial manner and not in any detail. The present work departs from this attitude,

and goes into various aspects of the adaptation of aquatic mammals in considerable detail. Such a treatment necessitates a consideration, not only of the Cetacea and Sirenia which are entirely aquatic, but also of a long series of other forms leading gradually less and less exclusively aquatic lives, passing through the Pinnipedia, Hippopotamidæ, Ornithorhynchus, Castor, Lutra, down to forms like the Mink, which are only slightly modified. The author has also taken a wider view and instituted comparisons with members of the Reptilia, living and extinct, suited for the same manner of life.

In spite of the detail it contains, which is considerable, the book should be understood by any average lay reader who is interested in the subject. It forms a storehouse of knowledge also from which the zoologist can draw much that is new. The value of this aspect of the work is enhanced by a very full bibliography and a number of well-chosen and well-produced illustrations.

The most obvious need for an active aquatic mammal is a means of progression through the medium in which it lives, and so the first chapter is devoted to a consideration of the mechanism of swimming. The second is also in a measure introductory, for it discusses what is meant by the term aquatic mammals, and gives brief descriptions of the individuals or groups that fall within its scope. The remaining chapters take up various external and internal characters, and each of them constitutes a very useful review of the particular feature, treated always with original contributions from the author's own researches.

Altogether it is a book upon which the author is to be heartily congratulated, and it should have a wide appeal.

C. H. O'D.

MEDICINE

Sensation and the Sensory Pathway. By JOHN S. B. STOPFORD, M.D., F.R.S. [Pp. xii + 148, with 19 illustrations and 5 tables.] (London: Longmans, Green & Co., 1930. Price 7*s.* 6*d.* net.)

IN this book the author gives the result of personal researches into the structure and function of the sensory pathway of the skin, which have extended over fifteen years. These have included investigations of many hundreds of patients suffering from injuries to or lesions of the peripheral or central nervous systems, as well as studies in comparative anatomy, which must be taken into account in any satisfactory explanations and interpretations of the clinical observations.

He reviews the important work done in this field during the past twenty or thirty years, and pays tribute to the classical experiments and observations of Head, which he describes as epoch-making. As many will know, in order to obtain a series of well-controlled observations, Head carried out an experiment upon himself. Two cutaneous nerves were divided in his arm, then immediately sutured together with as little distortion and disturbance of their relations as possible, and the stages of recovery closely observed by Rivers under experimental conditions. The result of this experiment was to convince Head that recovery took place in two stages, the first involving the primitive sensations, the second the discriminative, which he called respectively the protopathic and the epicritic.

Stopford, though adding to and emending some of Head's observations, supports his main contention of a two-stage recovery, of which he gives an interesting and suggestive explanation. He associates the first with the growing out of the regenerating fibres to the widely distributed and primitive sensory endings, which centrally make connection with the thalamus, and the second with growth to the less abundant and more discriminative endings, for whose proper action associations with the sensory cortex have to be established. Another interesting point he makes is that, owing to the irregular course of the nerve fibres in a nerve trunk, where bundles frequently cross

and anastomose or run in a spiral manner, the chances of nerve fibres with similar distributions coming together after section will depend very much upon the absence of distortion or loss of any considerable length of nerve. As a matter of observation it is found that even in the most satisfactory conditions, such as prevailed in Head's case, there is not complete recovery of function, whilst following secondary sutures after "freshening" of a nerve there is usually very incomplete recovery of the discriminative functions. On the other hand, after neurolysis there is a good chance of very complete recovery, and recovery moreover of all sensations at the same time, without the appearance of the two stages.

This valuable and attractively short book (and in these days of much printing a small pithy book is something to be grateful for) ends with a chapter devoted to a consideration of the kind and distribution of the end organs of the skin, that most ancient of sense organs, which "enables man to protect himself from a harmful environment, and by touch to study himself and learn about his surroundings."

W. C. CULLIS.

Microscopic Pharmacognosy. By WILLIAM MANSFIELD, A.M., Phar.D., Ph.G. [Pp. x + 211.] (London: Chapman & Hall, Limited, 1929. Price 15*s*. net.)

THESE drawings illustrative of the microscopic structure of powdered drugs are well and clearly drawn and are on the same scale of magnification, although its actual amount is not stated; they are supplemented by drawings of sections of the different structures involved, *e.g.* leaf, bark, root, etc. On the page opposite to the drawings of characteristic structures are short descriptions, stating the salient features. With these two aids the student of Pharmacy should be able to get a working knowledge of the microscopic characters of a number of drugs and their adulterants.

E. M. C.

MISCELLANEOUS

Human Speech [pp. xiv + 360, price 25*s*. net] **and Babel** [pp. 93, price 2*s*. 6*d*. net]. By Sir RICHARD PAGET. (London: Kegan Paul, Trench & Trübner, 1930.)

THE first of these books forms one of a series entitled *The International Library of Psychology, Philosophy, and Scientific Method*, and the author has given in it a popular but detailed exposition of the scientific basis of phonetics. The main part of the work is taken up with a description of the experiments on resonators in which the author has employed his gifted ear to synthesise speech sounds, and to analyse them into their frequency components. At the same time the achievements of other workers in this field are not forgotten, and the reader will find a comprehensive account of the progress and present position of research in this subject. The section on voice production is necessarily restricted, since this subject demands a book to itself for its proper exposition, but one is pleased to see that several fallacies current among some teachers of singing are ably refuted by the author. Apart from the purely scientific sections, the book consists of an elaboration of the theory that the origin of speech is to be found in pantomimic gesture with the mouth. This thesis is illustrated by accounts of the history and development of a number of primitive languages.

In the second little book, one of the *To-day and To-morrow* series, Sir Richard Paget again outlines the gesture theory and concludes with an eloquent appeal for the reform of the English language along rational lines, but one is afraid that his suggestion for the establishment of a department to manufacture new words as they are required, and to direct a censorship of slang, will never bear fruit.

E. G. R.

Photographic Printing Processes. By Capt. Owen Wheeler, F.R.P.S. [Pp. xvi + 260.] (London: Chapman & Hall, 1930. Price 8*s.* 6*d.* net.)

This attractive little book deals in an elementary way with photographic printing by chemical, as distinct from mechanical, methods. The author's aim has been to provide information sufficient for any reader to select and work a process with some degree of proficiency; there is no doubt that he has succeeded. The book is, however, almost entirely devoted to what may be called the "recipes" of the craft; there is no attempt to explain the relation between the range of tones in the negative to that obtained in the print. This omission makes the book less clear than it might have been. Had the reader been furnished with an outline of this relationship he would have been enabled much more easily to choose the best printing material for a given negative and, vice versa, he would have been able to decide how to develop his negatives in order to suit a given printing material. There are several minor errors which in a second edition will doubtless be corrected; for example on page 165 it is recommended to dilute sulphuric acid by pouring water into the acid.

S. O. R.

A Study of the Induction Motor. By F. T. Chapman, D.Sc. (Eng.), M.I.E.E. [Pp. xvi + 289, with 195 figures.] (London: Chapman & Hall, Ltd., 1930. Price 21*s.*)

With the recent growth in the use of induction motors this volume comes as a most appropriate and valuable addition to our reference libraries. Nowadays manufacturers have sufficient knowledge of these motors to produce a good standard of machine, and it is only by a very searching inquiry into the theory of their operation, running parallel with a careful study of constructional details, that real progress in design can be made. The task undertaken by Dr. Chapman is primarily to elucidate the theory, to give designers and all those interested in this subject a solid foundation from which to work, thus helping them to develop new ideas. The method of analysis adopted is exceedingly good and most illuminating, enabling one to get such an insight into the special problems connected with these motors that no superficial survey would yield. The reader must be conversant with general principles, and have a fair knowledge of mathematics, but the argument is developed with such consistent logic that it is never difficult to follow. There is comparatively little descriptive matter, which is an advantage in a book of this type, because there is no tendency to divert attention from the main theme. A number of very useful numerical examples are employed to illustrate the application of the more important theoretical results, and in some cases the ideas are also developed graphically. Starting with the usual conception of a rotating field, produced in the ideal case by an infinite number of phases, the interaction of the stator and rotor currents are examined both mathematically and with the aid of vector diagrams. The mechanical construction of the machine is then very briefly considered, followed by a chapter devoted to windings, and another to the calculation of their inductance. Most of the substance of the book circles round the next section, which is given to the harmonic analysis of the air-gap field, and it is shown that only the fundamental frequency is of practical importance in determining the main characteristics of the motor. The squirrel-cage rotor is examined in some detail, and multi-speed devices, cascade connection, the Hunt machine, and starting arrangements are all carefully considered. An investigation of the magnetic circuit follows with some very useful information about the effects of saturation and flux pulsation. The book closes with a number of practical calculations based on the circle diagram,

the experimental determination of which is also explained. The general arrangement of the subject-matter is excellent, but the chapters devoted to the magnetic circuit might perhaps have been placed with advantage more towards the earlier part of the book. Dr. Chapman has every reason to be proud of his work. He is obviously particularly gifted in the art of exposition, and the publishers are to be congratulated on the splendid way in which they have set out and produced the volume.

H. M. Barlow.

The Hair: Its Care, Diseases, and Treatment. By W. J. O'Donovan, O.B.E., M.D. (Lond.), M.R.C.P. [Pp. x + 218 + 21 plates.] (London: J. & A. Churchill. Price 12*s.* 6*d.* net.)

Although hair is a possession common to all humanity and one highly prized, at any rate by women, it is not less remarkable than deplorable how widespread is the ignorance concerning its nature. Many folk still seem to regard their hair as a species of alien growth, like the mosses on old walls or fungi on trees. It is only when some serious trouble develops, and not always then, that the aid of the dermatologist is sought: for the most part, the care of our hair is entrusted to the hairdresser. Amongst hairdressers there is a movement making for increased knowledge, and some hairdressers are well versed in the science of trichology. Unfortunately, many hairdressers are woefully ignorant of its very rudiments. As Dr. O'Donovan remarks in this interesting book: " It is a great reproach to the hairdresser's calling that, although they have handled human heads day by day for centuries, it is the rarest possible event for a case of ringworm to be sent to a physician by a hairdresser." Dr. O'Donovan refers in his Preface to the educational movement amongst hairdressers and the need for encouraging it. It seems, therefore, rather regrettable that he has not written his book, which contains so much valuable information, in a style less technical. The general reader, one fears, will never peruse its pages, and the hairdresser who essays the task will find it no easy one. This is particularly unfortunate, since the volume contains many facts which deserve to be widely known.

One chapter of particular interest to everybody is that dealing with " The Public Health Aspect," and the dangers arising from the unsupervised barber's shop. Dr. O'Donovan gives the excellent regulations operative in the city of London, as well as the equally excellent recommendations made by him in the cases of two large orphanages where severe epidemics of ringworm occurred.

There is, too, an interesting section dealing with the dangers attendant upon the use of depilatories. This is especially needed to-day, when the absurd craze for depilated armpits has, as the result of assiduous propaganda on the part of manufacturers, reached such large dimensions amongst the young women of this country.

Dr. O'Donovan's book also contains a chapter dealing with hair-dyes, in which, however, their dangerous character is rather over-stressed. Actually, few cases of hair-dye intoxication have been recorded with dyes other than those of the *para*-phenylene-diamine group. The salts of cobalt hardly deserve to be labelled poisonous, and those of copper seem to be much less toxic than was formerly supposed. The really toxic ingredient in most metallic hair-dyes (except those based on lead) is the pyrogallol, a vegetable product used in conjunction with the metallic ingredients, though much depends on the concentration and the care or lack of it with which the dyes are used. In the case of walnut-extract, the colouring principle would appear to be juglone, not pyrogallol as stated. As a hair-dye, this extract has been described as worthless.

These are some of the points of general interest in Dr. O'Donovan's book. The dermatologist will find in it much other information of value,

though he may not always see eye to eye with the author in his classification of the diseases of the hair, and may think that in some cases there is lack of precise definition. The subject is undoubtedly a very difficult one. Some conditions, such, for example, as monilethrix, are extremely rare, so that opportunities for scientific study are few and far between. Dr. O'Donovan has done good service in gathering together details of the less common pathological conditions, and his book is beautifully and adequately illustrated.

On the whole, one cannot but feel that trichology has made relatively little advance during the past few years, and that many problems still await complete solutions. Undoubtedly the discovery of the depilatory virtues of thallium is of value in the treatment of ringworm, etc. The salts of this metal, however, are very poisonous, and the treatment, details of which are given, are fraught with danger, especially in the case of children. The X-rays have also been pressed into service, and have proved themselves of value, though also possessed of very dangerous potentialities.

Alopecia areata is considered very fully in Dr. O'Donovan's book; and there is a section devoted to Alopecia senilis, a condition very common especially in men, which fact, the author thinks, may be due to the wearing of hard, ill-ventilated hats. "This trouble," he says, "is incurable, but I would," he adds, "hesitate to deny that rigorous treatment will not put a drag upon the pace of its ravages."

H. S. Redgrove.

Cogitans Cogitata. By Herbert Wildon Carr. [Pp. xii + 110.] (London: The Favil Press, 1930. Price 6*s*.)

In this little book Prof. Carr gives a concise statement of his philosophic creed unencumbered by criticisms and arguments which might make it difficult to see the wood for the trees. This does not mean that the author is content to be dogmatic, but only that he considers he has already furnished all that is necessary by way of critical argument in his other works. The philosophy here outlined is a Monadism, that is to say, the world is conceived to consist of "active, living, individual subjects of experience," which, after the manner of Leibniz and even earlier writers, Dr. Carr calls "monads." These monads are purely spiritual, and have no external form or properties. In fact there are no external, material objects. So-called material things are merely appearances in the perceptions of the monads. Such a view, as the author frankly admits, makes knowledge solipsistic. But this appears to him to be a self-evident fact, and he thinks that it is an illusion to suppose "that we are disinterested observers who contemplate the universe from a privileged point of vantage outside it"—"we can no more view the universe from without than we can step outside ourselves." "What we behold in the world is only our own activity reflected to us in the imagery which gives that activity expression." Prof. Carr finds an analogy to his theory of knowledge in the zoologist's discovery of certain organisms in the depths of the ocean which contrive to see in the dark by means of the light which they themselves generate. One wonders whether this analogy would have been available if Dr. Carr's theory of knowledge were true. But perhaps this is not altogether a fair criticism. A solipsistic theory of knowledge will probably appear strange to most men of science, who are wont to assume that knowledge concerns an objective reality and an objective order independent of the knowing mind. Dr. Carr has been influenced largely by some of the recent physical and biological theories. Even some hardened men of science have been influenced by the same theories so far as to lean towards some kind of idealism or spiritualism. These, no doubt, will be particularly interested in the confession of faith of the *Cogitans Cogitata*. As a close student of the history of science I must say that this tendency does not seem in accordance with the whole spirit and development of science,

and in some ways is rather reminiscent of the mythological habit which preceded science and which has also periodically obstructed its progress after it had made a beginning. But that is a personal impression, and the cosmic riddle is sufficiently complex to leave room for many rival hypotheses, including that of Prof. Carr. Anyway, *Cogitans Cogitata* makes interesting reading, and is a joy to handle. The author's attention to the material appearance of the book is specially commendable in view of his denial of the reality of mere externals.

A. WOLF.

Possession Demoniacal and other among Primitive Races, in Antiquity, the Middle Ages, and Modern Times. By T. K. OESTERREICH, Professor at the University of Tübingen. Translation by D. IBBERSON, M.A. [Pp. viii + 400.] (London: Kegan Paul, Trench, Trübner & Co., Ltd., 1930. Price 21*s*. net.)

THIS book, which is a translation of the German edition of 1921, furnishes a comprehensive history of the state of possession as it has been manifested among ancient and modern, primitive and advanced peoples. Quite half the volume consists of quotations, and the writer appears to have been more interested in descriptions and sources than in classification or generalisation. It is supposed that the condition is essentially the same in all cases, however different its manifestations may be owing to the influence of religious or other beliefs. A broad division is made between so-called spontaneous possession and artificial or voluntary possession. The experience is generally forgotten by the subject, and auto-descriptive accounts are rare. But the phenomenon has been extremely widespread, and the history of religion can furnish abundant descriptions of it. The vast majority of these were written by those who themselves believed in a thorough-going dualistic explanation, whether inspirational or demoniacal, and considerable distortion is to be expected on that account. The mere multiplication of cases is not sufficient to warrant the belief in the complete change in voice, features, and bearing which were supposed to be controlled by a second individual spirit. Prof. Oesterreich implies at times that, in his opinion, these changes were so great that the subject could not possibly have counterfeited them while in a normal state. In spite of its occurrence in classical times, as is instanced by the Pythoness of Delphi and by the Sibyls, the condition almost disappeared as civilisation advanced. It rapidly declined with the decline in the belief in demons and spirits of all kinds. In recent times the spiritualist movement has revived the belief in a spirit-world, and cases of possession have been produced with greater frequency. This aspect of the subject is hardly touched on here. The translation is effective.

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BOOKS RECEIVED

(Publishers are requested to notify prices.)

Barlow's Tables of Square, Cubes, Square Roots, Cube Roots, and Reciprocals of all Integer Numbers up to 10,000. Third Edition. Edited by L. J. Comrie, M.A., Ph.D., H.M. Nautical Almanac Office. London: E. & F. N. Spon, 57 Haymarket, S.W.1; New York: Spon & Chamberlain, 120 Liberty Street, 1930. (Pp. xii + 208.) Price 7*s.* 6*d.* net.

Contributions to the History of Determinants, 1900–1920. By Sir Thomas Muir, D.Sc., LL.D., F.R.S., C.M.G., formerly Superintendent-General of Education in Cape Colony. London and Glasgow: Blackie & Son, 1930. (Pp. xxiv + 408.) Price 30*s.* net.

The Quarterly Journal of Mathematics. Oxford Series, Vol. I, No. 2, June 1930. Oxford: at the Clarendon Press, 1930. (Pp. 77–174.) Price 7*s.* 6*d.* net.

Applied Mathematics for Engineers. Vol. I, Graphical Statics. By T. Hodgson, B.A., B.Sc., Mathematical Lecturer at the City and Guilds (Eng.) College, Imperial College of Science and Technology. London: Chapman & Hall, 1930. (Pp., Vol. I, vii + 183; Vol. II, vi + 293.) Prices 9*s.* 6*d.* and 13*s.* 6*d.* respectively.

Theory of Functionals and of Integral and Integro-Differential Equations. By Vito Volterra, For. Memb. R.S., Professor in the University of Rome. Edited by Luigi Fantappie, Professor in the University of Palermo. Authorised Translation by Miss M. Long. London and Glasgow: Blackie & Son, 1930. (Pp. xiv + 226.) Price 25*s.* net.

Number, the Language of Science. By Tobias Dantzig, Ph.D., Professor of Mathematics, University of Maryland. London: George Allen & Unwin, Museum Street. (Pp. x + 260, with 11 plates.) Price 10*s.* net.

Geometry of Four Dimensions. By A. R. Forsyth, Sc.D., LL.D., Math.D., F.R.S., Emeritus Professor, Imperial College of Science and Technology, London. Cambridge: at the University Press, 1930. (Pp., Vol. I, xxix + 468; Vol. II, xi + 520.) Price 75*s.* net.

Algebraic Charts. By Edgar Dehn. (Six charts.) London: Oxford University Press, 1930. Price 3*s.* 6*d.* net.

Addition-Subtraction Logarithms to Five Decimal Places. By L. M. Berkeley. New York: White Book and Supply Co., 36 West 91st Street. (Pp. xii + 135.) Price $3.25 postpaid.

The Theory of the Potential. By William Duncan MacMillan, A.M., Ph.D., S.C., Professor of Astronomy, the University of Chicago. London: McGraw-Hill Publishing Co., 6 Bouverie Street, E.C.4, 1930. (Pp. xiii + 469.) Price 25*s.* net.

Star Clusters. By Harlow Shapley. London: McGraw-Hill Publishing Co., 6 Bouverie Street, E.C.4, 1930. (Pp. xi + 276.) Price 15*s*. net.

Astronomy: An Introduction. By Robert H. Baker, Ph.D., Professor of Astronomy in the University of Illinois. London: Macmillan & Co., St. Martin's Street, 1930. (Pp. xix + 521, with illustrations.) Price 16*s*. net.

Comets. By Charles P. Olivier, Professor of Astronomy and Director of Flower Observatory, University of Pennsylvania. London: Baillière, Tindall & Cox, 8 Henrietta Street, Covent Garden, W.C.2, 1930. (Pp. x + 246, with 7 plates.) Price 16*s*. net.

The Universe Around Us. By Sir James Jeans, M.A., D.Sc., Sc.D., LL.D., F.R.S. Cambridge: at the University Press, Second Edition, 1930. (Pp. x + 363, with 24 plates.) Price 12*s*. 6*d*. net.

Determination of Orbits of Comets and Asteroids. By Russell Tracy Crawford, Professor of Astronomy, University of California. London: McGraw-Hill Publishing Co., 6 Bouverie Street, E.C.4. (Pp. xi + 233.) Price 20*s*. net.

Optical Rotatory Power. A General Discussion held by The Faraday Society, April 1930. (Pp. 265–461.) Price 10*s*. 6*d*. net.

Intermediate Physics. By R. A. Houstoun, M.A., D.Sc., Lecturer on National Philosophy in the University of Glasgow. London: Longmans, Green & Co., 1930. (Pp. xviii + 638.) Price 10*s*. 6*d*. net.

X-ray Technology: The Production, Measurement, and Application of X-rays. By H. M. Terrill, Ph D., Associate in Physics, Institute of Cancer Research, and C. T. Ulrey, Ph.D., Research Physicist, Westinghouse Lamp Company. London: Chapman & Hall, 1930. (Pp. viii + 256, with 143 figures.) Price 21*s*. net.

Sound Waves and their Uses. Six Lectures delivered before a Juvenile Auditory under the Auspices of the Royal Institution, Christmas, 1928. By Alexander Wood, M.A., D.Sc., Fellow and Tutor of Emmanuel College, Lecturer in Experimental Physics, Cambridge University. London and Glasgow: Blackie & Son, 1930. (Pp. x + 152, with 141 figures.) Price 7*s*. 6*d*. net.

Physical Principles of Electricity and Magnetism. By R. W. Pohl, Professor of Physics in the University of Göttingen. Authorised Translation by Winifred M. Deans, M.A., B.Sc. London and Glasgow: Blackie & Son, 1930. (Pp. xi + 356, with 43 figures.) Price 17*s*. 6*d*. net.

The Study of Crystals. A General Introduction. By T. V. Barker, Fellow of Brasenose College, Oxford. London: Thomas Murphy & Co., 1 Fleet Lane, E.C.4. (Pp. xvi + 137, with 181 figures.) Price 8*s*. 6*d*. net.

Band Spectra and Molecular Structure. By R. de L. Kronig, Ph.D., University of Groningen. Cambridge: at the University Press, 1930. (Pp. x + 163.)

Lecture Experiments in Optics. By B. K. Johnson, F.R.M.S., Demonstrator in the Technical Optics Department of the Imperial College of Science and Technology, with a Foreword by Prof. L. C. Martin, D.Sc., A.R.C.S., D.I.C. London: Edward Arnold & Co., 1930. (Pp. 112, with 90 figures.) Price 8*s*. 6*d*. net.

Applications of Interferometry. By W. Ewart Williams, M.Sc., Lecturer in Physics in the University of London, King's College. With a Preface by O. W. Richardson, F.R.S. London: Methuen & Co., 36 Essex Street, W.C. (Pp. viii + 104, with 43 diagrams.) Price 2*s*. 6*d*. net.

An Outline of Wave Mechanics. By N. F. Mott, Lecturer in Theoretical Physics, the University, Manchester. Cambridge: at the University Press, 1930. (Pp. 155.) Price 8*s*. 6*d*. net.

A Treatise on Light. By R. A. Houstoun, M.A., Ph.D., D.Sc. London: Longmans, Green & Co., 1930. (Pp. xi + 494, with 340 figures and 2 plates.) Price 12*s*. 6*d*. net.

In the Realm of Carbon. The Story of Organic Chemistry. By Horace G. Deming, Professor of Chemistry, University of Nebraska. New York: John Wiley & Sons; London: Chapman & Hall, 1930. (Pp. x + 357.) Price 15*s*. net.

Introduction to Physiological Chemistry. By Meyer Bodansky, Ph.D., Professor of Pathological Chemistry, University of Texas. Second Edition, Rewritten and Reset. New York: John Wiley & Sons; London: Chapman & Hall, 1930. (Pp. ix + 542.) Price 20*s*. net.

Physiology and Biochemistry of Bacteria. Vol. II, Effects of Environment upon Micro-organisms. Vol. III, Effects of Micro-organisms upon Environment. By R. E. Buchanan, Ph.D., and Ellis I. Fulmer, Ph.D. London: Baillière, Tindall & Cox, 8 Henrietta Street, Covent Garden, W.C.2, 1930. (Pp. Vol. II, xvi + 709, with 57 figures; Vol. III, xv + 575, with 2 figures.) Price 34*s*. each.

Analytical Chemistry. Based on the German Text of F. P. Treadwell, late Professor of Analytical Chemistry at the Polytechnic Institute of Zurich. Translated and Revised by William T. Hall, S.B., Associate Professor of Analytical Chemistry, Massachusetts Institute of Technology. Vol. I, Qualitative Analysis. Seventh English Edition, Revised. New York: John Wiley & Sons; London: Chapman & Hall, 1930. (Pp. ix + 610.) Price 23*s*. net.

Recent Advances in Physical and Inorganic Chemistry. By Alfred W. Stewart, D.Sc., Professor of Chemistry in the Queen's University of Belfast. Sixth Edition. London: Longmans, Green & Co., 1930. (Pp. xi + 387, with 37 figures and 5 plates.) Price 18*s*. net.

Enzymes. By J. B. S. Haldane, M.A., Sir William Dunn Reader in Biochemistry, Cambridge University. London: Longmans, Green & Co., 1930. (Pp. vii + 235.) Price 14*s*. net.

An Index to the Chemical Action of Micro-organisms on the Non-Nitrogenous Organic Compounds. By Ellis I. Fulmer, Ph.D., Professor of Biophysical Chemistry, Iowa State College, and C. H. Werkman, Ph.D., Associate Professor of Bacteriology, Iowa State College. Assisted by Anella Wieben and Calvin R. Breden, Instructors in Chemistry, Iowa State College. London: Baillière, Tindall & Cox, 7 Henrietta Street, Covent Garden, W.C.2. (Pp. xiii + 198.) Price 20*s*. net.

A Textbook of Inorganic Chemistry for University Students. By J. R. Partington, M.B.E., D.Sc., Professor of Chemistry at East London College, University of London. Third Edition. London: Macmillan & Co., St. Martin's Street, 1930. (Pp. viii + 1083, with 391 figures.) Price 15*s*. net.

The Materials of Life. A Simple Presentation of the Science of Biochemistry. By T. R. Parsons, M.A., B.Sc. London: George Routledge & Sons, 1930. (Pp. 288.) Price 10s. 6d. net.

Quantum Chemistry. A Short Introduction in Four Non-Mathematical Lectures. By Arthur Haas, Ph.D., Professor of Physics at the University of Vienna. Translated by L. W. Codd, M.A. (Oxon.). London: Constable & Company, 1930. (Pp. ix + 75.) Price 6s. net.

The Electrochemistry of Solutions. By S. Glasstone, D.Sc., Ph.D., F.I.C., Lecturer in Chemistry at the University of Sheffield. London: Methuen & Co., 36 Essex Street, W.C. (Pp. ix + 476, with 38 figures.) Price 21s. net.

A Textbook of Organic Chemistry. By A. F. Holleman, Ph.D., LL.D., D.Sc., F.R.S.E. Seventh English Edition. Completely revised with the co-operation of the Author. New York: John Wiley & Sons; London: Chapman & Hall, 1930. (Pp. xx + 594.) Price 17s. 6d. net.

Photochemistry. By D. W. G. Style, Ph.D., with a Preface by A. J. A. Allmand, M.C., D.Sc., F.R.S., Professor of Chemistry in the University of London, King's College. London: Methuen & Co., 36 Essex Street, W.C. (Pp. vii + 96, with 9 diagrams.) Price 2s. 6d. net.

The Scientific Fundamentals of Gravity Concentration. By Josef Finkey, A.M., Professor of Ore Dressing of the School of Mines, Sopron, Hungary. Bulletin, Official Publication of School of Mines and Metallurgy, University of Missouri. Translated into English by C. O. Anderson and M. H. Griffiths. Rolla, Missouri, U.S.A.: School of Mines and Metallurgy, 1930. (Pp. 295.) Price $1.

Impurities in Metals: Their Influence on Structure and Properties. By Colin J. Smithells, M.C., D.Sc. London: Chapman & Hall, 1930. (Pp. xiii + 190, with 181 figures.) Price 18s. net.

Limestones: Their Origins, Distribution, and Uses. By F. J. North, D.Sc., F.G.S., Keeper of the Department of Geology in the National Museum of Wales. Foreword by Sir Henry P. Maybury, G.B.E., K.C.M.G., C.B., M.Inst.C.E., President Institute of Quarrying, sometime Director-General of Roads, Ministry of Transport. London: Thomas Murby & Co., 250 Fourth Avenue, 1930. (Pp. xxiii + 467, with 236 figures.) Price 16s. net.

Outlines of Physical Geology. Prepared from the Third Edition of Part I of a Textbook of Geology by the late Louis V. Pirsson and Charles Schuchert. By Chester R. Longwell, Professor of Geology in Yale University. New York: John Wiley & Sons; London: Chapman & Hall, 1930. (Pp. v + 376, with 275 figures.) Price 15s. net.

The Dispersal of Plants throughout the World. By Henry N. Ridley, M.A., C.M.G., F.R.S., F.L.S. Illustrated by Miss N. B. Moss and the Author. Ashford, Kent: L. Reeve & Co., Lloyds Bank Buildings, 1930. (Pp. xx + 744, with 22 plates.)

An Elementary Course in General Physiology. Part I—Principles and Theory. By G. W. Scarth, Professor of Botany, McGill University, Montreal. Part II—Laboratory Exercises. By F. E. Lloyd, Macdonald Professor of Botany, McGill University, and G. W. Scarth, Professor of Botany, McGill University. New York: John Wiley & Sons; London: Chapman & Hall, 1930. (Pp. xxi + 258, with 66 figures.) Price 13s. 6d. net.

Size and Form in Plants, with Special Reference to the Primary Conducting Tracts. By F. O. Bower, Sc.D., LL.D., F.R.S., Emeritus Professor of Botany in the University of Glasgow. London: Macmillan & Co., St. Martin's Street, 1930. (Pp. xiii + 232, with 72 figures.) Price 12*s*. 6*d*. net.

Manual of Bacterial Plant Pathogens. By Charlotte Elliott, Associate Pathologist, Bureau of Plant Industry, United States Department of Agriculture. London: Baillière, Tindall & Cox, 8 Henrietta Street, Covent Garden, W.C.2, 1930. (Pp. ix + 349.) Price 22*s*. 6*d*. net.

The Seasonal Distribution of Farm Labour Requirements. By W. H. Kirkpatrick, C.D.A., Dip. Agric. Econ., University of Cambridge Department of Agriculture, Farm Economics Branch. Report No. 14. Cambridge: W. Heffer & Sons, 1930. (Pp. iv + 44.) Price 2*s*. net.

Factors affecting the Price of Potatoes in Great Britain. By Ruth L. Cohen, B.A., Newnham College, Commonwealth Fund Fellow. Cambridge: W. Heffer & Sons, 1930. (Pp. iv + 55.) Price 2*s*. 6*d*. net.

University of Cambridge Department of Agriculture, Farm Economics Branch Report, No. 16. No. 3, Sugar Beet in the Eastern Counties, 1929. Being the Final Report on an Investigation into the Financial Results of Sugar Beet Growing, and the Principal Factors influencing them during the Three Seasons 1927, 1928, and 1929, with Particular Reference to the Latter Year. By C. Burgess, M.A., Dip. Agric. Econ., and P. E. Graves. Cambridge: W. Heffer & Sons, 1930. (Pp. ix + 69.) Price 2*s*. 6*d*. net.

Poultry Husbandry. By Morley A. Jull, Senior Poultry Husbandman. Bureau of Animal Industry, U.S. Department of Agriculture. London: McGraw-Hill Publishing Co., 6 Bouverie Street, E.C.4, 1930. (Pp. ix + 639, with 229 figures.) Price 20*s*. net.

The Bureau of Entomology: Its History, Activities, and Organisation. By Gustavus A. Weber, Institute for Government Research. Service Monographs of the United States Government, No. 60. Washington: The Brookings Institution, 1930. (Pp. xii + 177).

Vertebrate Embryology. A Textbook for Colleges and Universities. By Waldo Shumway, Ph.D., Professor of Zoology, University of Illinois. Second Edition, Thoroughly Revised. New York: John Wiley & Sons; London: Chapman & Hall, 1930. (Pp. x + 311, with 196 figures.) Price 18*s*. 6*d*. net.

Principles of Animal Biology. By Lancelot T. Hogben, M.A., D.Sc., Professor of Social Biology in the University of London. London: Christophers, 22 Berners Street, W.1. (Pp. xxiv + 332, with 125 figures.) Price 8*s*. 6*d*. net.

The New Evolution Zoogenesis. By Austin H. Clark. London: Baillière, Tindall & Cox, 8 Henrietta Street, W.C.2, 1930. (Pp. xiv + 297, with 141 figures.) Price 13*s*. 6*d*. net.

Histological and Illustrative Methods for Entomologists. By H. Eltringham, M.A., D.Sc., F.R.S. With a Chapter on mounting whole insects, by H. Britten, F.E.S. Oxford: at the Clarendon Press, 1930. (Pp. xi + 139, with 21 figures.) Price 7*s*. 6*d*. net.

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The Hair: Its Care, Diseases, and Treatment. By W. J. O'Donovan, O.B.E., M.D., M.R.C.P. London: J. & A. Churchill, 40 Gloucester Place, Portman Square, 1930. (Pp. x + 218, with 40 figures.) Price 12*s* 6*d*. net.

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Non-Metallic Inclusions in Iron and Steel. By Dr. Carl Benedicks, Director of the Metallografiska Institutet, Stockholm, and Hon. Vice-President of the Iron and Steel Institute and Helge Löfquist Assistant at the Metallografiska Institutet, Stockholm. London: Chapman & Hall, 1930. (Pp. xi + 311, with 139 figures.) Price 30*s*. net.

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The Person of Evolution. Studies of Instinct as Contributions to a Philosophy of Evolution. By W. D. Lighthall, LL.D. Montreal: H. A. Kennedy & Co., 620 Transportation Building. (Pp. 216.)

The Eighteenth-century Revolution in Science—the First Phase. By Andrew Norman Meldrum, I.E.S., A.R.C.Sc., D.Sc. London: Longmans, Green & Co. (Pp. 60.) Price 4*s*. 6*d*. net.

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Temperature Chart, prepared from Numerous Sources. By Vivian T. Saunders, M.A., Assistant Master, Uppingham School, 15th April, 1930. London: John Murray, 50 Albemarle Street, W.1. Price, unmounted and packed in cardboard roll, 5*s*. 6*d*. net; mounted on strong white linen in cardboard roll, 9*s*. 6*d*. net; mounted on linen, varnished on rollers, 15*s*. 7*d*. net.

A Modern Introduction to Logic. By L. S. Stebbing, M.A. London: Methuen & Co., 36 Essex Street, W.C. (Pp. xviii + 505.) Price 15*s*. net.

Dictionary of Biological Equivalents: German-English. By Ernst Artschwager, Pathologist. Bureau of Plant Industry, U.S. Department of Agriculture. London: Baillière, Tindall & Cox, 8 Henrietta Street, Covent Garden, W.C.2, 1930. (Pp. 239, with 6 plates.) Price 20*s*. net.

Essays and Addresses, Sociological, Biological, and Psychological. By A. Surgeon. London: H. K. Kewis & Co., 1930. (Pp. xiii + 266.) Price 10*s*. 6*d*. net.

Fifty-Two Years of Research, Observation, and Publication, 1877–1929: A Life Adventure in Breadth and Depth. By Henry Fairfield Osborn, with Complete Bibliography, Chronological and Classified by Subject, 1877–1929. New York: Charles Scribner's Sons, 1930. (Pp. xii + 160.) Price $1.50 net.

Last and First Men: A Story of the Near and Far Future. By W. Olaf Stapledon. London: Methuen & Co., 36 Essex Street, W.C. (Pp. xi + 355.) Price 7*s*. 6*d*. net.

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The Animal Mind. By C. Lloyd Morgan, F.R.S. London: Edward Arnold & Co., 1930. (Pp. xii + 275.) Price 12*s*. 6*d*. net.

Religion and the Reign of Science. By F. Leslie Cross, M.A., B.Sc., Librarian at Pusey House, Oxford. London: Longmans, Green & Co., 1930. (Pp. ix + 111). Price 4*s*. net.

The Bronze Age. By C. Gordon Childe, B.Litt., F.R.A.I., F.S.A., Professor of Prehistoric Archæology in the University of Edinburgh. Cambridge: at the University Press, 1930. (Pp. xii + 358, with 31 figures.) Price 8*s*. 6*d*. net.

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The Papyrus Ebers. Translated from the German Version by Cyril P. Bryan, M.B., B.Ch., B.A.O., Demonstrator in Anatomy, University College, London. With an Introduction by Prof. G. Elliot Smith, M.D., D.Sc., Litt.D., F.R.C.P., F.R.S. London: Geoffrey Bles, 22 Suffolk Street, Pall Mall, S.W.1. (Pp. xl + 167, with 16 plates.) Price 10*s*. 6*d*. net.

The Skeletal Remains of Early Man. By Aleš Hrdlička. Smithsonian Miscellaneous Collections, Vol. LXXXIII (whole volume). Washington, U.S.A.: Smithsonian Institution, 1930. (Pp. x + 377, with 20 plates and 39 figures.)

The Mysterious Universe. By Sir James Jeans, M.A., D.Sc., Sc.D., LL.D., F.R.S. Cambridge: at the University Press, 1930. (Pp. viii + 154, with 2 plates.) Price 3*s*. 6*d*. net.

Map. Standard Time Zones of the United States and Adjacent Parts of Canada and Mexico as at April 1, 1930. National Bureau of Standards. Miscellaneous Publication No. 111. For Sale by the Superintendent of Documents, Washington, D.C. Price 10 cents.

The African Republic of Liberia and the Belgian Congo. Based on the Observations made and Material Collected during the Harvard African Expedition, 1926–7. Edited by Richard P. Strong. In 2 vols. Cambridge, U.S.A.: Harvard University Press; London: Oxford University Press, 1930. (Pp. Vol. I, xii + 568, with 443 illustrations; Vol. II, vi + 1064, with 33 illustrations and 28 text figures.) Price 67*s*. 6*d*. net.

Thunder and Lightning: Being the Thirty-second Robert Boyle Lecture delivered before the Oxford University Junior Scientific Club on June 7, 1930. By G. C. Simpson, C.B., F.R.S. Oxford: The Vincent Printing Works, 1930. (Pp. 15.)

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SCIENCE PROGRESS

RECENT ADVANCES IN SCIENCE

APPLIED MATHEMATICS. By F. E. Relton, M.A., B.Sc., Imperial College of Science and Technology, London.

Fluid Motion.—The days when the study of the perfect fluid can offer any real assistance to the study of the fluid of nature, "with all its imperfections heaped upon it," seem to be drawing to a close. For years the former was nurse to the latter and fulfilled its functions admirably; but the infant is growing up and learning to stand on its own feet, though naturally its first steps are somewhat tottering.

The perfect fluid theory enabled us, by a suitable choice of sources and sinks in a uniform stream, to represent the profile of an airship. A piece of pure mathematics, in the hands of Joukowsky, provided a beautiful transformation that would conformally represent a circle as a curve that bore an astonishingly good likeness to an aerofoil section. To this, Joukowsky added another proposition, to the effect that a judicious combination of uniform irrotational flow with circulation would bring the stagnation point to the tail.

The experimental substantiation of this result was good enough to satisfy for a time all reasonable demands, inasmuch as even if the actual phenomena were not precisely those of the Joukowsky analysis, the net result was sufficiently close to actuality to warrant one in using it for practical purposes.

But "this progress, it goes on." The speed achieved by aeroplanes rose and rose, until at last it has been forced upon some acute minds that the compressibility of the medium was perhaps not a factor to be ignored. When an aerofoil is placed in a steady stream, the resistance remains low and the lift coefficient rises as the speed of the stream is increased. But there comes a time and a speed, depending on the shape of the body and the angle of incidence, when a breakdown occurs. The lift rapidly falls off and the resistance rises. Is it inherent in the mathematics that a steady flow is not possible above a certain speed, and what part is played by the compressibility of the medium?

It must have come as something of a shock to many

students of hydrodynamics to realise how little attention has ever been paid to the possibility of the medium being compressible. Yet the reason is not far to seek. All the reward that most mathematicians can expect from their labours is the kudos accruing from a successful investigation, and the allurements of non-linear partial differential equations are not strong. Leave the perfect fluid, where the continuity is impressed by the velocity v having zero divergence, and come to the compressible medium where the divergence of ρv is zero, ρ being the density. As a functional connection between the density and the pressure we adopt the adiatatic law and immediately we are landed in the trackless wood of non-linear partial differential equations.

An early tentative approach to the solution was made by Rayleigh (*Phil. Mag.*, 1916). Taking the two-dimensional case, he indicated a possible line of advance whose essence is successive approximation. This method he applied to the circular cylinder. The success of a method of this type depends in part on the ease with which the successive approximations can be made, and certainly on the rapidity of their convergence. If it eventuates that for some speed of flow the approximations diverge, then it may be there is something inherent that prohibits steady motion above a certain velocity.

An ingenious mechanical method of approaching the problem was devised by G. I. Taylor. Working on the well-known analogy between flow of electric current and flow of an incompressible liquid, he saw that the approximations needed for a compressible fluid could be replaced in the analogue by variations in the thickness of the conducting sheet. The method was tried out on a circular cylinder and on an aerofoil section; in each case there ultimately came a speed whereat the successive approximations diverged.

This answer is not as decisive as may at first sight appear. The variations in the thickness of the conducting sheet are based on approximative methods whose validity is not irrefragable. The motion of compressible fluids will certainly attract considerable attention in the future. Meantime an excellent account of the work is to be found in the *Jour. of the Lond. Math. Soc.*, vol. 5, pt. 3, no. 19.

What hope is there of progress? Analytically perhaps not very much, for reasons previously indicated. It rather looks as though in the first case we must look to some band of investigators, imbued with the industry of ants and undismayed by the monotony of their task, to work out some case numerically with the minimum of assumption and the maximum of accuracy.

Coming to the incompressible liquid, a paper by G. R.

Goldsbrough, on the "Tidal Oscillations in an Elliptic Basin of Variable Depth," appeared in the *Proc. Roy. Soc.* for December 1930. Work had previously been done by Jeffreys and by Goldstein on the allied problem with uniform depth, a problem whose ultimate solution necessarily lacks that analytical elegance which is, and should be, the æsthetic joy of your true mathematician. In the present paper a suitable choice of the variable depth obviates the lengthy calculations arising from elliptic cylinder functions.

The adopted paraboloidal law of depth leads to a differential equation of the second order possessing four regular singularities. We thus have an equation allied to Lamé's equation and a particular case of the equation discussed by Heun. It is solvable in polynomials, and it mercifully happens that the satisfaction of the boundary conditions does not necessitate using the second independent solution, which involves the logarithmic term.

The author, having discussed some of the earlier modes of vibration and the nature of the resulting nodal curves, allows the elliptic basin to degenerate into the circular form. This affords a comparison with the known results for a circular basin as given in Lamb's *Hydrodynamics*. In a final section of the paper, the question of longitudinal oscillations is examined with a view to comparison with the work of Chrystal and of Proudman on seiches.

On more academic lines, there is a paper in the *Phil. Mag.* for January 1931 by Relton, on "The Slow Rotation of an Anchor Ring in a Viscous Liquid." The motion of most of the commoner surfaces in a viscous liquid has already been examined; but somehow the anchor ring seems to have escaped attention, in spite of the fact that there are co-ordinates specially designed for its treatment.

The paper proceeds on the ineluctable ignoration of the inertia terms, and bears internal evidence of hasty composition. It makes considerable demands on one's knowledge of pure mathematics, and is, in fact, a fearsome compost of dyadics, orthogonal curvilinears and the Riemann P-function. There are no numerical illustrations owing to the dearth of tables of associated Legendre functions.

Elasticity.—It is over twenty years since Walter Ritz published his famous paper in Crelle on a new method for solving certain variation problems in mathematical physics. If the paper has not been utilised to the extent that one might have expected, the reason probably lies in the fact that applied mathematicians did not find it easy going. The late C. G. Knott used it to calculate the deflection of a uniformly loaded square plate. His object was to check some experimental

results obtained by Crawford, but the agreement between the two left something to be desired.

At a later date the problem of the loaded plate was tackled by Hencky, and the answer to the question as to which, is the better method is largely subjective. More recently K. Friedrichs in the *Göttinger Nachrichten*, 1929, has indicated a method which, in certain cases, enables us to correlate two problems in the calculus of variations, one of them iso-perimetrical, in such a manner that the maximum value for the one problem is the minimum value for the other. By this means an estimate of the error in the solution may be obtained.

This method has been utilised in one of two connected papers, written by N. M. Basu and published in the *Phil. Mag.* for November 1930. In the first of the two papers the Ritz method has been applied to the problem discussed by Knott, and a check has been evaluated by the method of Friedrichs. It appears from the results that the deflection of a uniformly loaded square plate can be represented with considerable accuracy by a quite simple formula.

The application of the same method to the torsion problem will give good approximations to the conjugate function. But the actual stresses and displacements depend on the torsion function and not on its conjugate, and here the method avails not at all. Accordingly in the second paper Basu has developed a method for constructing the torsion function in a convergent infinite series of polynomials.

In the same issue of the *Phil. Mag.* appears a paper by Profs. Dinnick and Lokshin on "the Lateral Bending of Bars Limited by Surfaces of the Second Order." The stability of struts with variable cross-section is of considerable practical importance, and a number of cases have already been worked out in detail. In the present paper the line of thrust is taken as the axis of figure, and the boundary surface is a conicoid, but not necessarily of revolution. The cases for the various quadrics are worked out and numerical illustrations are liberally added. It appears that in all cases the critical load can be represented by a very simple formula of the type favoured by engineers.

Stellar Structure.—Recent discussion on stellar structure has taken a distinctly lively turn, and there were brisk interchanges at the meeting of the R.A.S. on January 9. It all arose over a paper by Prof. E. A. Milne, published in the November issue of the *Monthly Notices of the R.A.S.* A previous meeting having left little time for discussion, the R.A.S. decided to make something of a field day of it by devoting the whole of the January 9 meeting to discussing Milne's paper.

The matter at issue can be stated quite simply: "What are

the stars like inside ?" But the battle over this issue, like the feud between Hapley and Pawkins in Wells's story of *The Moth*, dates from some time back and looks like being very protracted. Rather more than a decade ago, in or about 1918, Eddington propounded his mass-luminosity law. Eddington proceeded on the assumption that internally the star was gaseous, and the resulting law provided a mathematical basis for the undeniable fact that the weight of a star was the deciding factor in its luminosity.

The law was neither exact nor universal; but it did serve to give, with a high degree of probability, the luminosity of a star within close limits when one knew the weight. On fitting in the observational data, the results were perhaps not all that could be desired; an unwanted factor of ten appeared all along the line. This was disturbing, but at least it possessed the merit of consistency. Nor was it much more than disturbing. Just as no engineer worth the name would blench in the face of an error of 5 per cent. (provided the issue at stake were not financial), so an astrophysicist can view with impavid eye an unwanted multiplier of ten. And why should he not, accustomed as he is to envisage temperatures running to millions of degrees and matter of which a ton would fail to fill a matchbox?

The hypothesis of a gaseous interior to the stars found little favour in the eyes of Jeans, who propounded alternatively hypotheses *ad hoc*. Each investigator pursued his own line of inquiry, and each achieved a measure of success. The two theories, both endowed with an illustrious protagonist, were there for all to choose between who possessed the requisite knowledge. If Eddington's theory could not claim to account for the colossal density of "white dwarfs," Jeans had hypotheses which, like the theory that bodies gravitate to earth by the action of the spirits of the departed dead, are as difficult to disprove as to prove.

About a year ago a third investigator entered the lists. Prof. E. A. Milne, with an eye on Occam's razor and pondering over copper spheres, covered or bare, found a solution of Eddington's equation that its author had overlooked. Let us, then, said Milne, start from the outer layer of the star, where our scientific footing is on data reasonably assured. Rather than fashion hypotheses concerning the interior and find how well or ill the deductions from these hypotheses fit the facts, let us take the steady state, with gravitation equilibrating radiation pressure; let us integrate the equilibrium equations from the boundary inwards and see whither we are led.

Hoc est do quo agitur. But Jeans will not have it. With a

superiority warranted by his eminence, he tells Milne that his investigations are *vieux jeu*, inasmuch as the proposed line of inquiry was adumbrated by Jeans some years ago. And there the matter stands. No one of the three agrees with either of the other two. Some day a satisfactory conclusion will doubtlessly be reached, though perhaps " not without dust and heat," to use Milton's phrase. It will have to be a very shrewd clout that makes one of the three investigators abandon the hypothesis of his choice ; for to quote again from *The Moth*, " your common man has no conception of the zeal that animates a scientific investigator."

PHYSICS. By L. F. Bates, B.Sc., Ph.D., F.Inst.P., University College, London.

The Copper Oxide Cell.—A series of papers has recently appeared on the photo-electric properties of the copper oxide cell. A very extensive examination of its properties has been made by Auwers and Kerschbaum (*Ann. der Phys.*, **7**, p. 129, 1930). They prepared their cells by forming a layer of Cu_2O upon a copper plate and by placing another metal electrode upon the oxide layer. If the two metal plates are joined in series with a galvanometer and the oxide is illuminated, a current flows through the galvanometer. Such arrangements are known in the German literature as " Sperrschichtphotozellen," because experiment has shown that the effects with which we are mainly concerned are confined to the surface of separation between the oxide and the copper plate. Actually, we have three effects to bear in mind. Firstly, the cell may be used as a rectifier—it has been used commercially as the copper oxide rectifier—and here we are concerned with its properties in the dark. Secondly, its resistance may change on exposure to light ; and, thirdly, a current may be produced in the absence of an applied electromotive force in the manner already described. Auwers and Kerschbaum have examined the behaviour of the cell from a theoretical standpoint, discussing, in particular, whether its mode of action is adequately represented by the insertion of an electromotive force or by an additional source of current in the circuit diagram. They have also discussed the possibility of separating the second and third of the effects mentioned above. Now W. Schottkey (*Phys. Zeit.*, **31**, p. 913, 1930) showed quite definitely that the third effect is produced by electrons emitted from the Cu_2O layer under the action of light. These electrons wander towards the copper plate. The kinetic energy which they possess when liberated by the light is used up in passing through the separating layer,

when they become conduction electrons and have potential energy associated with them. An electromotive force is thus established, and a current may then flow through two resistances in parallel, the oxide layer itself and the external resistance which joins the copper plate to the other electrode. Schottkey describes a very neat experiment, due to Baust, in which the other electrode consisted of a narrow rectangular gold strip. A narrow band of light was thrown upon the oxide surface parallel to and at varying distances from the gold electrode. The current recorded by the galvanometer decreased with increase in the distance between the gold electrode and the band of light in exactly the way the above conception would lead us to expect. This, too, was shown by Auwers and Kerschbaum, who also showed that the photo-current was directly proportional to the intensity of illumination of the oxide surface. The photo-electromotive force, however, was not directly proportional to the intensity, thus showing that we are actually dealing with a new source of current as distinct from a new electromotive force. Moreover, the shape of the curve of electromotive force against intensity of illumination showed clearly that the resistance of the oxide layer decreases on illumination. If an external electromotive force is placed in series with the photocell and the galvanometer, we obtain, of course, the usual characteristic curve in the dark. With a given illumination of a cell which shows a large change of resistance on exposure to light, this characteristic is rotated, and all the characteristics for different intensities of illumination practically pass through one definite point, where the potential, for brevity, is called the stopping potential. The rotation of the dark characteristic is such that points above the stopping potential move towards the regions of greater current. Cells which exhibit a large current on illumination appear to show a change of the stopping potential with increase in intensity of illumination.

The properties of Cu_2O have also been investigated by W. Vogt (*Ann.*, 7, p. 183, 1930), who prepared plates of thin oxide by heating a copper plate 0·5 cm. thick to a temperature of 950 to 1,000 degrees C. for about thirty hours in air The outer layer of black CuO was scraped off, leaving a plate of Cu_2O about 1 mm. thick, with which the electrical conductivity, the Hall coefficient, the thermo-electric power and the thermal conductivity were measured. He indicates that the rectification effects observed with these plates are probably due to traces of CuO, or possibly to other compounds of oxygen with copper, and that they probably occur at the surface of separation of the Cu_2O and CuO.

In discussing the properties of Cu_2O we have really been

discussing the properties of a semi-conductor. We will now address ourselves to the question put by A. Meissner (*Zeit. für Phys.*, **65**, p. 158, 1930), *viz.* What is an insulator? When a plate of quartz cut perpendicularly to an electrical axis is placed between two metal plates between which electrical oscillations are maintained, resonance vibrations may be set up in the quartz. If the quartz is surrounded by a mixture of helium and neon, two definite regions on the edge of the plate will glow, thus marking out a direction in the crystal, making an angle of about 34·5 degrees with the optic axis. In this manner three planes of maximum resonance, which intersect at angles of approximately 90 degrees, are marked out. Meissner assumes that these planes are those in which the heaviest atoms in the crystal are most densely congregated. This provides him with a method for working out the crystal structure of quartz, where the heaviest atoms are those of silicon. From an examination of quartz, which is a very good insulator, Meissner was led to formulate the characteristic features of an insulator. These appear to be formation of cells of positive and negative ions lying close together with very strong binding forces between them, and the existence of polar axes. This is easily proved in the case of asphalt by melting the substance when under the influence of a strong electric field. The asphalt particles set along definite axes, and, on solidification, the molecular complexes retain the positions they previously occupied in the electrical field. The solid thus formed is piezo-electric, like a quartz plate. The piezo-electric phenomena are often distorted by the passage of electricity into the substance during the forming process, and by the production of positive and negative space charges due to the motion of electrolytic ions.

W. Meissner and H. Franz (*Zeit. für Phys.*, **65**, p. 30, 1930) have extended their investigations, using liquid helium, by measuring the conductivities of certain carbides and nitrides. The nitrides of titanium, vanadium and zirconium are supraconducting below temperatures of 1·2, 1·3 and 3·2 degrees K respectively, a double nitride of titanium and zirconium being supraconducting below 3·0 degrees K. The simple carbides of titanium, wolfram, molybdenum, tantalum and niobium are supraconducting below 1·1, 2·8, 7·7, 9·2 and 10·1 degrees K, whilst Mo_2C is supraconducting below 2·4 degrees K. It is interesting that the pure metals tantalum and niobium only become supraconducting when cooled to considerably lower temperatures than their carbides, *viz.* 4·4 to 8·2 degrees K; yet the specific resistances of the carbides are considerably greater than those of the pure metals of which they are composed. Moreover, the above supraconducting compounds are not confined to one crystalline structure,

for they include compounds with hexagonal as well as cubic structure, and the crystalline form of the compound in general is not the same as that of the metal. The view that supraconductivity is due to the motion of electrons from atom to atom apparently does not obtain support from some of these measurements, *e.g.* in the case of tantalum carbide, where the grating constant is larger than that of the pure metal, and in which the distance between two metal atoms is also greater than the corresponding distance in the pure metal, at any rate at room temperature. We have, of course, no information of the way in which the size of the outer electron orbits of the metal atoms are affected by the presence of atoms of carbon or nitrogen. Again, if we assume that supraconductivity arises when free electrons can no longer lose energy by collision with metal atoms, we must make a similar assumption in the case of collisions between free electrons and the carbon or nitrogen atoms in these supraconducting compounds.

Other German contributions to which attention may specially be directed are on the relation between the paramagnetic properties of molecules and their chemical constitution, by D. M. Bose (*Zeit. für Phys.*, **65**, p. 677, 1930), a comparison of two methods for the determination of contact potential differences, by G. Mönch (*Zeit für Phys.*, **65**, p. 233, 1930), an interference experiment on X-rays with Lloyd's mirror, by W. Linnik (*Zeit für Phys.*, **65**, p. 107, 1930), and a small apparatus for the production of very low temperatures, by M. Ruhemann (*Zeit für Phys.*, **65**, p. 67, 1930).

Experiments with High Velocity Positive Ions.—The work of Gamow has given us an expression for the probability that a swiftly moving positive ion may enter an atomic nucleus and combine with it. An examination of this expression shows us that it should be possible to detect the products of such collisions when the positive ions are protons which have acquired their energy by falling through a potential difference of about 300 K.V. The problem therefore arises of making discharge tubes which will stand up to such voltages. The investigations of J. D. Cockcroft and E. T. S. Watson (*Proc. Roy. Soc.*, **129**, p. 477, 1930) lead them to choose the canal-ray tube as the method of production of protons, and to accelerate the latter by means of a steady potential produced by rectifying the current from a low-frequency transformer, smoothing it out by a condenser of a few thousandths of a microfarad capacity. They excited the canal-ray tube by a separate 60 K.V. transformer, the whole being raised to a potential of 300 K.V. above the earth, the necessary current being supplied through two kenotrons in series. It is impossible to give full details of their arrangements here ; it is sufficient

to state that the authors have designed kenotrons which will stand up to discharges of over 300 K.V., and that they hope later to erect apparatus capable of standing up to 500 K.V. Each rectifier consisted of special electrodes mounted inside a bulb of thick-walled glass, 30 cm. in diameter, with arms about 75 cm. long, to the ends of which suitable corona shields were fixed. The two rectifiers were connected in series, and they were continuously evacuated through a similar insulation bulb, provided with tubular electrodes to distribute the electrical stress and thus preve it puncture of the glass. This bulb was inserted at the junction of the two rectifiers, the evacuation being carried out by a three-stage Burch oil-diffusion pump. Hence both rectifiers were always equally hard or soft, so that puncture due to the overloading of one rectifier was avoided. The positive-ray tube was of the Wien type, waxed into a groove in a flat steel ground joint, which, by motion upon a second flat ground surface, permitted the adjustment of the positive-ray tube with respect to the discharge chamber. The latter was a bulb, similar to the rectifier bulbs, provided with cylindrical electrodes, so that the positive rays could pass through it into a small experimental chamber. So far, experiments have been carried out with mixed beams of protons and molecules at voltages up to 280 K.V., and examination made of the radiation produced by their impact on targets of lead and a beryllium salt. Very definite indications of a non-homogeneous radiation were detected by an electroscope. The average hardness of the radiation from lead with protons of 280 K.V. was about 40 K.V. The intensity of the radiation was of the order of one ten-thousandth of that produced by an equal electron source, and increased extremely rapidly between 250 and 280 K.V., there being no marked difference in intensity between the radiations from the two types of targets.

Infra-red Spectra.—An exceedingly useful account of the experimental arrangements for the investigation of the infra-red spectra regions is given by Bailey, Cassie and Angus (*Proc. Roy. Soc.*, **130**, p. 133, 1930). Their experimental technique will be of interest to all who are faced with the problem of using a Paschen galvanometer under unfavourable conditions, for these workers are actually working during the daytime to an accuracy of 1 mm. scale division with a galvanometer whose sensitivity is 10^{-10} to 10^{-11} ampere per mm. deflection with the scale at a distance of 1 metre, in a neighbourhood which would certainly not be chosen for its freedom from vibration and electromagnetic disturbances. Yet the expedients adopted to eliminate these disturbances were comparatively simple. Mechanical disturbances were eliminated by placing

the Paschen galvanometer with its heavy shields on a large block of wood supported by four pairs of the thinnest available porcelain crucibles, placed at the corners. Each pair of crucibles was placed one on top the other and separated by a small piece of five-ply wood and a few pieces of filter paper. Electromagnetic disturbances were suppressed by surrounding the galvanometer with a loosely wound copper and *mu* metal shield, over which were placed the two Paschen shields, between which *mu* metal was again loosely wound. The masses of galvanometer, the wood block and the other accessories being known, their centre of mass was located, and the galvanometer was placed on a modified Julius suspension (*cf.* Johnsrud, *Journ. Opt. Soc. Amer.*, **10**, p. 609, 1925), so that by means of adjustable masses the centre of mass of the whole system was made to coincide with that of the magnet system. Tape-bound springs acted as shock absorbers to the systems. The leads from the thermopile, which was a 20-junction bismuth-silver thermopile enclosed in an airtight case, were completely enclosed in earthed composition tubing. The apparatus was designed for the spectroscopic examination of gases and vapours by means of a prism spectrometer, as a preliminary step. Quartz, fluorite, rock salt and sylvine prisms were used. The absorption tubes were of glass with circular rock salt end plates pressed against their ends with a touch of vaseline. The gas entered the absorption chamber through a large glass spiral as described by Robertson and Fox (*Proc. Roy. Soc.*, **120**, p. 128, 1928). Sulphur dioxide gave a fundamental absorption band, not previously recorded, at 16·494μ, and another at 8·680μ together with combination bands at 5·345μ and 4·338μ. The partial resolution obtained with most of the bands was found sufficient for the determination of the size and shape of the sulphur dioxide molecule, its moments of inertia and its fundamental vibrational frequencies.

Energy Losses of Electrons in Nitrogen.—So much has been written of the analysis of electron beams by deflection in a magnetic field, that one is apt to overlook the advantages of using electrostatic fields for such purposes. Recently, Rudberg (*Proc. Roy. Soc.*, **129**, p. 628, 1930 ; *cf.* also Hughes and Rojansky, *Phys. Rev.*, **34**, p. 284, 1929, and Hughes and McMillen, *Phys. Rev.*, **34**, p. 291, 1929) has measured, with a very high accuracy, the energy lost by electrons in inelastic impacts in nitrogen. The experimental conditions were such that practically only single collisions took place, and only those electrons which suffered very small deviations were actually examined. A slightly divergent beam of electrons, which had passed through a field-free space in nitrogen, emerged from a narrow slit. The beam was then deflected

through about 127 degrees by the field between two electrodes forming part of a cylindrical condenser. Thus the electron beam could be made to travel through a considerable distance in the gas before entering the deflecting chamber, without coming under the influence of stray fields. The deflecting chamber was evacuated by fast pumps. On its passage through the deflecting field the beam could be automatically refocused upon a second slit, through which it passed to a collecting cylinder. If the initial velocity of the electron beam corresponded to a potential fall of V_0 volts, the beam would be refocused at the second slit only if the potential difference V^1 between the two electrodes was given by $V^1 = 2 \cdot V \log r_2/r_1$, where r_1 and r_2 are the radii of the two electrodes. For small energy losses an auxiliary potential difference was used to accelerate the electrons after they had traversed the gas, *i.e.* just before they reached the first slit, so that by varying this potential difference it was possible to direct electrons which had lost any amount of energy into the collector and thus obtain an immediate measure of their energy loss. On plotting the current received by the collector against the energy loss, a sharp peak was always observed at about 12·8 volts.

In addition there were several smaller maxima at 10·9, 13·9, 15·8 and 18·0 volts, superimposed upon a continuous distribution of electrons. The latter was presumably related to the small maximum on the side of the 12·8-volt maximum corresponding to higher energy losses. A maximum at about 6·7 volts was shown to be due to impacts of the electrons with the electrodes. The maxima at 15·8 and 18·0 volts could not be correlated with any spectroscopic levels known at present. No maximum corresponding to the first ionisation potential of nitrogen was found, but it appeared likely that the continuous electron distribution on the side of the 15·8-volt maximum corresponding to larger energy losses was due to electron collisions involving single ionisation of the molecule, which would mean that the emitted electrons had a velocity greater than zero. By a more direct method of measurement no energy loss corresponding to nitrogen K radiation, expected at about 400 volts, was found.

BIOPHYSICAL CHEMISTRY. By Victor Cofman, D.Sc., King's College, London.

Structure in Liquids.—Much interest is attached to Hauser's experiments on the cessation of the Brownian movement of certain ultramicroscopic particles (*Kolloid Zeit.*, **48**, 1929, 57). A dilute suspension (1 per 1,000) of purified "bentonite" clay seen under the ultramicroscope shows irregular movements of translation and rotation (scintillation) of the particles. On

adding a little potassium chloride, about 0·2 per cent., the Brownian movement, and finally the scintillation, is stopped.

The immobilisation of the suspended particles can also be inferred from observations with the naked eye. If one moves one's finger through the above-mentioned dilute suspension of clay (after the addition of potassium chloride), a clear streak remains behind; the particles have lost their power of diffusion; one can "write" in the liquid, and the letters will last for hours.

The diameter of the suspended particles of clay is about 0·1μ, and their distance apart several microns. What are the forces that immobilise them? Hauser suggests orientation of water molecules around the electrically charged particles, the orientation extending over many rows of molecules.

The Mitotic Structure of Protoplasm.—The fact that systems which are as fluid as water can have microscopic rigidity and definite structure clears up a rather puzzling question which was discussed at the recent Faraday Society's Symposium. It is known that during the process of cell division the chromosomes and surrounding cytoplasm show a very definite structural arrangement; yet it is during this period that the protoplasm has its greatest fluidity, which is, according to some investigators, not very different from that of water. Until now, this seemed rather paradoxical, but Hauser's experiments show that great fluidity is not incompatible with fixity of position of suspended particles.

Thixotropism.—The phenomena described above are closely connected with "thixotropism." A 10 per cent. mixture of purified bentonite clay in water is of liquid consistence; in the presence of a trace of potassium chloride it becomes a gel; it can be readily reliquefied on shaking, but "sets" again on standing. This sol-gel transformation has been christened "thixotropism." It has been observed and described in several substances, and apparently plays an important part in living systems.

The Consistence of Matter.—When dealing with cases such as we have just described the traditional classification of bodies into solids, liquids, and gases is no longer sufficient. The study of colloids has brought to our knowledge many intermediate states, systems which possess some, but not all, the properties of solidity (hardness, elasticity, crystallinity). One hears of brittle liquids (starch with water, quicksands), "crystalline" gases (according to von Weimarn), and of solids that flow (thixotropism). The study of the "flow of matter" is fast developing into a specialised branch of science. It has already its own periodical, *The Journal of Rheology*, and the yearly meeting of rheologists at Lafayette College (Easton, Pa.)

brings together chemists, physicists, biologists, as well as men from the industry.

The Chemical, the Physical, and the Colloidal " Molecule."—The difficulty of fitting new facts into old theories and classifications is one of the constantly recurring problems of science. Even the concept of molecule, which seemed solidly built, shows signs of giving way under the weight and the variety of phenomena which it has been called upon to explain.

The chemist's molecule was derived primarily as a shorthand notation for the chemical properties of substances. It explained at the same time many physical properties like gas and osmotic pressure, electrochemical equivalence, lowering of freezing-point and of vapour pressure, optical activity, etc.

It seemed natural to attempt an extension of the molecular concept and to correlate the size, shape, or other *calculated* properties with experimental measurements on viscosity, X-ray diffraction, surface films, and with spectroscopic data. Attempts in this direction have been to a great extent successful, but they have also led to a certain haziness in the definition of the term " molecule." This indefiniteness has been the source of many controversies.

The structure, molecular weight, and size of large " polymerised " molecules (cellulose, rubber, gelatine) has formed the subject of animated discussions at recent chemical gatherings in Germany.[1] Staudinger has shown that, in the case of polystyrols, the specific viscosity of their solutions is directly proportional to their molecular weight ; hence he has calculated the molecular weight of rubber from the viscosity of its solutions, assuming the same simple relation between viscosity and molecular weight.

Other workers, represented by Meyer and Mark, object with reason that viscosity is a complex property, and can seldom be used as an index of molecular size. Relying upon evidence largely derived from X-ray spectography, they postulate in the constituent particles of rubber and cellulose, apart from the orthodox valence bonds, other less well-defined physical, or physico-chemical, forces.

A similar discussion, whether protein solutions are made up of " molecules " or of " micells," enlivened the proceedings of the recent Symposium of the Faraday Society at Cambridge (October 1930). Prof. Svedberg, whose development of the ultra-centrifuge has earned him the Nobel Prize, has devoted much time to the determination of the particle size (" molecular weight ") of various proteins by means of that apparatus.

Swedberg and co-workers divide the proteins into two groups : the members of the first group have molecular weights

[1] *Kolloid Zeit.*, **53**, pp. 19, 30 (October 1930).

which are simple multiples of 34·500 (*e.g.* Ovoalbumin, 34·500 ; Serum albumin, 67·500 ; Serum globulin, 103·800 ; Legumin, 208·000).

The second group is composed of proteins whose " molecular weight " runs into millions (*e.g.* H-Hæmocyanin of molecular weight 5,000,000).

The only argument for considering that the constituent particles of a protein dispersion are molecules appears to be the fact that they are of equal size. They do not show constant combining ratio or electrochemical equivalence.

Kruyt takes the point of view that the protein particles must be looked upon as micells in which the colloid-physical properties, rather than the chemical behaviour, play a predominant part.

One's conclusion whether a given system is, or is not, molecularly dispersed, will depend upon the property which is made the criterion of molecularity. A diamond is a single molecule from the standpoint of the X-ray spectroscopist. From the same point of view the particles in a gold sol would also be " molecules," though of varying size.

From spectroscopic observations in the infra-red, visible, and ultra-violet spectra, the physicists have begun to speak of OH and CH groups as molecules (*cf.* SCIENCE PROGRESS, **24**, April 1930, p. 577). Evidently physical molecules are not always the same things as chemical molecules. Provided we bear this in mind there is no objection to the extension of the meaning of the word " molecule."

GEOLOGY. By G. W. TYRRELL, A.R.C.Sc., Ph.D., University, Glasgow.

Petrology of the Igneous Rocks.—In his paper on " Rock Glass, and the Solid and Liquid States," Dr. L. Hawkes (*Geol. Mag.*, LXVII, 1930, pp. 17–24) has done petrologists a useful service in defining and elucidating the states of matter which chiefly concern their work. He clears up current confusions as to the nature of the solid and liquid states and, in particular, disposes of the widely held view that rock-glass represents enormously supercooled magmatic liquid. A glass is an amorphous solid, the state of aggregation of which, although it is practically unknown, is at any rate fundamentally different from that of a liquid.

Continuing their valuable work on the elastic properties of minerals and rocks, L. H. Adams and R. E. Gibson (*Proc. Nat. Acad. Sci.*, **15**, 1929, pp. 713–24) have now measured the cubic compressibility of certain basic minerals and rocks, namely, labradorite, jadeite, grossularite, almandite, and diabase from three widely separated localities. They find that,

at 15,000 megabaryes and 30° C., the maximum V. of longitudinal waves through basaltic rocks is 7·4 km. per sec. The compressibility of garnet and jadeite was found to be surprisingly low, and rocks crystallised from magmas of corresponding composition, or altered to eclogites, may transmit longitudinal waves with V. exceeding 8 km. per sec. "The existing data . . . place the possible components of the earth below 60 kms. and above the core, in the following ascending order of probability: holocrystalline basalt, eclogite, peridotite."

The system, leucite-diopside, according to N. L. Bowen (*Amer. Journ. Sci.*, XVIII, 1929, pp. 301–12), is a simple eutectic, with 61·5 per cent. diopside and temperature 1300° C. ± 2°. A mixture of diopside and leucite may be regarded as the simplest form of the rock *leucitite*; natural leucitites are more complex, and will be completely liquid at much lower temperatures. The inversion of leucite, and the straining of the crystals which is thus set up, may be a material aid to the formation of that intergrowth of orthoclase and nepheline which is known as pseudo-leucite.

A physico-chemical investigation of acmite, the soda-pyroxene which is a common constituent of alkali-granites and syenites, has been carried out by N. L. Bowen and J. F. Schairer (*Amer. Journ. Sci.*, XVIII, 1929, pp. 365–74). This mineral melts incongruently at 990° C. with the separation of hæmatite. On the cooling of a liquid of acmitic composition, hæmatite separates out, but reacts with the liquid at 990° C. to form acmite. If the reaction is incomplete, bipyramidal quartz separates from the liquid, in which sodium disilicate accumulates. It is suggested that the excess of sodium silicate which is frequently found in rocks carrying acmite and related soda-amphiboles is the result of crystal fractionation controlled by the incongruent melting of acmite.

In a further discussion of a system in which acmite, hæmatite, and quartz are involved, namely, the ternary system—$Na_2SiO_3 - Fe_2O_3 - SiO_2$,—N. L. Bowen, J. F. Schairer, and H. W. V. Williams (*Amer. Journ. Sci.*, XX, 1930, pp. 405–55) show that fractional crystallisation may follow different courses, and give rise to alternative residual magmas of quite different composition, according to the conditions of cooling. Hæmatite may or may not be found in the final product, and with extreme fractionation its place may be taken by free silica as quartz. Hence the production of an iron-rich residual liquid during the crystallisation of basaltic magma under certain conditions is no bar to the production of a siliceo-alkalic residual liquid (granitic or syenitic) under other conditions (*see* next paragraph).

In an important paper on the crystallisation of basalts Dr. C. N. Fenner (*Amer. Journ. Sci.*, XVIII, 1929, pp. 225–53) points out that the generally accepted scheme for the fractional crystallisation of basaltic magma, leading to the concentration of siliceo-alkaline residues (granitic and syenitic), should also lead, according to the same principles, to the concentration of iron relatively to magnesium. He quotes several examples of a smaller MgO : FeO ratio in igneous rocks as a whole than in the pyroxenes of early crystallisation which they contain. In a number of published examples the residual liquid from the crystallisation of a basalt is shown to be highly ferriferous, and to be capable of depositing magnetite, pyroxene, and feldspar. Dr. Fenner consequently believes that fractional crystallisation according to Bowen's scheme is not the whole story of differentiation, but that other processes may co-operate with it, or entirely overshadow it.

Dr. F. Walker's recent work on a tholeiitic phase of the quartz-dolerite magma of Central Scotland (*Min. Mag.*, XXII, 1930, pp. 368–76) has a bearing on Fenner's contentions. These rocks have a dark glassy base, which might be expected, on Fenner's views, to have a ferriferous composition. Refractive index and statistical methods, however, indicate that this glass is highly acid, and probably has a silica percentage of from 75–80. Some iron may have been abstracted from the basaltic residues by the late crystallisation of ferriferous minerals such as chlorophæite. Had the glass proceeded to crystallisation, it would doubtless have formed the interstitial micro-pegmatite which is so characteristic of the British Permo-Carboniferous quartz-dolerites.

The contents of the first half of the third part of J. H. L. Vogt's comprehensive memoir on "The Physical Chemistry of the Magmatic Differentiation of Igneous Rocks" (*Norske Videnskaps.-Akad. Oslo. I. Math.-Nat. Klasse*, 1929, No. 6, 131 pp.) can only be summarised by listing the sectional headings as follows: on the small rest-magma (= residual magma) dikes, approaching the granitic eutectic; on granitic rest-magma dikes within gabbros; on proto-enriched and on rest-magma-enriched dikes derived from lardalite, etc.; on complementary dikes; on dikes differentiated *in situ*; on the term "co-tectic line" instead of "eutectic line." The foregoing topics occupy sixty-four pages; the rest of the memoir is devoted to the study of the syenitic rocks, in which are included not only the syenites proper, but also the hypabyssal and volcanic members of the family.

In his valuable discussion of the significance of the word "eutectic," Dr. C. N. Fenner (*Journ. Geol.*, XXXVIII, 1930, pp. 159–65) shows that this term has been used in various

ways, with confusing results, especially that more or less incomplete analogies have been drawn between the final stages of the crystallisation of natural magmas, and those of laboratory melts. He suggests a return to the original use of the word, namely, to indicate a certain unique point on a crystallisation-diagram, and to use the terms "boundary lines" and "boundary surfaces" instead of "eutectic lines" and "eutectic surfaces," for the appropriate geometrical elements of the diagram (*see also* previous paragraph).

Two papers, by E. Sampson and C. S. Ross respectively, with a discussion by J. T. Singewald (*Econ. Geol.*, XXIV, 1929, pp. 632–49), deal with the question whether chromite is not sometimes a late magmatic product, or even produced by hydrothermal action. The question is answered in the affirmative; for although much chromite crystallises at an early magmatic stage, various lines of evidence indicate that a substantial amount of the mineral passes into a residual solution, and even into highly mobile aqueous solutions capable of considerable migration from the magmatic source.

In a suggestive and destructive criticism of theories of the origin of feldspathoidal igneous rocks, Prof. S. J. Shand (*Geol. Mag.*, LXVII, 1930, pp. 415–27) comes back to Daly's original "limestone-syntectic" theory, in which the origin of alkalic rocks is ascribed to the desilication of ordinary sub-alkaline magmas by the assimilation of limestone. It is fairly certain that typical ijolites, malignites, jacupirangites, shonkinites, pyroxenites, and other lime-rich types characteristically found in association with alkalic rocks, are formed in this way. Prof. Shand discusses the geological evidence in favour of the view that the lime-poor alkalic rocks, such as the foyaites, have also originated by this process.

In his recent discussion of the spilitic rocks, N. Sundius (*Geol. Mag.*, LXVII, 1930, pp. 1–17) compares the chemical composition of rocks of this group with that of corresponding rocks such as basalt, andesite, etc., in other groups. He finds that the spilites are distinctly separated from other petrographical groups by a great deficiency in potash, by a relative deficiency in alumina, and by an excess of ferrous iron and titania. As the textural characteristics of these rocks imply the crystallisation of plagioclase before pyroxene, it was believed that a change in composition towards the albitic end of the plagioclase series had occurred since the original crystallisation of the mineral. Dr. Sundius shows, however, that the assumption of an autometamorphic change of an early lime-feldspar to explain the formation of albite is unnecessary. The high ferrous iron content causes the "eutectic line" for plagioclase and pyroxene to be reached at an early stage in these rocks.

In the second part of his "Ausländische Systematik, Klassifikation, und Nomenklatur der Magmengesteine" (*Fortschr. Min. Kryst. Petr.*, **13**, 1929, pp. 235–311), Prof. K. H. Scheumann gives useful synopses of the classificatory systems of Hodge and Shand, and a further instalment of his glossary of new igneous rock names.

Prof. F. Loewinson-Lessing has applied his well-known statistical methods to analyses of liparites and dacites, with a view to the delimitation of these groups by means of the "acidity-coefficient" and other factors. The paper is in English (*C.R. Acad. Sci. U.R.S.S.*, 1930, pp. 179–84).

In a study of Pacific basalts, Dr. T. F. W. Barth notes that pure feldspathic basalts devoid of nepheline, and usually with olivine, often show a considerable amount of normative nepheline (*Journ. Wash. Acad. Sci.*, **20**, 1930, pp. 60–8). He has arrived at the conclusion that much of what appears to be normal plagioclase in these rocks is in reality the mineral anemousite, which is a plagioclase with carnegieite (the nepheline molecule in triclinic form) in solid solution. Barth proposes the name *pacificite* for these anemousite-basalts. In a described example the anemousite apparently occurs as an interstitial ground-mass to laths and phenocrysts of normal labradorite.

An exhaustive account of the geology and petrography of the albite-nepheline-syenite known as *mariupolite* is given by Prof. J. Morozewicz (*Min. u. Petr. Mitt.*, **40**, 1930, pp. 335–436), who first described and named this rare and interesting rock type. It comes from a series of obscure intrusions into the ancient crystalline rocks of the Azov block in the south of Russia. Foyaite, alkali-syenite, and pyroxenite are comagmatic with mariupolite. The memoir contains valuable discussions of differentiation, and of other regions containing comparable rocks.

The dikes and associated intrusions of Bute have been described by Dr. H. J. W. Brown (*Trans. Geol. Soc. Glasgow*, XVIII, pt. 3, 1929, pp. 388–419). The majority of the intrusions are of Kainozoic age, although examples of Devonian, Carboniferous, and Permo-Carboniferous ages are described. Carboniferous dikes are common in the southern half of the island. The Kainozoic dikes are dealt with under the headings: Brunton type of tholeiite, olivine-tholeiites of Brunton type, tholeiite (Salen type), tholeiite (Corrie type), tholeiite (Largs type), tholeiite (Cumbrae type), quartz-dolerites. New chemical analyses of a quartz-dolerite and of an olivine-tholeiite of Salen type are given.

Some remarkably interesting petrographical results have accrued from the recent re-mapping of Central Ayrshire by

the Geological Survey, and these have been published in brief by V. A. Eyles, J. B. Simpson, and A. G. MacGregor (*Trans. Geol. Soc. Glasgow*, XVIII, pt. 3, 1929, pp. 361–87). Igneous rocks of Lower Old Red Sandstone, Calciferous Sandstone, Millstone Grit, and Permian periods, are dealt with. Chief interest attaches to the rocks of the last-named period. They are divided into three groups: (1) the kylite suite, including essexite, olivinic theralite-dolerite, and picrite; (2) the teschenite and alkali-dolerite suite, including subgroups of the true teschenites, ophitic alkali-dolerite with veins of analcite-syenite, and ophitic olivine-dolerite with biotite; and (3) decomposed olivine-dolerites, occasionally carrying quartz introduced by juvenile gas agencies. The less alkaline types seem to be earlier in time than the highly alkaline teschenites and kylites.

The last volume of the *Transactions of the Edinburgh Geological Society* contains a remarkable *tour de force* by the veteran amateur petrologist, Mr. T. Cuthbert Day (XII, pt. 2, 1930). Two-thirds of the volume are occupied by eight papers from his pen, in which are recorded no fewer than forty-eight new chemical analyses, mostly of the Carboniferous igneous rocks of the Lothians. This accession of new data will prove of immense value in the study of the Scottish Carboniferous petrographical province. The titles listed below will serve to illustrate the range and contents of Mr. Day's work: "Chemical Analyses of White Trap from Dalmeny, Granton, Weak Law, and North Berwick," pp. 189–94; "Volcanic Vents on the Coast from Tantallon Castle eastwards to Peffer Sands, and at Whitberry Point," pp. 213–33; "Chemical Analyses of Phonolites from Traprain Law, the Bass Rock, and North Berwick Law," p. 234; (with A. G. Stenhouse) "Notes on the Inchcolm Anticline," pp. 236–51; "Two Large Xenoliths within the Phonolite of Traprain Law," pp. 252–5; "An Igneous Dike in the Quartz-banakite of Bangly Quarry, near Haddington," pp. 256–9; "The Intrusive Rock of Frances Craig and the Teschenite of Ravensheugh," pp. 260–2; "Chemical Analyses of Thirteen Igneous Rocks of East Lothian," pp. 263–6."

Four "felstone" intrusions in Central Berwickshire are described by J. Irving (*Geol. Mag.*, LXVII, 1930, pp. 529–41). Dirrington Great Law, Dirrington Little Law, and Blacksmill Hill consist of riebeckite-felsite closely allied to the well-known rock of the neighbouring Eildon Hills, and hence probably of Carboniferous age. The fourth mass, Kyleshill, is an orthophyre, with andesite, whose geological relationships are obscure. These intrusions may represent the denuded remnants of a laccolith which has been injected into the conglomerates of the Upper Old Red Sandstone.

The very interesting variations in petrographical character and specific gravity of rocks collected at regular intervals from a boring which penetrated the greater part (224 feet) of the Easter Dalmeny (Midlothian) teschenite sill, are described by Sir J. S. Flett (*Summ. Prog. Geol. Surv. for* 1929, pt. 3, 1930, pp. 59–74). As usual, picrites and picro-teschenites, three bands of each, are found below the median plane of the sill, and are bordered at top and bottom by normal teschenite, the upper band passing upwards into a strongly analcitised variety. Sir J. S. Flett believes that the absence of alkali-feldspar from the picrites is due to upward gaseous transfer of alkali-silicates, whilst the bytownite and olivine are due to gravitative settling. The bytownite, however, may represent sunken lime-feldspars which have been dissolved and re-crystallised in the lower and hotter part of the sill. The major banding which the sill displays may indicate the injection of new differentiated magma, or fluxion movements in a partly differentiated mass.

In describing the geology of the Shiant Isles (Hebrides), Dr. F. Walker has made a remarkable contribution to the theory of gravitative differentiation (*Quart. Journ. Geol. Soc.*, LXXXVI, 1930, pp. 355–98). The islands consist mainly of a great sill of crinanite, with a picritic or even dunitic modification towards the lower margin, which has been produced by the subsidence of olivine crystals. The sill rests upon Upper Liassic shales. Thick bands of alkaline zeolitised pegmatite, which contain large pseudomorphs after nepheline, occur in the picrite. These are believed to have been produced by pressure on a crystal-mesh. The lower sill of Eilean Mhuire contains an upper layer of analcite-syenite, which is attributed to some mode of auto-intrusion by alkaline residual magma.

Dr. J. A. Smythe's memoir on " A Chemical Study of the Whin Sill " (*Trans. Nat. Hist. Soc. Northumberland, Durham, and Newcastle-on-Tyne* (N.S.), VII, pt. 1, 1930, pp. 16–150) is perhaps the most detailed study of any one igneous mass that has ever been published. It records the results of the investigation of no fewer than 2,950 samples collected from the whole field of exposure of this famous sill. Many specific gravity determinations have been made; and two analyses of composite samples, with ten analyses of individual rocks, have been executed for this research. Dr. Smythe has studied the chemical and mineral composition of the rocks ; the chemical composition of the individual minerals ; the composition of the micropegmatite ; the process of differentiation ; the connection between chemical composition and specific gravity ; the whin dikes associated with, and the basalts intruded into, the sill ; the various alterations due to chloritisation, pectoliti-

sation, and vein solutions; and the problems of assimilation and emplacement. The last-mentioned problem is dealt with in an original manner, and Dr. Smythe has shown how variations in the incidence of superincumbent pressure have caused variations in the mineral composition from place to place.

The so-called "syenite" of Hestercombe, Somerset, is now shown to be diorite and quartz-diorite by E. D. Evens and F. S. Wallis (*Geol. Mag.*, LXVII, 1930, pp. 193–9). The intrusion is dike-like at one place, and sill-like at another. It is regarded as a marginal extension and modification of the Dartmoor granite.

After describing the "heavy mineral" suites of the granites of Northern Brittany, the Channel Islands, and the Cotentin, in order to assist sedimentary petrographers on questions of provenance, especially in regard to South of England sediments, Dr. A. W. Groves (*Geol. Mag.*, LXVII, 1930, pp. 199–218) gives a valuable discussion of the validity of the method of "heavy mineral" suites in the correlation of granite masses. He concludes that the heavy minerals of normal granite give a fair representation of the content of minor constituents in the original magma, but great care must be taken to select material uncontaminated by country rocks.

BOTANY By Prof. E. J. Salisbury, D.Sc., F.L.S., University College, London.

Œcology.—A recent paper by W. O. Robinson (*Soil Science*, September 1930) is of considerable interest in relation to the well-known toxicity of bog waters. He finds that these contain a high proportion of soluble iron and manganese. Experiments with a considerable variety of soils showed that, when submerged, these exhibited an increase of soluble iron and manganese which in the case of a Carrington loam soil from Iowa rose from 0·4 parts per million after one day's submergence, to 40 parts per million after sixty days. Indirectly this is attributed to soil organisms which provide the carbon dioxide that maintains the iron and manganese in solution in the form of proto-bicarbonates. The presence of organic matter is essential to the phenomenon, and the most responsive soils were found to be those having the highest proportion of organic material. Most of the submerged soils were observed to give off gas, and analyses showed that this was mainly methane which varied in the ten soils studied, from 47 to 66·8 per cent. with one exception, where this gas only constituted 16·2 per cent. of the total evolved. Three of the samples exhibited evolution of carbon monoxide (2·8–3·2 per cent.).

Zimmerman (*Amer. Journ. Bot.*, pp. 842–61, 1930) finds

that cuttings of *Salix pendula* will exhibit root growth in water with an oxygen content as low as 0·5 part per million, whilst normal growth of roots took place when the oxygen exceeded nineteen parts per million. *Hedera helix* showed slight root growth in only 0·00018 per cent. oxygen, and normal growth in 0·002 per cent. Tomato roots grew in water with 0·0002 per cent. of oxygen. In non-aerated water hypertrophied lenticels were developed but decreased with root development.

Eriophorum opacum (Bjornstr.) Fernald, a species hitherto known only from Canada, Northern Newfoundland, Scandinavia, Russia, Siberia, and Alaska, has been found by Fernald on the northern slopes of Ben Eigh in Scotland. The same author also records *Scirpus hudsonianus* Fernald (= *Eriophorum alpinum* L.), a species supposed to be extinct in Britain, from the same area (*Journ. Bot.*, January 1931).

The height and earliness of thirty-one species of flowering plants has been studied by Turesson on material collected from a considerable geographical area in Europe (*Hereditas*, xiv, 2, 1930). Earliness in summer flowering species appears to be combined with moderate height in the northern part of their range, whilst towards the south increased stature is combined with lateness in flowering. On the other hand, spring-flowering species present later-flowering biotypes in the northern part of their range. The oceanic climate of the western maritime region appears to be favourable to biotypes of low stature and late flowering in contrast with tall stature and earliness in the eastern continental climate with moderate summer precipitation. *Galeobdolon luteum* from Scandinavia had a mean height of 24·6 cm., whereas plants from Munich grown under the same conditions were 41·7 cms. *Ulmaria filipendula* from Omsk, Florence, Vienna, and Munich showed mean respective heights of 92·2 cm., 82·5 cm., 80·8 cm., and 79·4 cm. The author concludes that climate has an important selective effect upon the biotypes of a species.

Morphology.—The Loranthaceous genus *Arceuthobium* presents some unique features with respect to the structure of the anther, namely, that the archesporium is cylindrical, surrounding a central column of sterile tissue, and the epidermis constitutes the fibrous layer. These features were first recorded for *A. oxycedri* by Heinricher and Pisek, and have now been confirmed in a second species *A. pusillum* by Thoday and Johnson (*Ann. Bot.*, October 1930), who also find that the embryo-sac forms a conspicuous haustorium subsequent to fertilisation.

In the *New Phytologist* for December Dr. Agnes Arber publishes an able discussion of morphological theories regarding root and shoot, concluding that these are not to be regarded

as separate morphological categories but as equivalent entities. Attention is called in this paper to the terminal position of leaves in some grasses, the normal and regular production of shoots from some leaves, the dorsiventral modification of axes as in the pedicels of some cruciferæ, all of which, together with the production of so-called "adventitious" shoots and roots, emphasise the artificiality of rigid distinction between root, stem, and leaf.

Smith (*Amer. Journ. Bot.*, November 1930) has investigated the contractile roots of *Brodiæa lactea*, and finds that this is due, as described by Rimbach for other monocotyledons, to a longitudinal contraction and horizontal expansion of the inner cortical cells. The outstanding features of this species are that these "active" cells finally undergo complete collapse, and that the shortening may be so marked that the stele is greatly distorted. This distortion emphasises the passive part played by the central cylinder in the contraction process.

Anatomy.—The primitive gamopetalous corolla has, according to Kock, fifteen bundles, namely, a mid-vein to each corolla lobe and two marginal veins. Such a condition is met with in *Hamelia patens* (Rubiaceæ). In the compositæ the more primitive types of corolla (*e.g. Helianthus divaricatus*) exhibit a fusion of adjacent marginal strands in their lower region, so that ten strands are present, of which, however, those corresponding to the sinuses fork and follow the margins of the corolla lobes. The compositæ show varying degrees of reduction from this condition to disk florets in which only five bundles persist, representing the basal portions of the fused marginal strands, which end at the sinuses. Further reduction is met with in the ray florets, where extreme reduction is found in *Tussilago farfara.* Here, although four bundles constitute the floral supply, only two—or, more usually, one—persists in the corolla. The "split" in the ligulate corolla corresponds to a sinus, and the bundle in this position ends in the ovary wall (Kock, *Amer. Journ. Bot.*, November 1930).

H. M. Chute (*Amer. Journ. Bot.*, October) has studied the anatomy of various achenes, and finds that the achene of *Ranunculus acris* has a five-trace system, of which two are ventral and three dorsal. The ovule is supplied by a branch from one of the ventral strands. In *Ficaria verna* the ovule possesses only three strands, *viz.* two ventral and one dorsal, and whereas in most achenes the carpel trace is a single bundle, here its triple nature is evident, thus recalling the three-trace system characteristic of follicles. In *R. repens* and *R. bulbosus* the ventral strands continue with the dorsal into the style. In *R. pennsylvanicus* the dorsal laterals are sometimes, but not always, absent, whilst in *R. sceleratus* and *R. flammula* these

strands are absent and the ventral strands fuse with the median dorsal. In *R. longirostris* and *R. aquatilis* both ventral strands and the two dorsal laterals are absent, so that there is but a single trace from which the ovular supply originates. The achenes of *Waldsteinia geoides* have a three-strand carpellary trace, as have many follicles.

Cytology.—The cytology of various Amentiferæ has been studied by Woodworth (*Bot. Gaz.*, 1929–30, and *Amer. Journ. Bot.*, November 1930). In the Betulaceæ two series are recognised. Those genera in which the fundamental number of chromosomes is fourteen are *Betula*, *Alnus*, *Corylus*, whilst in *Carpinus*, *Ostrya*, and *Ostryopsis* the fundamental number is eight. Recent investigation of the Juglandaceæ reveals sixteen as the normal haploid number in *Juglaus* and *Carya*, although *C. alba*, *C. glabra*, and *C. ovalis* are tetraploid species, with thirty-two chromosomes as the haploid number. A relationship between the Juglandaceæ with the *Betula-Alnus* series of the Betulaceæ is suggested.

H. G. Brunn gives a review of the chromosome numbers in the genus *Primula*, in which a large number of species are considered. In general the chromosome numbers are uniform for members of the same section or subsection of the genus, and thus correspond in general to morphological features. The greatest variation appears to be in the subsection Eu-farinosæ, where diploid numbers of 18, 36, 54, 72, and 126 all occur. A diploid number of 22 is most frequent, but in the sections Inayatii, Souliei, Stenocalyx, and Glabræ, the number is 16, whilst 18 chromosomes occur in the Verticillatæ, Mala-coides, Capitatæ, Eufarinosæ (p.p.), and Sibiricæ (p.p.). Other diploid numbers recorded are 20 and 40, both of which occur in the Muscarioides; 24 in Septemlobæ and Bullatæ; and 44 in various sections (*Svensk Bot. Tids.*, **24**, 3, 1930).

Interesting results have been obtained by R. C. Malhotra (*Journ. Genetics*, vol. 23, No 2, 1930), from which it would appear that the percentage of staminate plants in the offspring of *Asparagus* is dependent upon the presence and amount of volatile oil in the pollen. By pollinating with pollen which has been sprayed with volatile oil, an increase of 26·1 per cent. ± 1 can be artificially induced in the number of staminate plants.

Cryptogams.—In an account of the species of *Phomopsis* occurring on Conifers, G. G. Hahn records *Phomopsis occulta* as the imperfect stage of *Diaporthe conorum*. This species occurs on no less than fourteen host genera, and has a wide geographical range. In contrast *Phomopsis abietina* is restricted to France and Germany, and only attacks *Abies pectinata*. Six other species are distinguished, of which two are new, *viz. P. mon-*

tanensis, which attacks *Abies lasiocarpa*, and *P. boycei*, apparently confined to *Abies grandis* (*Trans. Br. Myc. Soc.*, vol. xv, 1 and 2, 1930). In the same number J. H. Millar treats of the British Xylariaceæ, and T. B. Auret describes the endophytic fungus of *Lunularia*, which is identified as a species of *Phoma*.

Economics.—In the *Lingnan Science Journal*, vol. 9, No. 3, F. A. McClure gives a useful account of the cultivation in China of *Aleurites fordii* and *A. montana* for the production of Tung Oil, of which some $7,765,645 worth was exported in 1930 to the West for use in the manufacture of varnishes, paints, and linoleum. The cultivation is a long rotation cycle occupying 43–5 years, of which about 20 years only is actually utilised in the cultivation of the Tung crop, and nearly the same period in recolonisation by natural vegetation, which is then burnt, a corn crop grown for two or three years, followed by Tung.

PLANT PHYSIOLOGY. By Prof. J. C. Walter Stiles, Sc.D., F.R.S., The University, Birmingham.

Photosynthesis.—Some years ago Lundegårdh described experiments which suggested that the rate of photosynthesis does not always increase regularly with temperature as is usually supposed, but that in weak light with various concentrations of carbon dioxide, and in strong light with low carbon dioxide concentration, photosynthesis with increasing temperature reaches a maximum value at about 10° C., falls above this to rise again to a second maximum at about 20° C., and then falls again, rising to a third maximum at approximately 30° C. More recently O. A. Walther, working in Lundegårdh's laboratory, has recorded somewhat similar maxima in the case of the relation between temperature and photosynthesis of leaves of *Vicia Faba*. ("Zur Temperaturabhängigkeit der Assimilation bei Vicia Faba," *Flora*, **121**, 301–15, 1927.) Using atmospheric air as the source of carbon dioxide he found with low light intensity, that of a 50 candle-power lamp at about 8 cm. distance, an assimilation maximum at 6° C., a minimum at 7·6° C., and a second maximum from 10° to 20° C. With sunlight maxima were observed at about 9° C. and 18° C. In considering the value of these results it should be noted that the amounts of assimilation actually measured were very small, varying in the experiment with electric light between 0·000146 gm. and 0·000524 gm. The actual amounts of assimilation measured for the maximum at 6° C. and the minimum at 7·6° C. were respectively 0·000261 gm. and 0·000313 gm., corresponding to 0·00480 and 0·00357 mg. of carbon dioxide per 50 sq. cm. per hour. Having regard to the extremely small quantities

of carbon dioxide determined and to the small number of determinations made, combined with the known variability of plant tissue, it would seem that some confirmation of these alleged maxima and minima is desirable before the usually accepted view of the relation between temperature and photosynthesis, based on a considerable amount of careful work, can be regarded as incorrect.

Indeed, T. H. Van der Honert, in an investigation of the interaction of external factors in photosynthesis by the filamentous terrestrial alga *Hormidium* ("Carbon Dioxide Assimilation and Limiting Factors," *Rec. Trav. bot. néerlandais*, **27**, 149–286, 1930), found that the rate of assimilation increased continuously from 12° to 24° C. when high light intensity was employed. With low light intensity temperature has little effect, the measured assimilation at 20° being actually a little less than at 12°. Van der Honert's work was aimed at determining firstly the relation in the individual plastid between rate of photosynthesis on the one hand and the various external factors (carbon dioxide concentration, light intensity, and temperature) on the other, and secondly, the nature of the individual reactions which determine the rate of the whole photosynthetic process. In this work the relation between each of the external factors and photosynthesis was determined with different known values of the other factors. It was found that each factor might determine the assimilation velocity, limiting it in Blackman's sense. The deviations from Blackman's well-known scheme were very slight and could be ascribed to the fact that the various chloroplasts in a plant are subjected to different conditions in regard to carbon dioxide and light. Van der Honert showed that his results agree well with the view that photosynthesis is a chain of reactions consisting of (1) a diffusion process, (2) a photochemical reaction, and (3) a dark chemical reaction (the Blackman reaction). Each one of these may limit the rate of the whole assimilatory process. Thus if carbon dioxide concentration in the external medium is low, the rate of assimilation is limited by the rate at which carbon dioxide diffuses to the chloroplast. This rate is unaffected by light, and practically unaffected by temperature, for increase in the latter increases the rate of diffusion but reduces the solubility of carbon dioxide in the aqueous medium through which it diffuses to the chloroplast. Consequently, with low carbon dioxide concentrations the relationship between this factor and rate of assimilation is linear and independent of light intensity and temperature.

When light intensity is low the photochemical reaction will determine the rate of assimilation. This process is practically uninfluenced by temperature. It is also very nearly independent

of carbon dioxide concentration. This is attributed by Van der Honert to the assimilating agent, presumably chlorophyll, possessing a very great affinity for carbon dioxide so that it is able to saturate itself with carbon dioxide at very low external pressures of carbon dioxide. Since the rate of the photochemical process will be proportional to the quantity of the assumed carbon dioxide-chlorophyll compound formed at the surface of the chloroplast, this rate will be independent of the external carbon dioxide concentration when the breaking down of the addition compound is slow. With more rapid breaking down at higher light intensities carbon dioxide may become limiting, since it can only be slowly replaced by diffusion.

Lastly, when temperature is the limiting factor, that is with sufficiently high light intensity and carbon dioxide concentration, the temperature determines the rate of assimilation, this being increased 1·87 times for a rise of 10° C. But as already pointed out, when light or carbon dioxide is limiting, temperature has practically no effect.

Thus the three external factors, carbon dioxide, light intensity, and temperature can limit the whole of the assimilatory process through limiting the rate of, respectively, the diffusion stage, the photochemical stage, and the Blackman reaction.

A further step towards the elucidation of the stages in the photosynthetic process has been taken by R. Emerson, who has investigated the influence of chlorophyll content on rate of photosynthesis of the green alga *Chlorella*. In the first of two papers ("The Relation Between Maximum Rate of Photosynthesis and Concentration of Chlorophyll," *Journ. Gen. Physiol.*, **12**, 609–22, 1929) he described how, by varying the iron content of a nutrient medium containing glucose, healthy *Chlorella* cells varying in chlorophyll content may be obtained. The rate of photosynthesis of such cells was measured and the chlorophyll content determined spectrophotometrically. Over a range of chlorophyll concentrations from 0·018 to 0·107 measured in arbitrary units, rate of assimilation increases regularly with increased chlorophyll content, the carbon dioxide concentration and light intensity both being high and the temperature apparently about 20° C. The relationship is not quite linear, the assimilation increasing more slowly with chlorophyll content than it would were the relation linear. With different cultures, which might differ in respect of age, for example, the rates of assimilation with the same chlorophyll content, can differ, so that other internal conditions may affect the rate of the whole assimilatory process.

In a second paper ("Photosynthesis as a Function of Light Intensity and of Temperature with Different Concentrations of Chlorophyll," *Journ. Gen. Physiol.*, **12**, 623–39, 1929) the

same author recorded the results of experiments in which the rate of photosynthesis of samples of *Chlorella* with different chlorophyll content was measured at different light intensities and at different temperatures. Perhaps the most interesting of Emerson's conclusions is that the effect of temperature on the rate of photosynthesis is the same whatever the concentration of chlorophyll within the range of concentrations examined. Now Willstätter and Stoll had found that in leaves poor in chlorophyll the rate of photosynthesis is less affected by temperature than in leaves rich in the pigment, and this was explained on the ground that in the chlorophyll-poor leaves the photochemical reaction, practically unaffected by temperature, limits the rate of the whole process because of the low concentration of chlorophyll, the pigment being involved in this reaction. In chlorophyll-rich leaves the pigment is present in relative excess and it is therefore not the photochemical stage but the dark Blackman reaction, with a marked temperature coefficient, which limits the rate of photosynthesis. But now Emerson has found that lowering the chlorophyll content of *Chlorella* does not eliminate the effect of temperature, and he therefore supposes that chlorophyll must play a part in the Blackman reaction as well as in the photochemical reaction.

J. C. Ghosh (" Kinetik der Photosynthese bei Pflanzen," *Jahrb. f. wiss. Bot.*, **69**, 572–86, 1928) has examined mathematically the quantitative implications of Willstätter and Stoll's theory of the photosynthetic process, and has shown that the results of Harder on the photosynthesis of *Fontinalis* published ten years ago are in agreement with the theory. Values for the constants of the various reactions involved in the theory are determined for the temperature of 21·8° C. at which Harder's experiments were conducted.

E. C. Barton-Wright and M. C. Pratt (" Studies in Photosynthesis I. The Formaldehyde Hypothesis," *Biochem. Journ.*, **24**, 1210–16, 1930) have examined experimentally the claim of Klein and Werner that by the use of dimethylhydroresorcinol it can be shown that formaldehyde is produced in assimilation by water plants, and can be recognised in the liquid external to the plants. Barton-Wright and Pratt came to the conclusion that the formaldehyde is not the result of photosynthesis, but is due to the action of light on carbon dioxide and bicarbonates in the external liquid.

In a second paper (" Studies in Photosynthesis II. The First Sugar of Carbon Assimilation and the Nature of the Carbohydrates in the Narcissus Leaf," *Biochem. Journ.*, **24**, 1217–34, 1930) the same writers have recorded the results of analyses of Narcissus leaves in respect of hexose and sucrose, the leaves being collected at hourly intervals over parts of the day on

March 31, May 8, and May 20. It was found that the sucrose content rose during the day, including rainy periods, whereas the hexose content fell during the rain. Generally speaking there is through the day a continuous negative drift with time in the hexose concentration and a continuous positive drift in the sucrose concentrations. The authors treated their results statistically and came to the conclusion that hexose is the first formed sugar and that sucrose is formed later.

An investigation on similar lines to the one just mentioned has been made by H. F. Clements ("Hourly Variations in Carbohydrate Content of Leaves and Petioles," *Bot. Gaz.*, **89**, 241–72, 1930), who determined the hourly changes in simple sugars, sucrose, starch, and hemicelluloses in each of potato, soya bean, and sunflower on one day in each of the months July, August, and September respectively in two successive years, 1926 and 1927. Clements also concluded that the simple sugars, hexoses and pentoses, are the first sugars formed in photosynthesis.

Two papers by F. Górski dealing with the bubble-counting method of measuring photosynthesis should be noted. (" Recherches sur les méthodes de mesure de photosynthèse chez les plantes aquatiques submergées," *Acta Soc. Bot. Poloniæ*, **6**, 1–29, 1929 ; " Sur la précision de la méthode de la numération des bulles dans les recherches de photosynthèse," *Bull. de l'Acad. Polonaise des Sci. et des Lettres, Cl. Sci. Mat. et Nat. Ser. B.*, 1–37, 1930.) The author measured the volume of the gas given off in the form of bubbles from assimilating *Elodea* plants and compared this with the total volume of oxygen evolved, this being partly contained in the bubbles and partly dissolved in the water. He found the ratio of the total oxygen evolved to the volume of the gas in the bubbles very variable, the bubbles only constituting from 40 to 80 per cent. of the total oxygen. The author described the precautions which should be taken in using the method for comparative measurements of rate of photosynthesis.

Lastly, reference may be made to a paper by H. Meyer (" Untersuchungen über die Chlorophyllase," *Planta*, **11**, 294–330, 1930), in which is described an exhaustive investigation of the properties of the enzyme chlorophyllase. Perhaps the most noteworthy of the findings recorded in this paper are that the enzyme occurs in the roots of *Heracleum* and *Lamium* as well as in the leaves, and that the leaves of plants cultivated completely in the dark contain less chlorophyllase than those of normal plants, while plants subjected to intermittent illumination which develop comparatively large leaves with only a little chlorophyll, have approximately the same content of chlorophyllase as normal green plants.

PEDOLOGY. By Prof. G. W. Robinson, M.A., University College of North Wales, Bangor.

International Society of Soil Science.—The chief event of the year 1930 was the second International Congress of Soil Science held in Russia. This was attended by a representative body of soil investigators from all parts of the world. The Congress was followed by an excursion through the principal soil zones. The examination of profiles afforded good opportunities for becoming acquainted with certain aspects of Russian work on soil formation, which by reason of their personal and qualitative character had hitherto been imperfectly grasped by pedologists of other countries. The proceedings of the Congress will be dealt with in a later contribution.

New Books.—E. Blanck's monumental *Handbuch der Bodenlehre* (Julius Springer, Berlin) has now reached its sixth volume. Probably the most generally useful of these volumes is " Band III, Die Lehre von der Verteilung der Bodenarten an der Erdoberflache," which gives the fullest account hitherto available of the principal world soil groups. Among the contributors are Prof. H. Stremme, who deals with podsols, brown earths, and tshernosems ; Prof. A. A. J. De'Sigmond, who deals with chestnut earths, grey steppe soils, and saline soils ; and Prof. H. Harrassowitz, who deals with tropical soils, including laterites.

A. Stebbutt's *Handbuch der allgemeinen Bodenkunde* (Borntraeger, Berlin) may be strongly recommended for the excellent perspective in which the subject of soils is presented. The treatment is definitely from the standpoint of the author's own views, and the work thereby gains in authority and interest.

The study of the soils of the tropics is assuming increasing importance both from the economic and from the scientific standpoint, and the appearance of *Grundriss der Tropischen und Subtropischen Bodenkunde* by P. Vageler (Verlags-gesellschaft für Ackerbau, Berlin) is specially welcome if only by reason of the vast experience of the author in the subject with which he deals. This work should have an important influence on the development of our knowledge of tropical soils.

Climate, Geology, and Soil.—The emphasis laid during recent years on the effect of climate on soil formation has led to the necessity for a restatement of the part played by the parent rock material in determining soil characteristics. B. Polynov (*Soil Research*, 1930, **2**, 165–80) contributes a useful and suggestive paper dealing with this problem and shows that, within the great soil groups, differences in parent material exert an important effect on soil properties. It is well known,

for example, that the occurrence of quartzose sand as a parent material can result in the extension of the podsol zone into climatic regions generally occupied by brown earths. Calcareous material represses the tendency to lateritic weathering in the tropics and encourages a siallitic type of weathering. The author suggests a classification of rocks with respect to their characters as parent materials into (1) crystalline, such as igneous and schistose rocks; (2) transitional, such as gravelly moraines, arkoses, quartzites, and calcareous schists; and (3) sedimentary or weakly metamorphosed, such as sandstones, shales, loess, and chalk. These are again subdivided according to their basicity. A useful distinction is drawn between monochronogenous soils, or soils of primary weathering, and heterochronogenous soils, derived from material such as unconsolidated sediments which have already undergone weathering.

E. M. Crowther (*Proc. Roy. Soc.*, 1930, B; **107**, 1–30) has submitted published data for the composition of the clay fraction of certain American soils and the related climatic data to statistical analysis. The silica-alumina ratio of the clay fraction is correlated negatively with rainfall and positively with temperature. Under comparable climatic conditions, the lowest silica-alumina ratios occur in the clay fractions of soils of primary weathering from igneous rocks, whilst reworked aqueous sediments yield soils with the most siliceous clay fractions. Highly siliceous clays also occur in arid regions with incomplete leaching. Leaching is the dominant factor in soil formation.

Soils of Spain.—The soils of Spain, whose position in a world classification has been somewhat uncertain, are discussed in a paper by E. H. Del Villar (*Proc. Int. Soc. Soil Sci.*, 1930, **5**, 192–7; abstract only). They are divided into six series, namely, (1) peaty soils; (2) siallitic soils; (3) allitic soils; (4) calcareous soils; (5) alkaline soils; and (6) alluvial soils. The peaty series is characterised by the presence of acid humus. The siallitic series comprises soils with saturated or "mild" humus and may be equated with the brown earths of Ramann and other writers. In the allitic series sesquioxides predominate over silica in the clay complex and there is a tendency to the formation of superficial crusts as in the laterites, to which they are related. The calcareous series corresponds with the rendzina and tshernosems of middle and Eastern Europe. The alkaline series includes the well-known solonetz and solontchak types. The alluvial series includes recent alluvial deposits, but excludes old alluvia on which soils of the other series may have developed. In each series certain stages are recognised, such as the skeletal stage, and the oropedic stage, which in-

cludes the shallow soils of mountainous regions. Whilst the peaty, siallitic, allitic, and calcareous soils show a zonality conformable to the climate, the alkaline and alluvial soils are local in character.

Tropical Soils.—M. V. Agafonoff (*Soil Research*, 1930, **2**, 184–96) describes some red soils derived from basaltic rocks in Indo-China. The results indicate an excess of sesquioxides in the weathering complex. In certain cases ferruginous crusts are developed. These are considered to be due to the upward translocation of material during periods of desiccation.

Evidence has accumulated during recent years that podsol profiles are by no means confined to cool humid regions, but may also occur in the humid tropics. E. Balzarotti (*Soil Research*, 1930, **2**, 1–10) describes profiles from San Domingo which exhibit the bleached layer, and the underlying reddish-brown layer characteristic of podsols. They occur under savanna, with a mean rainfall of 1,288 mm. These circumstances would not suggest the possibility of podsolisation. Their occurrence may be associated with ground water conditions.

M. W. Senstius (*Soil Research*, 1930, **2**, 10–56) discusses the soils of Java and Luzon from the point of view of current theories of soil genesis. The soils show a well-marked vertical zonality and can be distinguished on the basis of temperature, the relationship of rainfall to evaporation, their permeability to water movements, and the position of the water table. The mechanism of the translocation of silica and sesquioxides is discussed.

F. J. Martin and H. C. Doyne (*J. Agric. Sci.*, 1930, **20**, 135–43) describe some laterites from Sierra Leone. They restrict the term lateritisation to the removal of silica from the parent rock resulting in a residuum containing a high proportion of alumina relative to silica. The norite of Sierra Leone in some cases shows a margin of soft yellowish-white material with brown specks, passing into a bauxitic material. It is noteworthy that the silica-alumina ratio of this light-coloured transitional material is about 0·30, a figure which excludes the possibility that the primary weathering product is an alumino-silicic clay of the kaolin type from which the laterite is differentiated. The proportion of iron decreases from the marginal zone. In other cases the iron oxide is not removed and acts as a cementing agent in the typical orange-red laterite with vesicular structure. Cases intermediate between bauxite formation and vesicular laterite formation occur. The silica-alumina ratios of the clay fractions of soils formed by the lateritisation of norite vary from 1·05–

1·69. Even the yellowish-white material, having a silica-alumina ratio of 0·18, yields a clay fraction whose silica-alumina ratio is 1·41. Lateritisation proceeds in detrital material with the formation of ferruginous sub-surface pans. One result of the formation of concretionary material is to increase the proportion of stones and gravel.

A. Matthaei (*Soil Research*, 1930, **2**, 57–67) discusses the interrelationship of climate, vegetation, and soil types in Chile.

Terra Rossa.—B. Filosofov (*Glinka Mem. Vol. Leningrad. Agric. Inst.*, 1928, 191–207), from an examination of "terra rossa" from the vicinity of Rome, concludes that the weathering process is analogous to lateritisation. The red colour is considered to be determined by the presence of calcium, although this constituent is not necessary for the formation of "terra rossa."

Soil Profile Development in North Wales.—G. W. Robinson (*J. Agric. Sci.*, 1930, **20**, 618–39) presents studies on the development of soil profiles in North Wales as illustrated by the character of the clay fraction. The data indicate the general tendency for sesquioxides to be removed from the surface soil and deposited in lower layers. The horizons of podsolic profiles are clearly distinguished by means of the silica-sesquioxide ratio of the clay fraction. It is probable that many of the profiles of North Wales have been truncated by erosion, so that the present surface soil may represent a former B horizon. Secondary podsols may be developed under heath vegetation in B horizons exposed in this way.

Soil Colloids.—S. Mattson (*Soil Sci.*, 1930, **30**, 459–95) has made a further important contribution to the knowledge of the laws of colloidal behaviour in soils. Iso-electric precipitates of hydrated ferric oxide and alumina have been prepared from the corresponding chlorides and sulphates. The sulphate precipitates are iso-electric at a lower *p*H than the chloride precipitates. When phosphate and silicate ions enter the sesquioxide complex the iso-electric *p*H is lowered by displacement of diffusible acid ions as well as hydroxyl ions. With high concentrations of phosphate and silicate, limiting values are approached, namely, $P_2O_5/Al_2O_3 = 1{\cdot}0$, and $SiO_2/Al_2O_3 = 3{\cdot}0$, for the iso-electric precipitate. Iso-electric precipitates always contain a small proportion of diffusible anions. The adsorption layer is very complex, and even where definite stoichiometrical proportions occur in the interior of crystals, the surface conditions may introduce complications. The data obtained for iso-electric precipitation of silica-alumina and silica-ferric oxide complexes under different conditions are in agreement with the observed composition of the soil

colloidal inorganic constituent under different climatic conditions. In arid climates, a complex with high silica-sesquioxide ratio occurs, stabilised by the presence of bivalent cations. Under tropical humid conditions, the protected silica is removed together with the protecting bases, and a complex richer in sesquioxides results. The high acidity due to the formation of acid organic matter in humid temperate and cold regions results in the loss of sesquioxides and the production of a siliceous type of complex. These conditions can be demonstrated by laboratory experiments.

During recent years the examination of clays by X-ray methods has yielded important information, and an interesting paper is presented by S. H. Hendricks and W. H. Fry (*Soil Sci.*, 1930, **29**, 457–76) in which the X-ray results for the colloidal clay of different soils are presented. Specific samples show characteristic diffraction patterns. It appears that montmorillonite, beidellite, Ordovician bentonite (or montmorillorite and quartz), and halloysite are common constituents. The methods used would appear to be of value for the characterisation of soil types.

L. D. Baver (*Soil Sci.*, 1930, **29**, 291–309) presents data to show the effect on physical properties of progressive neutralisation by different bases of an electrodialysed Putnam (U.S.A.) clay. The saturation capacity, determined conductometrically and potentiometrically, was found to be 57 mg. equivalents per 100 grs. clay. The reaction, as measured by pH, of a series of clays containing equivalent amounts of different cations was found to be in the order Li > Na > K > Mg > Ca > H, corresponding with differences in dissociation depending on hydration.

C. E. Marshall (*Trans. Faraday Soc.*, 1930, **26**, 173–89), from a study of the double refraction of clay suspensions, concludes that the particles are crystalline and that a clay saturated with a particular cation can be characterised by its double refraction. The results indicate that the cations occupy definite positions in the clay lattice.

Soil Organic Matter.—From the results of M. Phillips, H. D. Weihe, and N. R. Smith (*Soil Sci.*, 1930, **30**, 383–90) it appears that microbial decomposition of pentosans and cellulose in maize stalks, maize cobs, and wheat straw takes place rapidly under laboratory culture conditions at 28° C. Under suitable conditions, lignin of natural materials is also decomposed by organisms, although lignin prepared in the laboratory may not be thus decomposed. It is probable, however, that prepared lignin is not identical with natural lignin and that conclusions based on the behaviour of prepared lignin are not applicable to natural conditions.

H. Jenny (*Soil Sci.*, 1930, **29**, 193–206) has studied the variations in the nitrogen content of soils along two selected isotherms traversing regions of the United States with varying humidity. Both in the temperate regions, with mean annual temperatures of 51–53° F., and in the sub-tropical regions, with mean annual temperatures of 64–68° F., the average nitrogen content of grassland soils increases logarithmically with the humidity (expressed by the N-S quotient of A. Meyer). The data for forest soils do not justify any general inferences as to the relationship between humidity and nitrogen content. The nitrogen-temperature relationship is a continuous function always decreasing from north to south. The carbon-nitrogen ratio is unaffected by humidity in the temperate regions, and is nearly the same for forest and grassland soils.

C. W. B. Arnold and H. J. Page (*J. Agric. Sci.*, 1930, **20**, 460–75), from a study of alkaline extracts from soils receiving different treatment, conclude that a similar type of organic matter is extracted. M. M. S. Du Toit and H. J. Page (*J. Agric. Sci.*, 1930, **20**, 478–88) have studied the humification of certain plant materials. It would appear that lignin is the principal source of humified organic matter.

G. W. Robinson and W. McLean (*J. Agric. Sci.*, 1930, **20**, 345–7) have noted the occurrence of elementary carbon in the form of coal, etc., in certain cultivated soils in proportions sufficient to vitiate determinations of organic carbon by combustion methods.

W. R. Leighly and E. C. Shorey (*Soil Sci.*, 1930, **30**, 257–66) present data for the carbon-nitrogen ratio of a range of North-American soils. They show considerable variation, namely, from 35·2 : 1 in a silty clay loam subsoil from Louisiana to 3·5 : 1 in a fine sandy loam subsoil from Florida. In general, the ratio decreases from the surface downwards. It is suggested that a wide carbon-nitrogen ratio may indicate the presence of organic matter available for micro-organisms, and to that extent have a bearing on problems of soil fertility.

W. McLean (*J. Agric. Sci.*, 1930, **20**, 348–54) finds that the carbon-nitrogen ratio in a selection of fifty British soils ranges from 6·5 to 13·5 : 1, the average being 10 : 1. Sixteen foreign soils gave carbon-nitrogen ratios varying from 2·0 to 23·0 : 1. The results do not bear out H. Jenny's contention that the carbon-nitrogen ratio tends to become wider in colder climates and narrower in warmer climates.

Organic Hardpans.—The formation of organic hardpans in Florida has been examined by L. A. Richardson (*Soil Sci.*, 1930, **29**, 481–7). It would appear that a permeable soil, low

in silt and clay, freedom from electrolytes, and the presence of a water table near the surface during part of the year favour hardpan formations under the local climatic conditions. A definite concentration of alumina in the hardpan was observed. Non-hardpan profiles show a much more marked decrease in carbon-nitrogen ratio with depth than do hardpan profiles. The reaction of the profiles is generally acid, and it is not considered that they have ever been more acid than at present (*p*H, 5·6).

Changes in Submerged Soils.—W. O. Robinson (*Soil Sci.*, 1930, **30**, 197–218) gives an account of some investigations on the processes occurring in submerged soils. These soils are characterised by high proportions of soluble iron, calcium, magnesium, and manganese, in the form of hydrogen carbonates, and it is shown that these constituents are brought into solution by the carbon dioxide produced in the microbiological decomposition of organic matter. No increase in solubility occurs in the absence of organic matter. Apart from acidity due to carbon dioxide, there is no increase in acidity during short periods of submergence. Hydrogen sulphide and other sulphides are produced, and give a distinctive odour. Where conditions are favourable for translocation, submerged soils may suffer rapid losses of bases. Organic matter also disappears rapidly. Where organic matter is present, toxicity to plant growth may develop after a few days' submergence.

The rapid decomposition of organic matter under submerged conditions noted by the last author is not found in the laboratory experiments of F. G. Tenney and S. Waksman (*Soil Sci.*, 1930, **30**, 143–60).

Soil Physics.—G. B. Bodman and M. Tamachi (*Soil Sci.*, 1930, **30**, 175–95) have examined the behaviour of a number of Californian soils in the plastic state. Comparing the moisture content of soil-water pastes of equal stiffness, they find it to be closely correlated with the air-dry moisture content, the moisture equivalent, and the colloidal clay content. This relationship holds good for varying degrees of stiffness. Colloidal organic matter is similar to colloidal clay in its effect on the plastic properties studied.

L. C. Kapp (*Soil Sci.*, 1930, **29**, 401–12) has determined the heat of wetting of soil and mineral particles of different sizes, previously freed from organic matter. Whilst pure minerals, with the exception of hornblende and biotite, show unappreciable heat of wetting above 0·005 mm. particle size, soil particles exhibit this property to an appreciable extent in the 0·005–0·01 mm. fraction, and in some cases even in the next fraction. It is therefore considered that material up to

0·01 mm. should, so far as heat of wetting is concerned, be considered as colloidal.

W. B. Haynes (*J. Agric. Sci.*, 1930, **20**, 97–116) discusses further the problem of moisture distribution in the " ideal soil." Changes in moisture distribution are not strictly reversible, and may be discontinuous in character owing to the cellular character of soil pore-space.

New Methods of Mechanical Analysis.—C. E. Marshall (*Proc. Roy. Soc.*, 1930, A, **126**, 427–39) describes a centrifugal method of mechanical analysis depending on the fall of different-sized particles from a thin layer of suspension floated over a column of a denser liquid, such as an aqueous solution of sucrose.

METEOROLOGY. By E. V. NEWNHAM, B.Sc., Meteorological Office, London.

L. WEICKMANN, who has for several years made a study of the occurrence of periodic pressure waves in the earth's atmosphere, contributes another paper on this subject in *Zs. Geophysik. Braunschweig.*, **6**, 1930 (pp. 291–6), entitled " Die dominierende Luftdruckwelle des strengen Winters 1928/9." It may be recalled that this winter brought severe cold to a large part of North America and Europe. In the British Isles the month of February was notable for the very low temperatures that occurred, the air temperature at a height of four feet falling below zero (Fahrenheit) in parts of England. On very few occasions since the severe winters of the early 'nineties has weather nearly so cold occurred in this country; the severest spell began with a sudden onset of intensely cold and dry wind that could be traced in its origin to a wind current that moved southwards from the polar basin on the east side of an anticyclone centred over Scandinavia.

Weickmann found a very pronounced periodic variation of atmospheric pressure during this winter, of 4 to 5 millimetres (about 6 millibars) amplitude, and having a period of 20 days. The waves occurring between October 28, 1928, and February 19, 1929, were found to have a point of symmetry on December 21, 1928, so that the main features of the variations during this period were similar at equal intervals before and after the latter date. He shows that they can be regarded as oscillations of the atmosphere of the " polar cap," *i.e.* of the region of cold air lying north of the westerlies of temperate latitudes. It may be noted by the way that intermittent discharges of the air from this region play an important part in the theories of cyclones and the general circulation put forward by Bjerknes and his school, a discharge taking place behind a certain proportion of temperate cyclones in the form of a northerly wind

that finds its way into the north-east trade winds, and thence into the tropics. Weickmann had previously made a similar analysis to the one under review for the winter 1923–4; in 1928–9 the "centre" of oscillation was displaced from the position found in the earlier investigation to a region around the Barents Sea, whence the waves were propagated radially. The effect of these waves on atmospheric pressure throughout the Northern Hemisphere are illustrated in his paper by specially prepared charts.

A. Angström gives an interesting account of a rather novel line of investigation into the distribution of dust in the earth's atmosphere in a recent paper, entitled, "On the Transportation of Dust from Low to High Latitudes through the Circulation of the Atmosphere" (*Geog. Ann.*, Stockholm, **12**, 1930, pp. 88–92). The subject is not normally treated on such a large scale; most of the research done so far has been directed towards giving a quantitative account of the variations of the dust content at a particular place without much inquiry into the source of the dust, except when the influence of industrial or domestic smoke has been the main interest underlying the work, as in the well-known treatise of Shaw and Owens, entitled, "The Smoke Problem of Great Cities." Angström uses actinometric measurements as the observational basis for a mathematical analysis of the dust transported through a vertical plane facing north and south. He finds a formula for a quantity Δ which represents the net gain of dust per annum in a vertical column of 1 square centimetre section which extends throughout the atmosphere. This quantity is found to increase with increasing latitude from the equator to the poles, being negative between latitudes 35° N. and S. He calculates that the deposition of the dust around the poles, whereby presumably the excess of dust received advectively over that which passes out by the same process is disposed of in order to keep the mean quantity held in suspension constant, would result in a deposit 1 cm. thick in 2,000 years. Owing to uncertainty in the observational data on which the calculations are based, the author lays no claim to obtaining more than an idea of the orders of magnitude of the quantities calculated.

Lord Rayleigh has contributed (London, *Proc. R. Soc.*, **126** (**A**), 1930, pp. 311–18) a valuable discussion on the phenomenon known as the "green flash," which is sometimes observed when the sun disappears behind the horizon in very clear weather. His object was to find out whether the phenomenon can be explained simply as a result of normal atmospheric dispersion, or whether it is necessary to assume the existence of abnormal horizontal stratification of the atmosphere into layers of different density such as is known to occur in connection

with the mirage. The intermittent occurrence of the effect even in cloudless weather, may at first sight suggest that a simple explanation founded on dispersion would not be adequate.

Atmospheric refraction at the horizon, which amounts to about 35 minutes of arc, gives a separation of about 22 seconds of arc for the C and F lines of the solar spectrum. Now the resolving power of the unassisted human eye would not be sufficient to separate lights of different wave-length having this degree of separation, and a greater degree of dispersion would seem to be necessary if the disappearance of the limb of the sun behind the horizon is to give a colour effect like the green flash, which is visible to the naked eye. Experiment, however, did not bear out this view. The first arrangement for artificially reproducing the phenomenon consisted of a crown-glass prism with an angle of three quarters of a degree, giving the required 22 seconds of arc for the dispersion of the C and F lines. An illuminated slit simulated the setting sun, and the artificial obscuring horizon was constructed from a Gillette razor blade, the sides of which were blackened, while the edge was kept clean. The distant horizon was simulated by placing the razor edge in the focal plane of a telescope of unit magnifying power, thereby giving parallelism to the rays from the blade and slit without disturbing the dispersion caused by the prism. When the illuminated slit was gradually obscured by the blade, a flash of colour was seen at the last disappearance. The colour was blue when daylight was used to illuminate the slit, and greenish-blue when the source of light was relatively deficient in blue and violet, as was the case when an opalescent electric bulb was used. The fact that the flash had not the vivid green colour of the natural phenomenon may perhaps be explained by a still greater deficiency of blue and violet in the light of the setting sun.

An alternative arrangement of great ingenuity was devised in order that a much closer approximation to the natural phenomenon could be made. The same prism was used. Light from the sun was admitted to the prism through a shutter, the prism being arranged with its refracting edge vertical, while vertical screens limited the width of the entering beam. By these means a luminous strip was thrown on a screen 20 metres distant. It was observed that this strip was coloured at its edges, being red and greenish-blue in accordance with the smaller and greater refraction undergone respectively by light of these colours. The first screen was then removed, and the luminous strip was allowed to fall upon a vertical white screen only 1 mm. wide. On allowing the illumination to travel across this, at the last glimpse the latter was found to be greenish-blue in colour, an effect which lasted for about

three seconds, as happens in the case of the natural green flash. Lord Rayleigh thus found normal atmospheric dispersion to be adequate to produce the effect. The fact that it is often not seen when the sun is visible at sunset he attributes to want of clearness of the atmosphere on those occasions, and considers that the use of a telescope is of assistance only in so far as it increases the apparent size of the strip of green light.

A paper describing measurements of dew made in Italy on certain nights in August 1929, by P. Sulla Gamba, appears in *Boll. bimens*, **48**, 1929. pp. 58–69. The dew was automatically recorded by three instruments, in which the collecting surface was the top of an ebonite plate, placed 10 cm. above the ground among various kinds of undergrowth. A deposit equivalent to 0·3 mm. of rain was obtained on some nights. It may be pointed out that from a measurement of this kind it is not possible to calculate the dew that would have been deposited, for instance, on a leaf of the same size as the ebonite plate exposed in the same situation, for the amount is dependent upon the fall of the temperature of the collecting surface below that of the surrounding air, and this fall is greater the more effectively heat is prevented from reaching the collecting surface from below. It is a matter of common observation that more hoar frost is deposited after a clear still night on poorly conducting substances than on substances that conduct heat well; this was pointed out more than a century ago by Dr. Wells in his "Essay on Dew." The author appears to attach importance to a fact that came to light in his experiments, namely, that dew continued to be deposited for an hour or more after the minimum temperature had been reached, and even after the sun had risen, and appeared to regard this as inexplicable in the light of current theories about dew. It would appear, however, that a slight rise of temperature due either to a slight increase of wind, or to some increase in the radiation received from the sky, would not necessarily prevent the ebonite surface from continuing to maintain itself, by vigorous outward radiation to a nearly clear sky, at a temperature below the dew point of the air around it. If slight air movement were to set in after almost complete stagnation, as readily happens about sunrise, then there would, as Wells showed, be a tendency for increased formation of dew on account of the greater volume of air cooled by contact with the chilled surface.

A study of the question as to whether the presence of overhead electric power lines increases the risk of damage by lightning in their neighbourhood has been initiated by the Société Météorologique de France. There appears to be a general consensus of opinion among those who took part in

this enquiry in favour of regarding them as of no influence in the sense that wires are not more liable to be struck by lightning than other raised objects, and that their presence is without influence upon thunderstorm formation. On the other hand, the geographical distribution of frequency of lightning damage has been shown not to be a random one, but apparently to be connected with the distribution of different geological strata. According to C. Dauzère ("Récherches sur les orages," *Météorologie*, Paris, 5, 1929, pp. 145–68), compact limestones tend to be immune, while slaty schists, granites, and ophites are often struck, and lines of geologically different rocks in general tend to be regions of frequent lightning damage. This writer does not consider that the observed relationships can be explained by variations in the electrical conductivity of the rocks, and suggests that experiment supports the view that the danger zones are regions with a particularly great degree of ionisation of the air, with a preponderance of negative ions. It is maintained that this view accords with Simpson's theory that in most cases lightning is a discharge from a positive cloud to negative earth.

The same paper contains a study of hail formation. The theory is advanced that hailstones are formed by the attraction of the ice crystals of cirrus cloud, positively charged by ultra-violet light, for the super-cooled water drops of the upper parts of the cumulo-nimbus cloud, which according to Simpson are mainly negative or neutral. It is argued that the process will result in positively charged hail, as actually observed by McClelland and Nolan. According to this theory the formation of hail is dependent on ionisation of the air and not simply on thermodynamical processes, and the view is advanced that without the aid of this theory the geographical distribution of hail in France cannot be explained.

ARTICLES

SOME NEW ASPECTS OF RADIO-ACTIVITY

By C. D. ELLIS, Ph.D., F.R.S., Cambridge University.

One of the striking features of modern physics has been the succession of theories which have been proposed to deal with the dynamics of atoms and electrons. The classical Newtonian mechanics formed a sound basis for the first electrical structure of the atom proposed by J. J. Thomson, but when Rutherford showed experimentally that the atom was built in a way that was inevitably unstable on the classical mechanics, a new fundamental theory, the quantum theory, was put forward. For many years this directed and dominated research with admirable results, but slowly the rate of advance diminished, the number of problems it was incapable of dealing with increased, and it became clear that the true theoretical basis for dealing with atomic problems had not yet been reached. The wave mechanics was put forward expressly in order to deal with these difficulties. In this case the theory was proposed on theoretical grounds, although the essential basis, the existence of material waves, might well have first been demonstrated experimentally. Here again the early advances and successes of the theory were numerous and rapidly made, because the theory was dealing with the type of problem for which it had been constructed. But there are not signs lacking that history may be repeating itself, and that problems are accumulating which are insoluble on this theory.

A striking success of the wave theory has been found in its ability to deal with the α-particle type of radio-active disintegration. This has given a new vigour to researches in nuclear physics which had hitherto been entirely experimental and unguided by any theory, and at first sight it appeared that the wave mechanics was precisely the theory required to deal with nuclear problems. This seems, however, to be an unduly optimistic view, since there are at least two phenomena presented by the nucleus which resist the attacks of the wave mechanics to just the same degree as intensity problems did the old quantum dynamics or the emission of light spectra did the classical theory. It may be that only a slight extension of the theory will be required, but we must not lose sight of the

possibility that the explanation of these two phenomena may require a new theory which will absorb the wave mechanics just as the wave mechanics absorbed the quantum theory. It is this possibility, even if at present it can be regarded as extremely remote, which lends a peculiar interest to these two phenomena.

The first of these phenomena is concerned with the details of the emission of electrons from the nucleus in the β-ray type of radio-active disintegration. The essential point which is difficult to understand is why the velocities of the electrons from any one radio-active body vary over wide limits. This seemed on several grounds to be so improbable that many experiments have been made to see whether this effect was not due to some subsidiary process, but every experiment showed that this was not the case, and that in the β-ray type of disintegration the electrons emitted from the nucleus form a continuous energy spectrum.

The second phenomenon is what is termed the internal conversion of the γ-rays, and is concerned essentially with the transfer of energy from an excited nucleus to the electronic structure of the same atom. Experiments have shown that to produce the effects which are observed, there must be an intimate coupling between the nucleus and the electronic structure which by itself is a matter of great interest. The wave mechanics certainly suggests a simple mechanism by which this can happen, but a closer quantitative calculation of the effect brings to light a striking divergence between theory and experiment.

While no explanation of this failure to deal with these two problems has yet been proposed, it is not difficult to see in a general way where the theory breaks down.

The Continuous β-Ray Spectrum

It is well known that the process of radio-active disintegration consists in the emission of a particle from the nucleus of the atom, resulting in a change of the atomic number and the formation of a new atom. Two types of disintegration are known, involving respectively the emission of an α-particle, that is, a positively charged helium nucleus, and a β-particle, that is, an electron. Radium is a typical example of α-particle disintegration. The radium atom has an atomic weight of 226, and an atomic number of 88. The α-particle which is emitted from the nucleus has a mass of 4 and a charge of $2e$, so that what is left of the radium nucleus has an atomic weight of 224 and an atomic number of 86. This, when it has assumed its correct outer electronic structure, is an atom of the inert gas radon, which in its turn is radio-active. The β-particle

disintegration scarcely alters the atomic weight, owing to the small mass of the electron, but the atomic number in this case increases by unity. Thus radium B, which is an isotope of lead, atomic weight 214, atomic number 82, changes into radium C, an isotope of bismuth, atomic weight 214 as before, but atomic number 83.

We will first consider the α-particle disintegration, because this can be described by the wave mechanics, and the contrast with the β-particle case which cannot be so treated is illuminating. The rate at which the atoms of a radio-active body change into the following body is governed by what is termed

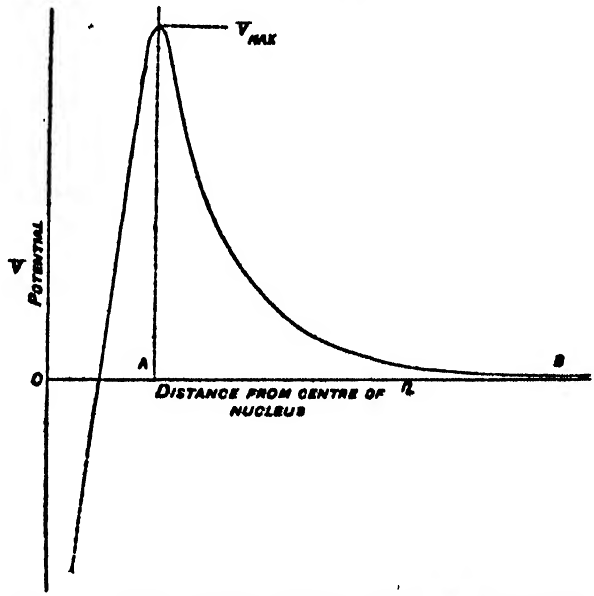

FIG. 1.—Diagram of the Potential Field near the nucleus.

the exponential law of decay. If initially there are N_0 atoms present, then after time t, there remain only $N_0e^{-\lambda t}$, when λ is a characteristic constant of the body. It has been shown that the interpretation of this is that the probability that a given atom shall disintegrate within a small interval of time is independent of the time which has elapsed since the formation of the atom. There is thus a characteristic indefiniteness in the sense that no fixed life is prescribed for every atom. When, however, the disintegration does occur, it is identically the same for each atom, and all the emitted α-particles have the same energy.

In considering how this ejection of the α-particles can occur, it is, of course, necessary to form some rough picture of how the α-particle is bound in the nucleus, and the simplest method is to assume a potential field of the type shown in Fig. 1.

The curve shows the potential energy of an α-particle at different distances r from the centre of the nucleus. In the region OA the α-particle is held in, whilst in the region AB it is repelled. OA in this case shows the radius of the nucleus and AB is the normal "inside" of the atom which is occupied by the electrons. Owing to the negative charge of the electrons, they of course experience an attraction in the region AB. Suppose now that there is an α-particle in the nucleus with positive energy E, which is less than V_{max} multiplied by the charge of the α-particle $2e$, *i.e.* $2eV_{max}$. In accordance with general quantum principles we consider this α-particle to be occupying one of the characteristic energy states of the nucleus. On the older quantum theory the only possibility for emission would involve an internal rearrangement by which this α-particle obtained sufficient energy to surmount the top of the potential barrier. Its energy of ejection would therefore be equal to or greater than $2eV_{max}$, since even if it just reached the turning-point of the field, it would gain kinetic energy equal to $2e\ V_{max}$ by repulsion from the nucleus as it traversed the distance A–B. Therefore, reversing the argument, we conclude, on the basis of the older quantum theory, that if a radio-active body emits an α-particle of energy E, the height of the potential barrier surrounding the nucleus is equal to or less than $E/2e$. In this way we can obtain a general idea of the height of the potential barrier surrounding the nuclei of heavy atoms. Supposing now α-particles of energy greater than this maximum are allowed to impinge upon such atoms, we should expect them to enter into the nucleus. If this were to happen it would be shown by what is called anomalous scattering. Actually no such effect is found to occur, and the results of such experiments can only be explained on the basis that the height of the potential barrier is very much greater than was deduced by the previous argument. This was a complete impasse until it was pointed out independently by Gamow and by Gurney and Condon that the wave mechanics provided a solution. On this theory the α-particle in its stationary state inside the nucleus must be represented by a wave function, which would be permanently stable if the thickness of the potential barrier were infinite. Actually, owing to the finite thickness, the boundary conditions for the wave motion can only be satisfied if there is a wave motion of the same frequency outside the barrier. This means that if we know that at a certain time the α-particle is inside the barrier, then at any subsequent time there is a chance that it will be found outside the nucleus. A closer consideration of this view shows that it leads precisely to the exponential law of decay, since there is nothing in it which gives a definite life to every nucleus, but

the theory merely gives a probability of disintegration at any time. A further point of great importance is that since the frequency of the material wave is the same both inside and outside the potential barrier, it follows that the energy of the α-particle when it is emitted will be the same as the energy it had inside the nucleus.

The importance of this result can scarcely be overemphasised. There is no longer any difficulty about the height of the potential barrier; this can be far greater than the energy of the emitted α-particle, since the α-particle leaks through the barrier now instead of having to surmount it. Further, by measuring the energies of the emitted α-particles, we at once obtain values for the energies of the stationary states inside the nucleus. The homogeneity of the energies of the α-particles is just what would be expected from well-defined nuclear levels. Briefly, the whole position was clarified by this theory, and a definite line of research on these α-particle nuclear levels was opened up.

Turning now to the consideration of the β-ray type of disintegration, we should expect that in much the same way the energy state of the electrons in the nucleus would be at least indicated by the energy of emission of the β-particles. The surprising fact, however, is found that the disintegration electrons from the same type of nucleus do not all have the same energy. The general method of experiment is to take a small source of the radio-active body under investigation, and to place it in a magnetic field. The electrons which are emitted describe curved paths in the field, the curvature depending inversely on the velocity. By arranging a measuring device, such as an ionisation chamber or a Faraday cylinder in some definite geometrical position relative to the source, it is possible, by varying the magnetic field, to test in succession whether electrons of specified velocities are present or not. The result for a typical β-ray body radium E is shown by the full curve in Fig. 2, from which it will be seen that the disintegration β-particles form a continuous spectrum. The immediate interpretation of this result would appear to be that the electrons inside the nucleus have varying energy, but this idea was so difficult to accept that many suggestions were put forward to explain this dispersion of velocities as being due to some secondary process after the electrons had actually left the nucleus. It was thought that the β-particles were initially ejected with definite energies, as in the α-ray case, but that owing to subsidiary processes they lost varying amounts of energy before they emerged from the atom and could be measured. This point of view was definitely disproved by the experiments of Ellis and Wooster and of Meitner. The prin-

ciple of the experiments was much the same in the two cases, and can be described as follows. A known quantity of the radio-active body radium E was placed inside a very small lead calorimeter whose walls were sufficiently thick to absorb completely the emitted β-particles. The energy of the β-particles was converted into heat, which was measured by ordinary calorimetric methods. Other experiments had shown that there was no easily detectable radiation from radium E other than the β-particles, but there was, of course, the possibility that energy might be carried away from the atom in a variety of forms which could not be detected in the presence of the strong β-ray radiation. If this were the case such energy would definitely be less penetrating than the β-rays, and would also be stopped in the walls of the lead calorimeter, and add its effect to the heating. If the hypothesis of constant energy of disintegration was to be maintained, it is clear that the heating effect per atom should correspond to the upper limit E_0 of the continuous β-ray spectrum. An electron corresponding to this energy would be one that escaped without any energy loss, whereas one at the point E_1 (see Fig. 2) would have dissipated the amount $E_0 - E_1$ in the hypothetical subsidiary process. If, however, the electrons were actually emitted from the nucleus with the varying energy as shown in Fig. 2, then the heating experiment should lead to a value corresponding to the centre of gravity of this curve. The results of the experiment were decisive. The upper limit E_0 is close to 1 million volts, the centre of gravity of the curve is about 350,000 volts, while Ellis and Wooster's measurement gave 335,000 volts with an error of 10 per cent., and Meitner's measurement gave 356,000 volts with an error of 6 per cent. This result certainly applied only to one body of radium E, but there was sufficient indirect evidence to justify extension to all β-ray bodies, and it is now generally accepted that the β-ray disintegration has this peculiar indefiniteness.

The only feature of definiteness which appeared to remain was a sharp upper limit to the energy of the emitted electrons, such as is indicated in Fig. 2. A great many experiments had shown that this appeared to hold for most β-ray bodies, and it was generally felt that this would be a matter of cardinal importance in attempting an explanation of the origin of the continuous spectrum. However, recently even this last relic of definiteness has been swept away, and it appears that there is no definite upper limit, but that the spectrum tails off exponentially to higher energies without ever reaching a definite end, as is indicated by the dotted line in Fig. 2. These experiments have been carried out by Terroux in the following way. A source of radium E was placed near a Wilson expansion

apparatus, so that a beam of the disintegration electrons could traverse the gas space. The cloud tracks of the electrons were photographed in the usual manner, but in addition at the moment of expansion a magnetic field was applied parallel to the axis of the chamber. This caused the tracks of the electrons to be curved, and knowing the value of the magnetic field, it was possible to estimate the velocity of each electron from the curvature of the tracks. This gave a very powerful and delicate method for investigating the velocity spectrum of the emitted electrons. By taking a series of photographs it was possible to form a statistic of the different amounts of curvature, and so to construct directly a curve of the type shown in Fig. 2. It was soon found that there were a small number of

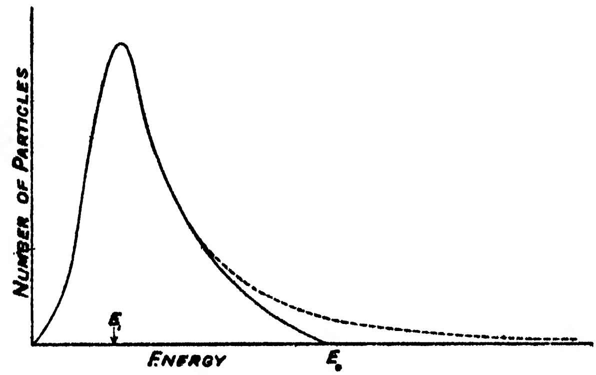

FIG. 2.—Distribution of Energy of the β-particles from Radium E.

tracks with speeds greater than that corresponding to the upper limit E_0. The number of these tracks was certainly very small, and by any other method of investigation would have been almost impossible to detect, but owing to the preciseness of the cloud-track method there could be no doubt that these electrons were actually emitted by the radio-active substance. This experimental method has as yet only been applied to radium E, but it seems unlikely that this body is different from the other β-ray bodies, and we must change our orientation to the problem, and conclude that in the β-ray type of disintegration an atom has a finite chance of disintegrating with any energy.

There is first of all a difficulty of a very general nature which is created by these results. If we are to believe in the detailed validity of the principle of the conversion of energy, we must conclude that the nuclei of an atom which has just shown a β-ray disintegration must have different internal energies. Now, there are plenty of cases where the next disintegration is

of the α-ray type where, as we have emphasised, the disintegrations are so identical that it is difficult to believe that the nuclei have different energy contents. It is, of course, possible to imagine that there is some sink of energy in the nucleus which can be used to take up or supply the energy differences required without having a detectable effect on the sharply quantised α-particle levels. Any such theory is obviously very unsatisfactory, and would be almost impossible to test. No serious attempt has yet been made in this direction.

When we attempt to apply some type of wave mechanical picture similar to that used for α-particles, we obtain still more incomprehensible results. We have seen that the general evidence of the latest work seems to indicate that a nucleus may in its disintegration emit an electron of any energy. This is equivalent to assuming that the curve of Fig. 2 never actually reaches the axis, and that there is no end point even at higher energies, and while this has not been proved, it appears to be the most reasonable deduction from the experiments. If we argue in the same way as was successful for α-particle disintegration, we are therefore forced to conclude that there may be electrons of enormously great energy in certain of the nuclei of a specified radio-active body, whereas other nuclei will have the corresponding electron in a state of very low energy. This conclusion is so improbable that we must doubt whether it is true in this case that the energy of the electrons outside the nucleus gives us any indication at all of the energies they have inside the nucleus.

The diagram in Fig. 2 seems closely similar to that representing the distribution of energies among the molecules of a gas, and one is naturally led to look for some statistical cause for the dispersion of the energy. While there would be no essential difficulty in considering that the final velocity of emission was subject to the laws of chance, there is a marked difference in this case from that of the Maxwellian distribution of the energies of molecules in a gas, which is equivalent to denying the principle of the conservation of energy if we are to apply the same ideas. In the case of the gas the distribution of velocities is due to the random factors governing the collisions, but the conservation of energy is maintained because there is a constant interchange of energy between different molecules, so that what one molecule loses another gains. Now, it has been abundantly proved that the whole process of disintegration of a nucleus is entirely unaffected by the amount of radio-active material present. The disintegration follows the same laws, whether there are few or many atoms present, and this can only mean that there is no interchange of energy between different nuclei. If then we consider that the nature of the

phenomenon shows that it is a statistical effect, then this statistical effect must occur entirely inside the nucleus, and coupled with this view we must abandon the application of the conservation of energy to this peculiar problem.

Conclusions such as these are distasteful and forced, and one cannot help thinking that there is something far more fundamental behind this phenomenon, and that it will not be explained by what might be termed "patching" the existing theory. The fundamental problem involves the consideration of an electron in an extremely minute space, one that is comparable with what is ordinarily accepted as the radius of the electron. The electron must be thought of as a wave motion, and the conditions of the wave motion will be determined by the distribution of electric potential in the space. The electric potential in this theory is somewhat analogous to the refractive index in the propagation of light, but there is the additional complication that, whereas the refractive index is a property only of the medium and does not change with the intensity of the light, the electric potential does depend on the presence of the electron. The successes of the wave mechanics have been in those regions where the electric density was so small that it was possible to specify a stationary distribution of potential and neglect the fluctuations due to the motions of the electrons which were being considered. In the nucleus everything is so much more condensed that this approximation is no longer valid, and one has to consider, not the motions of an electron in a specified space, but rather the motions of an electron in a space the peculiarities of which are largely conditioned by the actual motion itself.

No solution of these difficulties has yet been proposed, but we shall see that the second point that is being considered in this article presents difficulties which can be referred again to the same fundamental problem of the behaviour of the electron in the nucleus.

The Internal Conversion of the γ-Rays

In a great many radio-active disintegrations there is an emission of electro-magnetic radiation in addition to the ejection of the particle either α or β from the nucleus. These radiations are termed the γ-rays. They are of much shorter wave-length than X-rays, and correspondingly more penetrating. The γ-rays from certain radio-active bodies—radium C for example—in passing through 1 cm. of lead only have their intensity cut down by one-half, whereas ordinary X-rays would be reduced to half-intensity by ¼ millimetre of lead. It follows that the frequency of γ-rays will be very much higher than that of

X-rays, and it is usual to express this by stating what is called the corresponding voltage. To generate X-rays in an ordinary X-ray tube requires potentials of 20,000 to 80,000 volts, but to obtain radiation from an X-ray tube of the same frequency as the γ-rays from radio-active substances would require potentials of the order of 300,000 to 3 million volts.

It has been shown that these γ-rays are emitted immediately after the actual disintegration, and it is not difficult to form a general idea of the process of emission. Before the disintegration the radio-active nucleus is in a relatively stable state, and except for the potentiality of disintegrating at some subsequent moment is in every respect analogous to an ordinary inactive nucleus. The constituent particles must therefore be considered to be mutually in equilibrium. It is clear therefore that the departure of one of these particles will upset this equilibrium, and that a rearrangement will be necessary before a new equilibrium state is reached. The γ-rays are considered to be emitted as a result of this rearrangement. This is more usually expressed by stating that the newly formed nucleus is left in an excited state, and that transitions are performed which carry it to its ground state, the energy liberated in this process being emitted as radiation in the usual quantum manner. On this view the γ-rays would be expected to be mono-chromatic, and to be associated with the characteristic energy states of the nucleus. We can extend this picture, perhaps a little hypothetically, by remembering that the general results show that whereas the electron in the nucleus appears not to be sharply quantised, all the evidence goes to show that the α-particles do exist in quantum states. We therefore imagine that the γ-rays are emitted by transitions of α-particles in just the same way as the optical of X-ray spectra are emitted by transitions of electrons between the stationary states of the atomic structure.

The γ-rays thus constitute the characteristic spectrum of the nucleus, and have the same importance for the study of its structure as have the optical and X-ray spectra for the structure of the atom. The only essential difference is that the γ-ray spectra are self-excited. The information that can be deduced from the γ-rays is of great importance in studying nuclear problems, and considerable attention has been directed towards them. The spectra are found to be in most cases very complicated, pointing to the existence of a whole series of possible excited states of the nucleus.

Several different methods have been developed for determining the wave-lengths and investigating the intensities of the γ-rays. The problem is slightly different from that presented by X-ray spectra, owing to the much higher frequency.

For example, the crystal method which is so much used for X-rays has only a limited applicability to γ-rays owing to the glancing angles at which reflection occurs being so much smaller. The method which has proved most generally successful is based on the photo-electric effect. When radiation of frequency ν falls on matter, the energy that is absorbed is taken up by the atoms in definite discrete quantities of magnitude $h\nu$, where h is Planck's constant. If the frequency is high enough, this energy $h\nu$ will eject electrons from the atoms of the substance, and to a first approximation the energies of these electrons will be equal to $h\nu$. Now, these electrons cannot

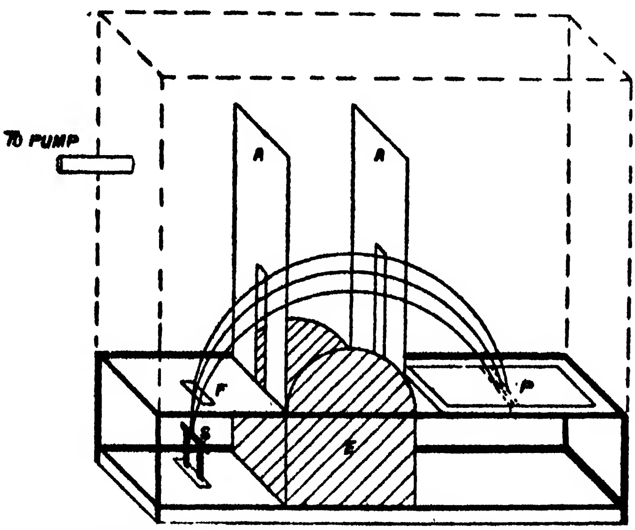

FIG. 3.—β-ray focusing apparatus. S. Radio-active source. F. Slit. P. Photographic plate. A.A. Screens. E. Lead to reduce effect of γ-rays.

be observed until they are clear of the atom, and a certain amount of work must be expended merely in removing them from the position in which they are held in the atom. Thus from a given atom radiation of frequency ν will lead to the ejection of electrons of several different energies $h\nu - K$, $h\nu - L$, etc., according as to whether the electron was originally in a K or L state, etc., of the atom. The method of using this effect to investigate the γ-rays of radio-active substances is illustrated by Fig. 3. At S is placed a small tube about 0·3 mm. in diameter and, say, 10 mm. long, containing the radio-active substance, and this is surrounded by a cylinder of some metal such as lead, about 0·3 to 0·5 mm. thick. The γ-rays emitted from the radio-active material traverse the lead, and lead to the ejection of photo-electrons. Considering now the γ-rays

of one frequency ν, we see that there will be an emission from the lead cylinder of several groups of electrons of different energies, the slowest being $h\nu - K$, the next being $h\nu - L$, and so on. A portion of this beam of electrons passes through the slit, and if the apparatus is placed in an evacuated box, in a magnetic field at right-angles to the plane of the diagram, the electrons follow circular paths, and each group comes to a focus as shown. If a photographic plate is placed here, each group of electrons gives a black mark. If the radio-active material emits γ-rays of several different frequencies, there will, of course, be a great many of these groups of electrons, but the analysis of these corpuscular spectra, as they are called, is greatly facilitated by the fact that the groups from the K level are by far the most intense. The problem then of measuring the frequency of the γ-ray is to identify the corresponding photo-electric group, to determine the energy of the electrons from the position of the line on the plate, and then to deduce the frequency from the equation $h\nu =$ energy of group + absorption energy. If another metal such as gold were used instead of lead, the spectra obtained would be similar, except that all the groups would have slightly greater energies, owing to the work of extraction of electrons from gold atoms being less than that of lead atoms. The traces on the photographic plate are found to be rather diffuse on the low-energy sides, and this is a natural consequence of the experimental arrangement. Electrons that come from the top layer of atoms will escape with their full energy, but while electrons ejected from atoms in the body of the lead tube will leave the atom with their full energy, part of this will be lost in collisions before they actually emerge from the tube. It is these electrons which cause the diffuseness of the line on the low-energy side.

We will now suppose that the source previously used at S in Fig. 3 is taken away and replaced by fine platinum wire coated with radio-active material. The normal amounts of material that are available would only serve to form about a mono-molecular layer. If now photographs of the electronic emission are taken in exactly the same way as before, then on the photographic plate there will be a general blackening due to the continuous spectrum formed by the disintegration electrons which were discussed in the previous section, but in addition it is found that there are a series of lines almost exactly similar to the groups ejected from lead by the γ-rays. A close examination shows that the only difference between this natural β-ray spectra and the corpuscular spectrum from lead is a slight difference in the energies of the groups. This can be exactly accounted for if these groups are assumed to originate by the photo-electric effect of the γ-rays on the radio-active

atoms. Now, we have already mentioned that the layer of radio-active material is extremely thin, and it follows that this photo-electric absorption must occur in the same radio-active atom that emits the γ-ray, since the chance of a γ-ray being absorbed in a neighbouring radio-active atom would be small compared with the chance of its being absorbed in a platinum atom, of which there are so many more. In agreement with this conclusion, it is found that in this case the groups are sharply defined, since the electrons originating in the thin layer of material on the surface of the wire do not suffer any loss of energy in escape.

It is clear that this phenomenon of the internal photo-electric effect is one of great interest, since it involves the absorption of γ-rays within distances comparable with or less than the wave-length. We would scarcely expect the ordinary absorption laws to hold for these very small distances, but the general evidence about the independence of the nucleus from the electronic structure would lead us to expect the transfer of energy to take place by electro-magnetic radiation. A suitable model for calculation of such an effect can be made as follows. We imagine a simple Hertzian oscillator to be located in the nucleus at the centre of the atom, and to be vibrating, so that it emits electro-magnetic radiation of frequency ν. At short distances comparable with the wave-length of the radiation, the forces are certainly different from those at large distances, but they are completely defined by current theory, and the effect on the electron moving in the K level can be calculated, and in particular the rate at which energy is transferred to this electron can be determined. It is then concluded that the ratio of the rate of transfer of energy to the electron to the rate of emission of radiant energy by the oscillator will be the same as the probability, in the actual atomic problem, of a quantum of radiation emitted from the nucleus being absorbed in the electronic structure and leading to the ejection of one of these natural photo-electrons. Calculations of this kind have been carried out, and the most recent results obtained by Casimir, using the modern wave mechanical picture of the electron, are shown in col. 3 of the following table:

Energy of γ-ray in volts × 10^{-5}.	Measured value of the conversion coefficient × 10^4.	Calculated photo-effect × 10^4.
6·12	61	4·6
7·73	48	3·7
9·41	61	3·3
11·30	62	2·8
12·48	57	2·6
13·90	14	2·4
17·78	16	1·8
22·19	13	1·6

These measurements refer to the γ-rays of the body radium C, and the first column shows the frequencies of the γ-rays expressed as is usual in volts. The second column shows the measured value of the internal conversion coefficient. This gives the probability of a quantum being absorbed in its own atom before it can escape, for example, the figure 60×10^{-4}, or ·006, shows that in 994 disintegrations out of 1,000 the quantum escapes and in 6 it is absorbed. The third column shows the results of the calculation, and the entire absence of agreement is most striking. Not only are the calculated values far too small, but in addition, while they decrease regularly with the increasing frequency of the γ-ray, the measured values do not show any such behaviour. The only conclusion is that the picture of the phenomenon on which this calculation is based cannot be correct. Now, the essential feature of this picture was that the radiating structure in the nucleus was definitely separate from the electron which absorbed the energy, and while this would be inevitably the case on the older particle view of matter, it is not true on the modern wave mechanics. On this theory the electron is to be thought of as a wave motion, and while this is mainly confined to the region near the Bohr K-orbit, a certain part of the wave function will exist throughout the volume of the nucleus. We may visualise this conclusion by stating that, although the electron is mainly in the K-orbit, it has a certain probability of penetrating and passing right through the nucleus from time to time. There is thus the possibility of an additional interaction of a completely different kind. Now, at this stage it is convenient to think more accurately about the emission of the γ-rays. We have seen how we can imagine them to be due to transitions of an α-particle from the excited state in which it is left by a previous disintegration to its normal state. An α-particle which is thus left in an excited state can make a transition to its normal state and emit radiation, the probability of whose conversion in the atom is probably fairly accurately given by the figures in col. 3 of the above table. In addition to this effect, however, we have concluded there must be that due to the penetration of the K or L electron into the nucleus, while the α-particle is still in an excited state and before it has a chance to radiate. There would then occur something analogous to a collision of the second kind in which the excess energy of the α-particle would be transferred directly to the electron which would escape from the atom, and the α-particle would be left in its normal state.

We can feel with some confidence that we are on the right lines in assuming that there are these two main methods by which energy is transferred from the nucleus to the K electrons, either by radiation or by what we will term the collision effect.

It is further reasonable to assume that the order of magnitude of the radiation effect is given correctly by the theoretical calculations as shown in col. 3 of the above table. The differences between the values in col. 2 and col. 3, therefore, show that the magnitude of the collision effect is about ten times the magnitude of the radiation effect. Now, the collision effect has been calculated approximately on current theory, and the important fact emerges that it should be considerably smaller than the radiation effect. We are thus faced with a divergence by a factor of at least 20 to 30 between the theory and experiment as regards the magnitude of this collision effect.

A disagreement of this kind is so striking that we cannot believe it to be due to any serious error in calculation. It must be due to something wrong in the fundamental postulates on which such calculations are based. When we come to examine the picture we have made of this collision effect, we cannot help but feel that qualitatively it is correct, in the sense that it postulates the presence from time to time of the electron in the nucleus, but then we are at once reminded of the fact that we have already encountered difficulties in dealing with the electron in the nucleus. Whether there be logical grounds for it or not, many people will feel a suspicion that the crux of the problem lies essentially in the behaviour of the electron in the nucleus. The two lines of thought which have led us to this conclusion are different in this respect, that the second, the internal conversion of γ-rays, can be explained qualitatively but not quantitatively, whilst the continuous spectra of β-rays appears to introduce a more fundamental difficulty involving the energy principle. It is therefore possible that the former might be explained by an emendation of the existing theory, which would leave the latter still unexplained. An alternative attitude, however, is to look at these two phenomena as definite signs that the wave mechanics is only a correct guide to those atomic problems where the distances involved are not too small. The wave-length of the material wave which describes the α-particle bears somewhat the same relation to the dimensions of the nucleus, as does the wave-length characterising the electron to the dimensions of the atom, and it is not therefore surprising that a theory which is competent to deal with the latter can also deal with the former. To treat the electron in the nucleus, however, we have to meet and solve problems where on current theory we should expect the wave-length of the material wave to be large compared to the dimensions of the nucleus. That difficulties of a fundamental nature immediately appear is only to be expected, and whether the existing theory will prove capable of modification to deal with such problems, or whether a new theory will be called into existence, only time will show.

SOME SIMPLER ASPECTS OF THE CORPUSCULAR THEORY OF LIGHT AND THE WAVE THEORY OF MATTER

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The new wave mechanics has found expression in a form so abstruse as to be almost discouraging to any but the specialist worker in that field. This has obscured the fact that many of the ideas involved are expressible in ordinary language and, as well as clearing up the general outlook, can be made to form a helpful and stimulating guide. It is the object of the present article to develop this aspect of the subject.

The best starting-point is the assumption of Planck that radiant energy E is only to be found in units whose energy is proportional to the frequency ν, so that

$$E = h\nu$$

where h is the well-known constant bearing his name. This theory was successful in many fields, but particularly in the way it accounted for the distribution of energy in the black body spectrum. A further extension due to Einstein rendered the photo-electric effect explicable, but, in so doing, conflicted with the accepted wave theory of light. Sufficiently short light waves falling on a metal surface eject electrons. As the intensity of the light is reduced, the number of ejected electrons falls off, but the velocity of ejection remains the same. The old-fashioned continuous wave theory proved powerless to explain this result. On the other hand, if light were conceived as a series of discrete units fired off from the luminous body, the way in which the ejected electrons fell off in number, but not in velocity, was easy to understand. The great difficulty was that interference now stood in the way.

> There would be a mighty clearance
> We should all be Planck's adherents
> Were it not that interference
> Still defies $h\nu$.[1]

[1] G. S., *The Post-Prandial Proc. of the Cavendish Soc.*, Bowes & Bowes.

G. I. Taylor [1] succeeded in obtaining perfectly sharp photographs of interference fringes with light so feeble that an exposure of 2,000 hours was necessary, and where the calculated concentration of quanta was only one in ten litres. Interference was thus almost out of the question, as there was but little for a quantum to interfere with.

An impasse thus appeared. Light from one point of view seemed almost certainly made up of discrete quanta, and yet from another point of view, it seemed almost equally certainly made up of continuous waves. The obvious solution of the difficulty was of course that it was both, but how such a thing was possible awaited the most interesting and suggestive theory of L. de Broglie.[2]

This theory supposed that when a light quantum was emitted, it was accompanied by a simultaneous emission of continuous phase waves. These waves differed from the waves of the classical theory in that they possessed no appreciable energy. All the energy was in the quantum, and this latter could move forward with the phase waves on any part of a wave front.

In the days of the elastic solid ether such a theory would at once have been discarded as having no mechanical basis. As Eddington [3] remarks: " It was the boast of the Victorian physicist that he would not claim to understand a thing until he could make a model of it; and by a model he meant something constructed of levers, geared wheels, squirts, or other appliances familiar to an engineer. Nature in building the Universe was supposed to be dependent on just the same kind of resources as any human mechanic; and when the physicist sought an explanation of phenomena, his ear was straining to catch the hum of machinery."

Much has been learnt since then, however. There are even some who have presented us with the rather discouraging picture of our having reached the limits of the knowable; but, without going thus far, it certainly does seem that our ordinary laws of motion are at least very incomplete when applied to such fine-grained structures as light waves and electrons. With this in view, the non-mechanical nature of de Broglie's theory need not worry us.

Remembering therefore that light quanta will only follow a track prepared for them by phase waves, we can return to the interpretation of the interference experiments of G. I. Taylor and others. Here it is the phase waves that interfere: the quanta merely follows the paths of the wave maxima,

[1] *Proc. Camb. Phil. Soc.*, **15**, 1909, p. 114.
[2] *Phil. Mag.*, **47**, 1924, p. 446.
[3] *Nature of the Physical World*, Camb. Univ. Press, p. 209.

and a photographic plate records the positions of the latter, whether the quanta, which affect the silver-bromide grains, arrive one at a time or in shoals. Likewise we can throw light on the old difficulty that if the photo-electric effect required that the quantum should be small and concentrated, the superior resolving power of a large telescope required that it should be large enough to fill the whole objective. It is clear now that it is the incident phase waves that perform this latter function and, at the same time, prepare a path whose precision, in the case of a star image, is determined by the diameter of the objective. The phase waves, in fact, behave like the classical waves except that they do not carry the energy : this is done by the quantum which moves with them. The outlook is thus very much cleared if the dual character of light waves be borne in mind.

One of the highly interesting discoveries of the last decade was that made by A. H. Compton [1] concerning the modification of the frequency of X-radiation on scattering. The classical wave theory, of course, was unable to predict anything else than that electrons should scatter radiation of the same wave-length as that incident upon them. Compton, however, was able to show that some of the radiation was changed in wave-length on scattering, and he found, moreover, that he could give a satisfactory account of his results by endowing the quanta with momentum, and by then treating the problem as a mechanical collision between these quanta and the electrons. This was a remarkable result, and has had far-reaching consequences. It is surprising that our laws of motion carry us so far.

Incidentally the possession of momentum by a quantum —or a " photon " as it is now sometimes called—enables us to take a very simple view of such phenomena as the pressure of radiation. The stoppage or reflection of momentum-carrying units would naturally be expected to produce a pressure, and many years ago, Lebedew, Poynting, and others were able to show, in a series of fascinating experiments, that this pressure is numerically equal to the energy contained in unit volume of the incident beam. If, then, we suppose that in this unit volume there are n quanta, and that they strike unit-absorbing surface in $1/c$ second (where c is the velocity of light), then the rate of change of momentum is nc times the momentum of the quantum. But the energy per unit volume is $nh\nu$, and therefore

$$\text{Momentum of a quantum} = nh\nu/nc = h\nu/c.$$

This result has been emphasised, as it leads up naturally to the question that if waves are accompanied by momentum-

[1] *Phys. Rev.*, **22**, 1923, p. 409.

carrying light corpuscles, are ordinary momentum-carrying material particles accompanied by waves? De Broglie assumed an affirmative answer to this question [1] and, moreover, assigned to the waves a definite length.

We can understand his result in a general way by a simple comparison. The wave-length λ associated with a quantum can be written

$$\lambda = c/\nu = \frac{h}{h\nu/c} = h/\text{momentum of quantum}.$$

The length of wave that de Broglie regarded as accompanying a moving material particle was given by

$$\lambda = h/\text{momentum of particle}.$$

The two cases are thus closely analogous.

At the time that it was made the theory seemed, no doubt, to many as fantastic or improbable, yet within about three years Davisson and Germer [2] and G. P. Thomson [3] had succeeded in demonstrating the existence of the waves. When electrons of definite velocity were fired at either a single crystal surface or at a thin foil consisting of many crystals, the reflected or transmitted electrons gave a pattern similar to that which X-rays would have given under the same circumstances. The calculated wave-length, moreover, was in good agreement with de Broglie's theory. This truly remarkable result, perhaps more than any other, makes us realise the insufficiency of our ordinary dynamics in dealing with such fine-grained phenomena as the motions of electrons. Evidence is accumulating that the influence of the waves is perceptible even in the case of atoms.

Wave mechanics, as it is now called, has, in the hands of Schrödinger, Heisenberg, Dirac, and many others, gone forward at a great speed—so great indeed that the vanguard of the fight is almost out of sight of the rank and file, and the issues are clouded by the dust thrown up in the conflict of matrices, eigen functions, and multi-dimensional co-ordinates.

To return to the simpler point of view, however, we can trace the influence of the de Broglie waves not merely in guiding the electrons through crystals, but actually within the atom itself.

If an electron of charge e and mass m moves round a nucleus of charge E in a circular path of radius r with velocity v, then we may write

$$Ee/r^2 = mv^2/r.$$

[1] *Phil. Mag.*, **47**, 1924, p. 446.
[2] *Phys. Rev.*, **30**, 1927, p. 705.
[3] *Proc. Roy. Soc.*, 1928, pp. 600 and 651.

The waves associated with the electron have a length λ given by

$$\lambda = h/mv.$$

Eliminating v between these two equations we have

$$\lambda = h\sqrt{r}/\sqrt{e\mathrm{E}m}.$$

Let us now compare this wave-length with the length of the orbit $2\pi r$. If the latter is not an exact multiple of the former, the wave train, in running round and round the orbit upon itself, will give rise to destructive interference. On the other hand, if the orbit is once, twice, three times the wave-length, then something rather similar to an ordinary stationary wave-system will be formed. Let us therefore make $2\pi r/\lambda$ equal in succession to 1, 2, 3, 4, ... n.

Clearly r is given by

$$r = n^2h^2/4\pi^2e\mathrm{E}m.$$

This expression we at once recognise as giving the radii of the circular Bohr orbits—a very significant result.

The new theories are thus full of meaning and suggestion. A purely mechanical basis we must admit is thrown to the winds, and with it, of course, goes the simplicity of our mental picture. It would seem as hard to imagine an electron guided by its waves as to imagine an aeroplane guided by the hum of its engines. It will take us time to get used to the new point of view. The behaviour of gross matter is well described in terms of our laws of motion, but it is surely not surprising, when we reach particles so far beyond our experience as electrons, that these laws should at least need supplementing by a consideration of the waves that apparently accompany the electrons in their paths.

"RELATIVITY" AND ORGANIC CHEMISTRY

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With the introduction into scientific philosophy of the Theory of Relativity, and of the still more revolutionary Principle of Indeterminancy, the quasi-axiomatic precepts of almost all branches of science have been subjected to critical review in recent years, with the result that, if nothing else has happened, the fundamental arguments for their main principles have been brought again into due prominence for open discussion.

Though organic chemistry, of all sciences, seemed to have acquired a safe foundation, based upon the theories of Kekulé, Van't Hoff, Baeyer, and their nineteenth-century contemporaries, from which had been planned so much successful synthetic work of great use both to industry and to medicine, yet modern scientific enquiry has revealed how uncertain has been this original foundation, and how much justification there has been for the action of those earlier " reactionary " critics who refused to avail themselves of such theoretical aids as "structural formulæ " without considerable reserve.

In general, the graphic formulæ of Kekulé and the three-dimensional models of Van't Hoff, modified but little by the " strained bond " conception of Baeyer and the " partial valency " theory of J. Thiele, have satisfied organic chemists, in spite of the great developments of modern physics in revealing the structure of the atom and the properties of the electron. For, whatever the chemical bond may be, whether localised " fields of force " or " shared electrons " either still or moving in complex orbits, yet both the graphic formulæ, and the old mechanical models, have sufficed to depict to high degrees of accuracy where, if not how, atoms are bound together within molecules. Investigators of the reasons for the occurrence of chemical reactions have been well aware of the failings of the usual diagrammatic representations, but many workers, interested only in the final results of their experiments, have been satisfied with their customary mode of expression. To some of the latter it has been a shock to find that the centre point of conventional stereochemistry—*viz.* the tetrahedral carbon atom—has been challenged from the home of relativity by Karl

Weissenberg [1]. Nevertheless, although the experimental grounds for the initial iconoclastic attack have since proved to be inadequate, the outspoken criticism has been welcome in reviving interest in what has been a far too dogmatically standardised subject for any true experimental science.

The hypothesis of the tetrahedral distribution of the valency bonds of the carbon atom in reality forms the basis, not only of the stereochemistry of carbon compounds, but of all structural chemistry, for from it, either by correlation or by analogy, have been developed structural configurations for other elements and for the complex compounds which can be built up from them. "Experimental verifications" of theoretical structures of complex compounds have been wonderfully successful, and many of the deductive investigations, such as those of Emil Fischer on the stereochemistry of the complex sugars, will always remain prominent as classic examples of true scientific reasoning. None of them, however decisive, can change the *theory* of the tetrahedral carbon atom from an hypothesis into a proven fact, for, after all, it is a theory expressed in terms of models, and in terms of these same models only is it reasonable. Even "Valency Bonds" are in reality quite as much pictorial concepts as the wooden rods and balls used to illustrate them, and must remain conceptual ideas even if interpreted further either as the tubes of force of Faraday or as the orbits of the spinning electrons of our modern physicists.

The realisation that chemical models cannot give pictures on the macroscopic scale of actual molecules need not form an argument to detract from their utility, provided the user is always quite sure of exactly what set of experimental properties any one model is intended to depict. Consequently one can employ usefully several entirely different sets of models for the representation of the same chemical molecule, no matter if they seem mutually contradictory. The underlying theoretical standpoint of stereochemistry, however, should be independent of any system of representation. This latter is the aspect of theoretical chemistry which is stressed by Weissenberg. He insists that one can never determine experimentally more than the symmetry of a chemical molecule as a whole, frequently not more than the average symmetry of an aggregate of molecules, and certainly not the actual relative positions of the individual atoms within any one isolated molecule. For example, it has been shown that the *symmetry* of methane and its derivatives is tetrahedral, but not that the carbon atom is surrounded by four other atoms in the geometrical positions of the regular tetrahedron. In methane the hydrogen atoms are all exactly equivalent to one another [2], and by no possible

experimental means can any one of them be differentiated sufficiently from any other for the requisite angular measurements to be made. No stereochemical researches have done more than determine the symmetry of whole chemical molecules, and several in the last twenty years have demonstrated the absurdity of evaluating molecular asymmetry only by the presence or absence of " asymmetric carbon atoms."

The belief in the asymmetric tetrahedral carbon atom as the originator of all geometrical and optical isomerism in carbon compounds has actually grown up through a gradual but unwarranted dogmatisation of some aspects of the theories of the early investigators of stereochemistry [3]. Historically, the theory developed from the realisation that molecules had definite structures in three-dimensional space in which mirror image " dissymmetry " was possible, so that for the quadrivalent carbon atom the pictorial representation of the valency bonds had to be tetrahedral and not square (*i.e.* uniplanar). Le Bel, for example, would never commit himself to the definite statement that valency bonds had relatively fixed directions in space, but considered that they might adjust themselves from compound to compound, or according to any experimental conditions affecting the resultant stability [4]. Actual molecules, nevertheless, had a perfectly definite and discernible degree of symmetry or asymmetry.

In actual experience it is seldom necessary to attempt the elucidation of molecular structure by utilising nothing more than an *a priori* examination of the three-dimensional symmetry or asymmetry of a whole molecule. Usually, the experimentally discoverable symmetry of organic molecules corresponds almost exactly with that of the structural model made up of tetrahedral carbon atoms, so that the more precise theory (even if less exactly true) is much the more convenient for practical application. In the same way exactly Newtonian mechanics is still of much more practical utility than that of Einstein.

Among simple types of organic compounds, however, there are a few substances, chiefly derivatives of penterythritol, $C(CH_2OH)_4$, for which a tetrahedral model does not *necessarily* give the true degree of symmetry. It has been pointed out (1) that, in penterythritol, the four carbinol (CH_2OH) groups *may* be more " equivalent " to one another—*i.e.* structurally less distinguishable from one another—if arranged in a planar, or pyramidal, configuration around the central carbon atom instead of in a tetrahedral configuration. Geometrically speaking, molecules, like crystals, have their individual portions the less distinguishable from one another the higher the degree of symmetry, and it is an open question, in the case of these particular compounds, as to which of these alternative

configurations is to be regarded as the more symmetrical.[1] Quite possibly the molecules in question exist, possessing the two structural forms as tautomerising isomers, so that any chemical evidence adduced from the reactions and behaviour of derivatives is not necessarily conclusive [5]. Purely physical measurements might be expected to solve this structural problem with greater ease, but here again indications so far appear to be inconclusive. Crystal structure evidence, from examination of several different derivatives of penterythritol, now indicates that the stabilised solid molecules have the normal tetrahedral structure, but it may be mentioned that the determination of the actual space group of these molecules has been a matter of considerable difficulty, the alternative tetragonal (pyramidal) configuration frequently appearing highly probable [6].

Physico-chemical measurements of the same substances in solution by no means eliminate the possibility of the existence of alternative structures. Dipole moment measurements of some of the compounds in question, for example of penterythritol itself and of its esters, indicate the presence of decided molecular moments, requiring therefore a degree of molecular symmetry more possible with a pyramidal molecule than with a tetrahedral molecule. With other compounds of the same general type, however, for example carbon tetrachloride, no such molecular dipole moment is observable. The conflicting results may of course be ascribed to the presence of oxygen atoms, which are indubitably not of linear symmetry, in all those compounds possessing dipole moments, but they can equally well be claimed to support the view that molecules can undergo structural changes so as to adjust their symmetry and stereochemical configurations to suit the potential energy requirements of their environment [7]. Much more could be said in support of this latter, less stereotyped, conception of molecular structure, which is, of course, in exact agreement with the stereochemical viewpoint of Werner, though it does not necessarily require the acceptance of his theories of valency.

The theory of valency adjustment according to energy requirement will of course account easily for such puzzling experimental facts as the Walden inversion, racemisation, epimerisation and mutarotation, although still other theories are required to explain the *mechanisms* of such chemical changes as these. Furthermore, this theory alone can explain adequately several new stereochemical properties of ring systems which have been discovered experimentally in the last ten

[1] The alternative configurations have tetragonal pyramidal and tetragonal bisphenoidal symmetries respectively, each of which can be built up from eight asymmetric space-lattice units.

years. It is well known that all the carbon, or hydrogen, atoms in both benzene and cyclohexane are exactly equivalent —*i.e.* indistinguishable from one another by any chemical or physical test. Further, the symmetry of all derivatives of these compounds is that of a model possessing a planar, regular hexagonal carbon ring, whereas any corresponding structural model built up of regular "tetrahedral carbon atoms" is much less symmetrical. Many of these possible structural models, employing only tetrahedral carbon atoms, were suggested during the nineteenth century, but, for reasons of practical utility, most have been abandoned for the more symmetrical plane ring structures which recognise in principle the "strain theory" of Baeyer, according to which atoms can join up into rings after deflection of the valency forces from their normal directions. This valency deflection is usually considered to necessitate the locking up of a certain amount of energy, as in the bending of a spring, but it may originate, in part, merely by the spatial requirements of surrounding groups, as Thorpe and Ingold have indicated in a whole series of experimental papers.

The planar strained ring of Baeyer, however, is not of necessity that possessing the least internal energy, if more than five atoms are involved, for, as Sachse pointed out in 1890, rings having only the normal tetrahedral angle (109° 28′) at each atom can be constructed if they are neither planar nor symmetrical.

Baeyer's own classic stereochemical researches with six membered ring compounds, however, indicate that a six carbon atom ring system definitely has hexagonal symmetry, not consistent with the strainless structure of Sachse. Nevertheless, recent investigations by W. Hückel in Germany, by S. G. P. Plant, and the late W. H. Perkin in this country, and by several others, have shown that when saturated five or six membered rings are fused together into systems of the type of decahydronaphthalene, more isomeric compounds can exist than can be accounted for by planar ring structures. Ring systems can become fused together in the "trans" positions, and, structurally, this seems possible only if one employs the strainless ring models of Sachse. An explanation, which moreover was a prediction, of these last discoveries was put forward in 1918 by E. Mohr, but it abandons the supposition that molecular structures are rigid and calculable. Mohr conceives strainless (large) rings as being mobile structures in which the relative positions of the atoms can change easily, with very little corresponding energy change, from one form of structure to another. For any strainless ring system, where this relative movement of atoms is possible, the resultant observable symmetry is that

of the intermediate structurally plane form. Only when two or more rings are fused together, having *two* atoms in common, is this complete interchangeability of structural form impeded. Then, declared Mohr, should isomerism be manifested, and, in fact, the experimentally observed isomerism does correspond exactly to his theoretical predictions. In modern terminology, one can say that valency adjustment is possible so that the greatest possible symmetry equivalence between the atoms is attained. Even when locked together into rings, atoms appear to have power to move, without actual rupture of valency bonds, so as to yield molecules of the highest degree of symmetry possible.

Actually, as the relativitist would point out, only the resultant symmetry of our chemical molecules, inspected in the aggregate, is observable, and of the actual relative positions of the various atoms, one can be certain of very little. To the pure stereochemist an atom is " a something " having a sphere of influence, as postulated by Werner, which can be built up, subject to the limitations of valency number, into compounds of definitely recognisable three-dimensional symmetry. If the macrocosmic scale model of the chemical molecule indicates this symmetry, it is a correct and useful model, however ridiculous may appear the mechanical contrivances used to represent the individual atoms. In the same way the fantastic astronomical orreries of our museums both had, and have still, a practical use, for not all astronomers, chemists, physicists, or mathematicians either can comprehend the scientific abstractions of symmetry, space, and time without mechanical and pictorial aid. Both abstractions and models have their uses, though unless one realises their limitations as much as their advantages one does not obtain the utmost possible value for their services.

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MODERN TECHNIQUE IN THE INVESTIGATION OF OPAQUE MINERALS AND ORES

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I. Introduction

Petrographic investigations, and largely also petrologic theories, have in the past shown a strong tendency to confine themselves almost completely to those minerals which are transparent in thin sections. The technique of the microscopical investigation of these minerals has reached a high pitch of perfection since the pioneer work of H. C. Sorby. The major rock-forming minerals are almost without exception of high transparency, but no really complete theories of petrogenesis (or, still less, of the ore-formation so often an essential and integral stage of the same process) can be attained until there is available a detailed petrography of those minerals in which the factors of absorption and reflection of light are all-important—the opaque minerals.

Isolated instances of the application of metallographic methods to the study of opaque minerals can be found far back in the literature of the last century, but the branch of petrographic investigation, at present variously known as mineralography, mineragraphy, chalkography, or ore-microscopy, was virtually initiated by the work of Campbell at Columbia, his first detailed paper being published in 1906 [1]. For the next fifteen years the work was confined almost entirely to America, methods being gradually developed and improved upon at a number of institutions. The two most important publications of this period dealing with the practical side were Murdoch's book [2], basing identifications largely on colour-distinction, and that of Davy and Farnham [3], in which an attempt was made to replace these distinctions by etch-reactions.

The post-war decade is distinguished by two significant developments: firstly, a great improvement in the optical methods of investigation, with an at least partially successful introduction of a quantitative element; and secondly, the growing use of physico-chemical theories as a guide to further

investigation of complex ores. Koenigsberger had, some ten years earlier, published descriptions of apparatus for determining anisotropism of opaque minerals, and Wright in 1919 [4] gave a rigid mathematical discussion and descriptions of various methods of procedure. Mineragraphic investigation was enthusiastically introduced into Germany by Schneiderhöhn, who had developed the methods largely independently in South-West Africa. Berek's contribution to Schneiderhöhn's book [5], and a later paper by Schlossmacher [6], are useful outlines of the theoretical basis, and bring us to the period which it is proposed to review in detail below. The number of papers now appearing from various centres in America, Canada, Germany, France, Holland, Russia, Rumania, and other countries afford an indication of the growing interest in opaque minerals.

II. The Preparation of Polished Surfaces

The grinding and polishing processes of the earlier workers were essentially those of the metallographer. Whilst in general satisfactory for large specimens of single minerals, it soon became apparent that a complex ore-specimen containing a number of different minerals (whose physical properties—hardness, brittleness, and so forth—may vary very widely) demands special treatment. Descriptions of modern procedure can be found in papers by Short [7], Schwartz [8], or in standard textbooks such as those of Schneiderhohn [5], and van der Veen [9]; a surface free from even minute scratches can usually be produced in less than ten minutes, and it seems unlikely that further work will ever seriously modify this technique for routine investigations. An important paper by Vanderwilt [10], however, presents a modified technique of application in special cases. By long-continued grinding on lead-surfaced laps with a minimum quantity of abrasive, a surface may be produced free from those inequalities between hard and soft ores which, whilst ordinarily no disadvantage and of considerable diagnostic value, may mask relations at boundaries or in fine intergrowths.

III. Optical Determinations

1. *Determination of Reflecting Power*

The reflecting power of a polished surface may be defined as the ratio of the intensity of the reflected light to that of the incident light for normal incidence. Drude and others have shown how this ratio is connected with the indices of refraction

and absorption, the simplest case being that of an isotropic body, for which:

$$R = \frac{(n-1)^2 + n^2k^2}{(n+1)^2 + n^2k^2},$$

where n is the refractive index and k the index of absorption. In a few cases n and k have been separately determined, and hence R calculated for various wave-lengths. The main use of such determinations, however, is to provide a standard by which more empirical determinations of relative reflecting powers may be referred to an absolute basis. The rapid determination of R is accomplished in practice by one of two methods—an adaptation of visual photometry or the use of a photo-electric cell.

Schneiderhohn has perfected, in conjunction with Berek, a simple form of photometer adaptable directly to the reflecting microscope, and not interfering with ordinary visual observation. An account of the theory of the instrument and a first series of results are given by Frick [11]. Light incident on the vertical illuminator is partly reflected before reaching the totally reflecting prism, and travels to the eyepiece field through an auxiliary tube by way of two nicols. The portion of the incident light transmitted through the illuminator falls on the specimen, and the reflected beam traverses the usual path, and illuminates an adjacent portion of the divided eyepiece field. The intensity of the direct beam is reduced to equality with that of the reflected beam by rotation of one nicol relative to the other, the rotation in successive cases giving a quantitative relationship between the respective reflecting powers. Frick worked in light of three different colours as yielded by Lifa filters, and compared his results with a polished plate of watchspring steel as temporary standard. By using the results of Hagen and Rubens for the reflecting power of steel, he calculates and lists an absolute reflecting power for over a hundred minerals. The degree of agreement shown in some cases in which the reflecting power has also been determined from direct measurements of n and k is indicated in the following table, where R has been calculated from values of n and k given by Drude and others (Orcel [15], and van der Veen [9]).

	R	
	Calc (Na light)	Frick (orange).
Silver	95·0	94
Gold	85·1	77—82·5
Copper	73·2	73
Platinum	70·1	65
Iron	56·1	53·3
Galena	44·3	36·4
Sphalerite	17·2	17·7

Orcel [12] has replaced the visual photometer by a photo-electric cell mounted directly on the microscope body-tube. A sliding reflecting-prism can be introduced to screen the light from the cell and deflect it into an eyepiece, so that with this method also visual observation is not interfered with except during actual measurements. The intensity of the reflected beam is proportional to the current, measured directly by a galvanometer connected in circuit with the cell, and as in the photometric procedure comparisons of relative reflecting powers are made with a standard mineral, such as galena. (With this method pure monochromatic light may be used, the current in this case being amplified by means of a valve amplifier; by comparing directly with the standard for any given wave-length the unequal sensitivity of the cell over the range of the visible spectrum is discounted.)

Frick states, in discussing Orcel's work, that the values given for the relative reflecting powers of galena and antimonite do not allow of comparison with his values or those of other observers, as they are determined only in ordinary light. The results so far published certainly refer to determinations in white light only, but it is satisfactory to note at any rate a general agreement with those obtained by visual photometry. Frick gives for antimonite R = 35·0 per cent. in the orange, and for galena R = 36·4 per cent., giving a reflecting power for antimonite of 95·5 per cent. relative to galena. Orcel's figures, taking into account the anisotropic character, but working in white light, range from 70 per cent. to 105 per cent. for the three principal crystal faces; Frick's figure of 30·8 per cent. for bournonite leads to a value of 84·6 per cent. relative to galena; Orcel's figures range from 84 per cent. to 90 per cent. A later paper by Orcel [18] gives absolute reflecting powers for a number of manganese minerals, and a comparison of these with Frick's results follows:

	Frick (orange).	Orcel (white light).
Polianite	30·8	33–42
Manganite	18·8	15–21
Hausmannite	21·0	16–19
Hollandite	25·7	28–37
Braunite	18·8	20–21

2. *Determination of Anisotropism*

The introduction of plane-polarised light and work between crossed nicols has placed in the hands of the ore-microscopist a powerful additional means of attack, even if only used qualitatively. The mere determination of isotropic or anisotropic character is of course often of direct diagnostic value; structures of aggregates can be determined, the limits of single crystals

are revealed without the need of etching; twinning in individual crystals can be detected, and the broad class of polymorphic inversions investigated.

It is in the quantitative aspect, however, that the most notable advances have recently been made. The case of plane-polarised light incident vertically on an anisotropic reflecting surface can best be treated in a manner analogous with that employed in considering transmitted light. The incident light can be resolved into two components, I_1 and I_2, parallel to the principal directions of the reflecting surface (neglecting for the moment the effect of absorption—anisotropic transparent reflecting surface), and from these arise two reflected components, R_1 and R_2. The intensities of R_1 and R_2 will depend on the refractive indices n_1 and n_2 in these directions. The reflected disturbance R will be the resultant of these components R_1 and R_2, and hence will be plane-polarised, but rotated through an angle depending on the ratio $\frac{R_1}{R_2}$ and therefore on the ratio $\frac{I_1}{I_2}$, or, ultimately, on the inclination of the vibration-direction of the incident light to the principal directions of the surface and on the refractive indices n_1 and n_2. As the stage is rotated, the amount of rotation of the plane-polarised reflected ray increases gradually from zero (vibration-direction of the incident light parallel to one of the principal directions) to a maximum depending on the relationship of n_1 to n_2. Thus between crossed nicols there will be four positions of darkness in a complete revolution, with four positions of maximum illumination between.

When the indices of absorption are taken into account, an additional complication arises, for a phase-difference is set up between the two reflected components in addition to an intensity-difference, and the reflected resultant is elliptically polarised. Actually, however, the degree of ellipticity is so slight, except when the absorption is very high, that it may usually be neglected in practical work; in extreme cases, a rotating quarter-wave plate may be placed below the analyser.

This discussion presents the basis of the two kinds of methods by which the properties of an anisotropic reflecting mineral can be quantitatively investigated—measurement of the intensities of the reflected components R_1 and R_2, or measurement of the maximum rotation of the reflected resultant. The earliest work in this field, by Koenigsberger, has been referred to briefly above. The incident light was unpolarised, and a Savart plate and analysing nicol were used to detect the presence of plane-polarised light in the reflected beam; a tilting glass plate enabled relative measurements to be made. Modifications of this apparatus have been suggested by Wright [4]

and Glaser [13], who employ a double-image prism and a rotating nicol to produce equal intensities in the adjacent fields. The Schneiderhöhn-Berek photometer described above is not so readily adapted to investigations of anisotropism, although Ramdohr [14] has outlined a method by which a comparison ocular is used to compare the intensity of each reflected component directly with that of a reflection from a standard surface of platinum.[1]

It is an outstanding advantage of Orcel's procedure with the photo-electric cell (see above) that it lends itself so admirably to the investigation of anisotropic reflection. A polariser is

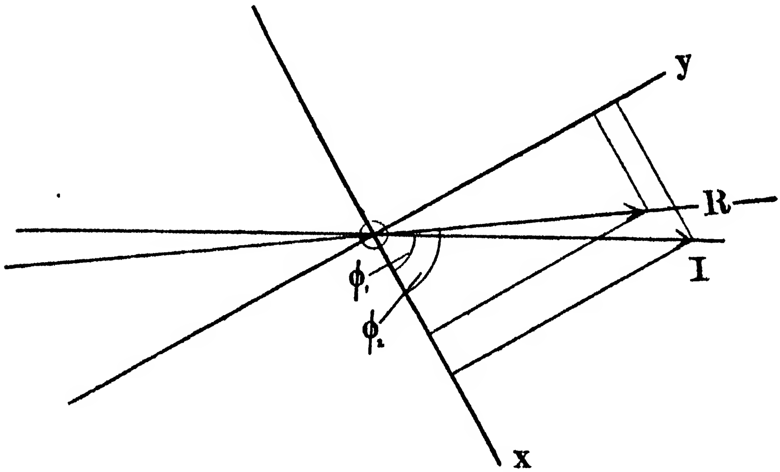

FIG. 1.

used in front of the vertical illuminator, and the maximum and minimum deflections of the galvanometer during a complete rotation of the stage are recorded, giving directly the ratio $\frac{R_2}{R_1} = \rho$. Orcel has recently shown [15] how this can be connected with ω, the angle of rotation of the plane of vibration of the reflected resultant R.

If x_1, y_1 are the amplitudes of I_1 and I_2 along Ox, Oy (Fig. 1), and x_2, y_2 are the amplitudes of R_1 and R_2 along Ox, Oy, then $\frac{\text{Tan } \phi_2}{\text{Tan } \phi_1} = \frac{y_2}{x_2} \div \frac{y_1}{x_1} = \frac{y_2}{y_1} \div \frac{x_2}{x_1} = \sqrt{\frac{R_2}{R_1}} = \sqrt{\rho}$ (the intensities

[1] It may be opportune to protest vigorously against the growing misuse of the term reflection-*pleochroism* by several writers to denote anisotropic reflection—a variation in *intensity* of the reflected light, and measurable in monochromatic light. Still worse is its use by at least one writer to describe the colour-changes between crossed nicols. Surely this awkward term should be restricted to such a phenomenon as that admirably displayed by covellite in plane-polarised white light.

of the reflected components being proportional to the squares of the amplitudes).

Hence, $\text{Tan}\,(\phi_2 - \phi_1) = \dfrac{\text{Tan}\,\phi_2 - \text{Tan}\,\phi_1}{1 + \text{Tan}\,\phi_2\,\text{Tan}\,\phi_1} = \dfrac{(\sqrt{\rho} - 1)\,\text{Tan}\,\phi_1}{1 + \sqrt{\rho}\,\text{Tan}^2\,\phi_1}$,

and for this angle of rotation to be a maximum, $\text{Tan}\,\phi_1 = \dfrac{1}{\sqrt[4]{\rho}}$,

whence $$\text{Tan}\,\omega = \frac{\sqrt{\rho} - 1}{2\sqrt[4]{\rho}}.$$

Direct measurement of the angle ω is the second of the two kinds of method mentioned above. Wright [4] suggested the use of his biquartz wedge-plate as a particularly sensitive device for detecting and measuring the rotation, though other sensitive plates or special eyepieces may be used. A prominent worker in this field has been Sampson, who published a short paper in 1923 [16], and a more complete account later [17]. Slight rotation is more readily detected if the analyser is rotated slightly from the crossed position relative to the polariser, the most sensitive position being slightly beyond the maximum rotation produced by the particular mineral under investigation. For quantitative work, Sampson stresses the importance of the Wright plate, which is uninfluenced by variations of reflecting power. The calculation given above confirms his statement that the positions of maximum illumination are not in the symmetrical 45° positions. It is evident, however, that more work is needed on the degree of rotation by actual minerals: whilst Sampson states that the greatest amount of rotation produced by any mineral is probably between 5° and 8°, a rotation as great as 5° being very uncommon, Orcel gives the error in his determinations of ω as of the same order as in the direct method, and gives a ρ-value for molybdenite corresponding to a value for ω as high as 11° 32′.

IV. Determination of Other Properties

Of the many other properties which have been investigated as of possible use for diagnostic purposes, the most important are colour, hardness, behaviour towards various reagents, and electrical conductivity.

Murdoch [2] relied largely on colour-distinctions for diagnosis, whereas Davy and Farnham [3] attempted to produce a scheme independent of colour, placing their chief reliance upon etch-reactions towards a set of standard reagents. It is now evident that grave danger lies in this complete substitution, for the etch-reactions of many minerals when associated with other species in a polished surface are complicated by

electrolytic and other effects, and may differ notably from those given by a pure isolated specimen. Etching remains of immense importance for revealing structures and for differentiating in many cases of close similarity, but its place as a major diagnostic test is being largely taken by the refined optical methods outlined above.

Colour comparisons were made by Murdoch by grinding linear boundaries and comparing the specimens in direct contact under the microscope. Schneiderhöhn [5] attempted to introduce a quantitative element by referring colours of minerals in polished surfaces to Ostwald's colour-scale, an image of the mineral being viewed directly in conjunction with the chart by means of a drawing ocular. He has determined in this way the Ostwald colour-index of a large number of minerals, but the method does not seem to have become popular with other workers. There is little doubt that Talmage [19] conforms to the views of the majority in looking for progress in direct colour-comparison effected by means of a double microscope. In routine work, comparison with determined samples will usually be sufficient; as an ultimate standard, polished surfaces of alloys of known composition have been suggested, or the insertion of wedge-shaped filters above a standard white reflecting surface.

Hardness may be of value in two essentially different ways. The testing of scratching hardness will probably never become more than a rough preliminary essay, by means of which the mineral may be placed in one of three large groups. It is ordinarily effected by means of a mounted steel needle, and each individual worker soon develops his own method of application. Attempts to introduce a rigid quantitative element into this test by means of complicated micro-sclerometers savour more of pure crystal physics, and will not receive widespread adoption. Grinding hardness, however, should be capable of more extended use, as Kalb has pointed out [20]. Wide differences of hardness are made obvious by prominent differences of relief, but smaller differences may be tested by the light-line method, and it still remains for a comprehensive list of minerals according to their relative grinding hardnesses as tested by this method to be drawn up.

Of electrical conductivity much the same is to be said. It was of value to earlier workers as a crudely simple test which separated certain pairs of minerals otherwise closely similar. Very often this can now be accomplished by measurements of reflecting power, or by means of polarised light. With attempts to apply more refined methods to determinations of conductivity in polished surfaces, absurdly large variations have been shown to exist, apparently amounting in some cases to hundreds

of thousands per cent. [Harvey, 21], and the problem passes from the field of ore-microscopy to that of pure crystal physics.

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Since this article was written, important further contributions have appeared in print. New results obtained with an improved form of the Berek visual photometer are incorporated in the forthcoming textbook by Schneiderhöhn and Ramdohr [22]. Berek has published the first of a series of articles dealing in an extended manner with optical theory [23]. Orcel has given a full account, with additional determinations, of his work with the photo-electric cell [24]. The appropriate references are appended as follows.

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F. C. P.

MODERN STUDIES IN COLOUR VISION

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Physiological Considerations [1]

It is still unquestioned that the rod-cone layer of the retina is the light-sensitive receiving surface. While the rods and cones number some twenty millions in all, the optic nerve contains less than one million distinguishable fibres. It is therefore natural to conclude that one function of the synaptic layers in the retina is the collection of the nerve pulses from several sensitive elements into a single nerve element. The fovea shows a thinning out of the inner nuclear and synaptic layers (although the outer nuclear layer is proliferated), and it is natural again to conclude that individual cones in the fovea are connected to single nerve fibres, since these anatomical views are in harmony with the known facts of visual acuity and visual threshold effects in faint light. Studies of the variation of visual acuity, which is supposed to depend directly on the spacing or "grain" of the functionally active retinal elements with brightness, are held by Hecht [1] and Houston [2] to show that the elements have thresholds which vary amongst themselves. Hence both the luminosity of the sensation and the visual acuity increase with intensity of the stimulus until a level is reached at which the acuity remains constant, *i.e.* above about twenty equivalent foot candles, when presumably all the foveal elements are in action. The visual sensation of brightness can, however, still increase.

There are some grounds for regarding the *optic* nerve as differentiated in character from the others, for the retina develops as an offshoot from the embryonic brain. Hence the results of studies based on the action of other nerves are to be applied to the optic nerve only with some caution. The so-called "all or none" principle of Adrian [3] and Lucas [4] is based on the observation that the response of a nerve to stimulation is incapable of quantitative variation. An increase of the intensity of the stimulus may increase the frequency of

[1] A general survey of the anatomical basis of colour vision will be found in Parsons's *Introduction to the Study of Colour Vision*, Camb. Univ. Press.

the response pulses, but will not alter their individual intensity. Troland [5] and others claim to have educed evidence of the same principle in the visual process. However this may be, there is no definite evidence of the existence of any such thing as the qualitative or quantitative variation of individual nerve pulses.

It follows that the vast simplification of colour theory resting on the three-component suggestion of Thomas Young [1] (or on allied theories) still retains its fundamental importance. The work of Jones [6] has confirmed Steindler's [7] results. There are about one hundred and fifty distinguishable steps of colour in the spectrum, and if any spectral colour be diluted with white, there are from sixteen to twenty-three distinguishable steps [8] until white is reached. The amazing range of colour sensations, and the equally striking fact that any hue, and most colours, can be produced by the additive intermixture of three spectral colours ; these make it clear that the facts of tri-chromatic colour mixture must hold the predominant place in the account of colour vision based on any possible theory. It is quite out of the question to assume that there are 150 different kinds of colour receptor in any small area of the retina, and few will care to support the conception of the qualitative differentiation of the nerve currents arising from any individual receptor. It still seems necessary to look for some triply or quadruply differentiated action ; either differentiated receptors or differentiated activities.

Philosophical Method

Having ventured thus far into the field of physiology, we turn for a moment to a more general consideration of scientific method. There are still those who propound general theories in words, and take refuge in general descriptions. It usually proves impossible or unduly difficult to reduce such theories to quantitative discussions, but until this has been done, no such theory can be accepted. The best test of any scientific theory is the pragmatic one. To what extent can it forecast useful results ? By far the greatest progress in the study of colour vision has been, and apparently will continue to be made on the basis of the three-component theory and those theories in direct connection with it, since it is only these, mainly speaking, which admit of quantitative tests.

The foregoing words do not minimise the very real value of

[1] *I.e.* that colour sensations are always produced by the stimulation of three fundamental colour sensations in various proportions. Young suggested that these fundamentals were red, green, and blue. See *Phil. Trans.*, 92 (1802), 12.

careful experimental work based on any theory. In the earlier, descriptive, stages of any subject, it is impossible to have too much exact information regarding the main phenomena. It was unfortunately the case that many of the earlier experimenters in visual observation were ignorant of many precautions which were essential to success. Hence recent years have seen a repetition of a great deal of earlier work under more exactly defined conditions. It may be of service to mention a few examples of comparatively recent experiments, and some of their theoretical applications.

Recent Experiments

As mentioned above, Jones and others have re-determined the so-called "hue sensibility" curve[1] in the spectrum, see

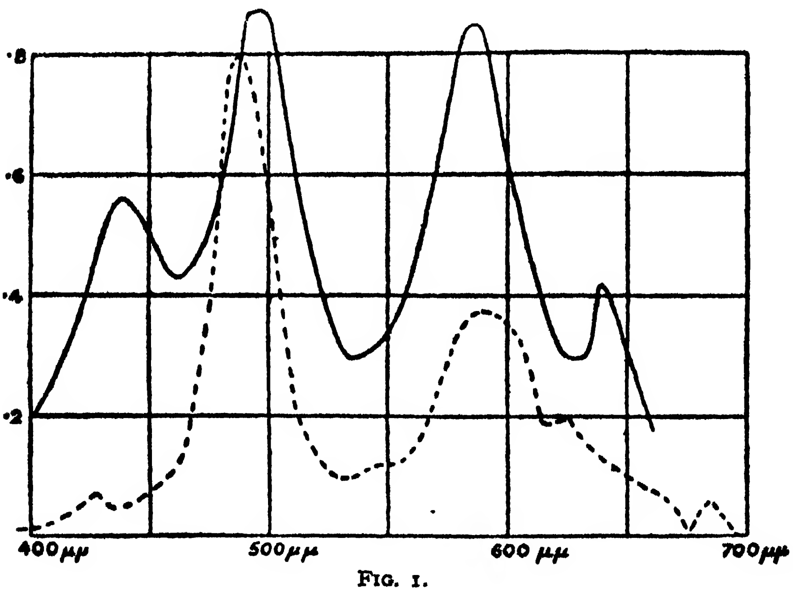


Fig. 1.

Fig. 1 (full line). The peculiar shape of the curve obviously demands explanation. Helmholtz was unable to explain it on the basis of the colour mixture curves of König and Dieterici without making transformations of the primaries which are extremely unlikely to represent any physical reality, as they would wholly distort the luminosity curve of the spectrum. Houston [9] has, however, plotted the spectrum curve (König

[1] The "hue sensibility" curve is the curve showing the relation between $d\lambda$, the smallest change of wave-length in the spectrum necessary to produce a recognisable change of colour, and the wave-length. The name is, however, not very appropriate.

and Dieterici) in the colour triangle (red and green plotted in Cartesian co-ordinates), and has then found the lengths on the curve corresponding to an interval of 10 mμ of wave-length. Plotting this against the mean wave-length, the dotted curve shown in Fig. 1 is obtained. It shows two main maxima, and if the spectrum curve were plotted to other theoretical primaries it is probable that the heights of these main maxima could be made to agree. More recent determinations of the spectrum curve, when treated in the same way, yield the two main maxima, but not the secondary ones. It is, however, very difficult to obtain a proper conception of the significance to be attached to an element of length in the colour triangle. The spectrum curve involves variations both of saturation and brightness. If it were possible to obtain a " sensibility " curve

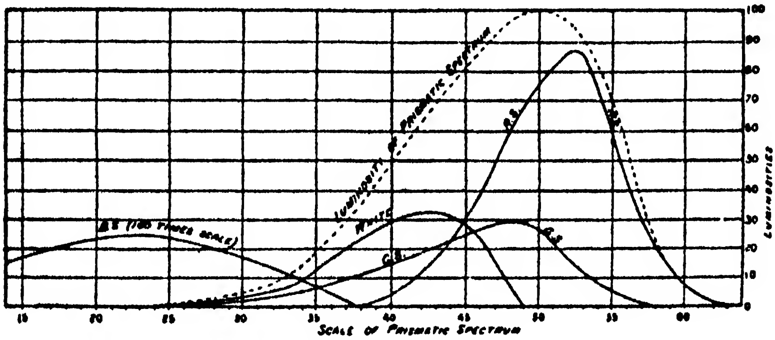


FIG. 2.

for colours at constant saturation, the results might be modified, and might be more capable of explanation on the colour triangle.

There is another standpoint from which the determination of the relative saturation of spectral colours is very important. The so-called " sensation curves " of König and of Abney [1] are given on the basis that equal ordinates represent white. Hence we can calculate the relative amount of white at any point. Abney's figure is reproduced in Fig. 2, from which a maximum of white is found at $\lambda = 0{\cdot}517\ \mu$. (The figure shows results in terms of Abney's arbitrary spectrum scale.) One experimental test for the relative saturation of spectral colours can be illustrated by a simple analogy. We have, say, seven jugs of coffee of different strengths or "saturations," and are to test the relative strengths by taste alone. We take seven glasses of water and find for each sample in turn the amount

[1] The curves representing the relative stimulation, by different parts of the spectrum, of the hypothetical primary sensations. See, again, Parsons's *Introduction to the Study of Colour Vision*.

of coffee which must be added to impart a just perceptible taste to the water. Clearly the strongest samples will require least. In another way the relative saturations of spectral colours can be found by the number of perceptible steps in dilution between the spectral colours and white.

Modern results are due to Jones and Lowry [8], and are represented in Fig. 3. The minimum saturation, or maximum relative white, is seen to occur at 0·57 μ, much farther to the red than Abney's white maximum. Hence it appears that the primaries must be modified to suit this criterion. Judd [10] and others [9] have shown how this may be done.

Studies of this kind point to the urgent necessity of a set

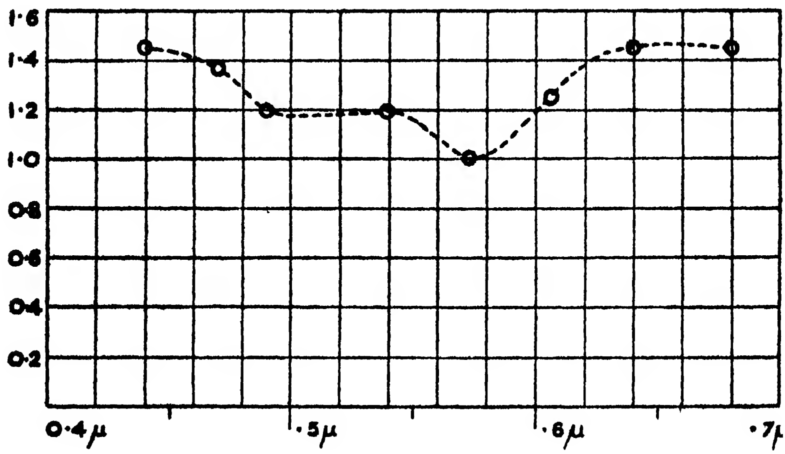


Fig. 3.

of trustworthy data on colour mixture representing the response of the normal retina. The results of König and Dieterici [1] represent determinations on one or two eyes, and the same must be said of Abney's curves. It is, moreover, well known that these earlier experimenters encountered severe technical difficulties due to stray light and other causes, which were likely to vitiate the results obtained for the ends of the spectrum where the luminous intensity is low. For these reasons a re-determination of the tri-chromatic mixture curves for the spectrum was undertaken by W. D. Wright [11] in the laboratories of the Technical Optics Department of the Royal College of Science under the direction of the present writer, and financially aided by the Physiology of Vision Committee of the Medical Research Council. A careful study of the instrumental requirements led to the production of a very efficient apparatus, in which stray

[1] Published in *Wiedemann's Annalen* as long ago as 1884, also in König's collected papers.

light was eliminated from the mixing spectral primaries by causing the dispersing prisms of the apparatus to act as purifiers, the light being reflected by mirror prisms placed in selected regions of the spectrum, and returned through the dispersing prisms to the mixture field; the second transmission taking place, however, in a part not traversed by the original light. The plan of the apparatus is shown in Fig. 4.

The results presented by Wright represent the observations of ten eyes, and these results were selected from those of upwards of forty observers; the main criterion of selection being

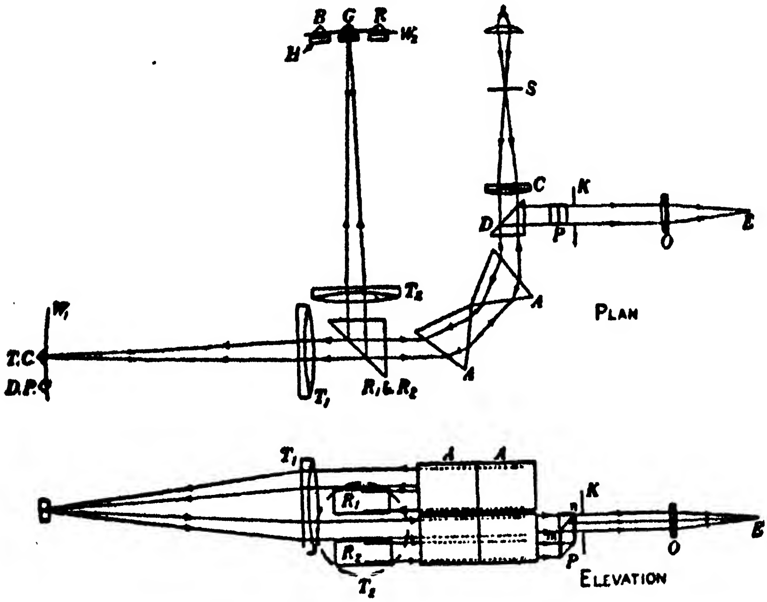


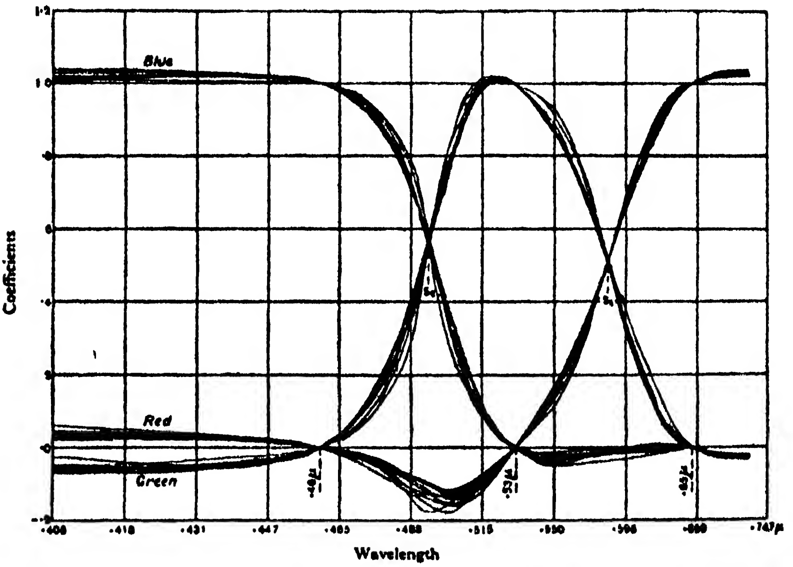
FIG. 4.—W. D. Wright's apparatus.

accuracy and consistency, in addition to "normality." Somewhat similar sets of results are expected soon to be published from another quarter in this country, and it is thus hoped to obtain a set of data which may command the greatest measure of confidence. So far, little has been done in examining the theoretical possibilities of the new data, but one important development arises from the scales which were used to assess the relative quantities of the primaries. Some of Wright's results are presented in Fig. 5, which shows the superposed curves of ten observers. The spectral primaries used were 0·65 μ, 0·53 μ, and 0·46 μ; the process of colour matching consists in finding the relative amounts or quantities of these primaries to match any spectral colour. In the first place it was assumed that equal numerical quantities of red and green

are required in the colour match for light of the wave-length $0{\cdot}5825\,\mu$, and equal quantities of green and blue in the match for $0{\cdot}494\,\mu$. When colour matches have been actually made at these points, the quantity scales are determined, and we have a measure for the primary quantity scales. Then the result of any colour match such as :

$$xC = 1303B + 344G - 99R,$$

can be transformed into a unit equation in which the sum of



Superposed coefficient curves of 10 observers

FIG. 5.—Wright's results.

the coefficients is unity, although they have the same ratios as in the above equation ; *i.e.*

$$C = {\cdot}842B + {\cdot}222G - 0{\cdot}064\,R.$$

The coefficients can now be plotted in diagrams such as Fig. 5, or in Cartesian diagrams where the coefficients of red and green are the abscissæ and ordinates respectively, as in Fig. 6. Since the primaries are spectral colours, the spectrum curve is exterior to the triangle. If it is desired, the results may be transferred to other sets of primaries, using methods developed by Ives [11a], Guild [11b], and others. The validity of such transferences is supported by a fair amount of experience, although more would be desirable. In a comparison of the colour vision of different observers, it is especially interesting

to ask them to make a match of white (standard white, N.P.L. specification, say). The white equation is found, and the white point can then be plotted in the unit triangle.

Consider an observer who is normal in all respects. He makes a match of the standard colour for 0·5825 μ in order to derive his quantity scales, and gets (say) a coefficient by which to multiply the red quantity. He thereafter makes a match of another spectral colour C and duly gives the measure of the

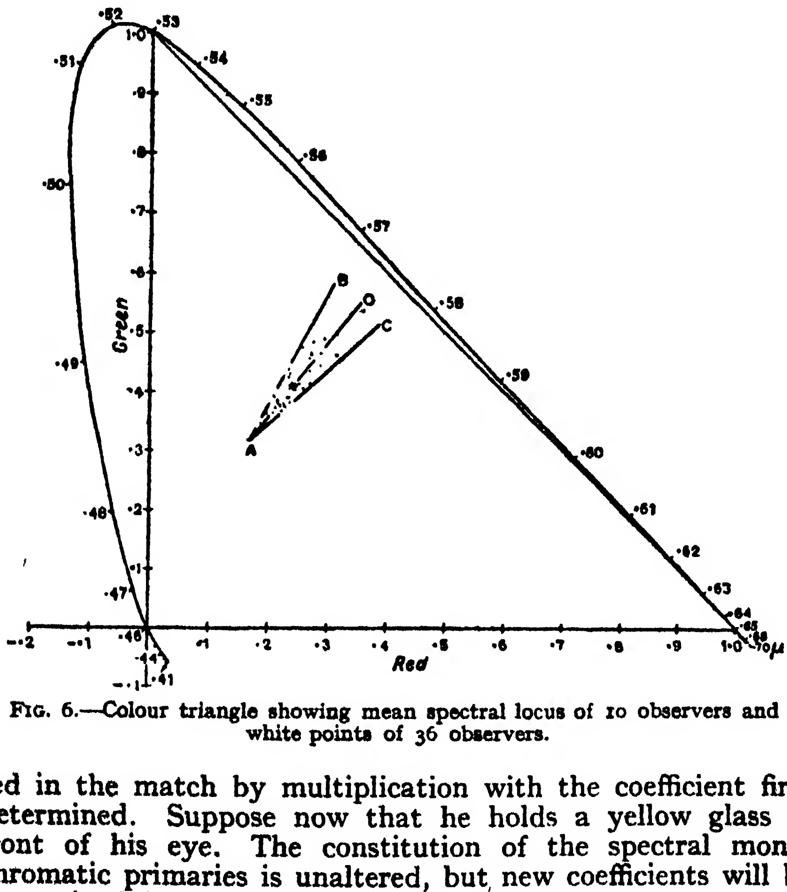


FIG. 6.—Colour triangle showing mean spectral locus of 10 observers and white points of 36 observers.

red in the match by multiplication with the coefficient first determined. Suppose now that he holds a yellow glass in front of his eye. The constitution of the spectral monochromatic primaries is unaltered, but new coefficients will be determined in the 0·5825 μ match. When the match of C is made, the corrected quantities will evidently have to be the same, and the same unit equation will result. This is not so in the case, however, of the unit equations for white found first *without* and next *with* the yellow glass, for the colour of the white will obviously be altered by the filter, and a different "white" point in the colour triangle will result.

It is in this way that the variations in the macular pig-

mentation of various observers have been shown up; the white points of thirty-six observers are shown in Fig. 6. It is worth noting that the colour of the macular pigment seems to vary, not only in saturation, but also in dominant hue. Many of the slight variations of colour vision usually described as "anomalous trichromacy" are ascribable to these variations of the macular pigment. The view of certain physiologists that the macular pigmentation is a post-mortem effect is quite incorrect. Its presence in the living eye can easily be demonstrated by

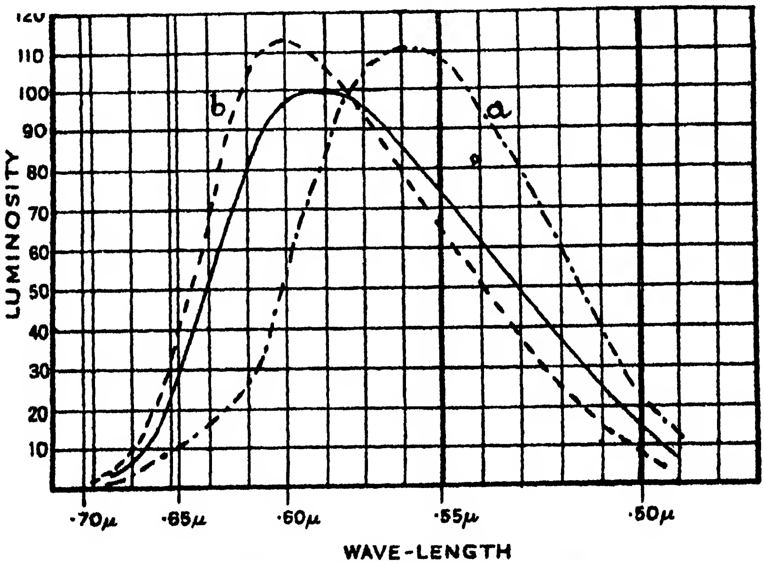


FIG. 7.—Relative luminosity in prismatic spectrum.
Full curve: normal.
Curve *a*: colour-blindness, type 1.
" *b*: " " " 2.

the peculiar appearance surrounding the point of fixation when the eye views certain coloured surfaces, the so-called "sensitive tints" of the dyer.

Colour Blindness

The position of the three-component theory with respect to "colour blindness" has hardly altered since the experimental work of Watson [12] on the spectral luminosity curves of colour-blind subjects. There is undoubtedly sound evidence of definite and well-marked alteration of the visibility curve as obtained by flicker methods. Two typical dichromat cases are shown in Fig. 7. Observer 1 would have a shortened

spectrum, and a neutral point. He would mistake red for green, and *vice versa*. He would match a mixture of green and violet light with white, and any amount of deep red light could be added to the mixture without his being aware that any change had been made, although this addition of red caused the mixture, to a normal eye, to change from bluish green to a bright red. (We quote Watson's paper here.)

Observer 2 would have a neutral point, and call the green of this part of the spectrum white. He would often call a green light white, or a white light green, and, with lights of small intensity and small angular magnitude, he would occasionally confuse green and red.

The paper detailing the above results was one of the last which Watson published before the war which cut short his life.

It should not be forgotten that König's sensation curves were largely based on his experiments with dichromatic vision, in which any spectral colour can be matched by a mixture of suitable "warm" and "cold" colours.[1] König, and V. Kries also, obtained evidence of two groups of subjects having characteristic gauging curves. If three gauging curves representing the relative stimulation of the corresponding primaries are necessary for trichromatic vision, then dichromatic vision of the two types was attributed to the removal of one or other of the primary sensations described as red or green. Hence the terms "red-blindness" and "green-blindness," to which exception has been justly taken as physiological terms.

A committee of the British Association (Leeds, 1927) reported as follows: "Any marked decrease in sensitivity to a special region of the spectrum should be specified as blindness; thus red blindness indicates decreased sensitivity to long wavelengths. . . ." "Any failure to discriminate colours should be described as confusion, prefixing the colours confused, *e.g.* yellow-green confusion. The number of distinct colours which are recognised in the spectrum is a rough measure of the degree of colour discrimination. . . ."

This committee refused to make any mention of the extremely thorough and careful work which led up to the discovery of the two main classes of colour blind; even the non-committal terms *scoterythrous* and *photerythrous*, suggested by Rivers [13] to describe these groups, are to be "abolished," and the committee prefers to base its nomenclature on the more obvious and directly ascertainable phenomena of colour blindness, giving as an example of "measurement" the "number of distinct colours" recognised in the spectrum, but refraining from mentioning how the "number" is to be

[1] A dichromat colour-blind subject can match all spectral colours by mixtures of suitable blue and red primaries.

measured. Such a determination, if carried out by the necessary apparatus with uniform comparison fields, would be of the greatest interest, but is a matter quite out of reach except by the most thorough and painstaking work. Apparatus such as the spectrometer of Dr. Edridge-Green [14], in which the eye views a spectral strip, and narrows it till it appears monochromatic, naturally gives few steps of distinguishable colour change, but the meaning of the measurement is problematical.

It is quite evident that further research work on the various phenomena of colour blindness is needed at the present day to confirm or disprove the results of earlier workers, but the nomenclature *trichromat*, *dichromat*, *monochromat*, for persons who need three, two, and one primary to match all colours in additive colour mixtures, is far too important and vital to be usurped. The terms *dichromat* and *trichromat* as used by Edridge-Green are taken by him to mean cases in which the colour-blind eye " sees two colours or three colours in the spectrum " when tested with the Edridge-Green apparatus. Such a nomenclature has only an arbitrary significance, and cannot be countenanced.

Physiologists must be assured that the use of a nomenclature (for scientific purposes) based on the phenomena of colour-mixture does *not* involve the acceptance of the Young theory, but pays only a just tribute to the most essential experimental facts. There is, no doubt, a case for a simple nomenclature for every-day practical use, if it does not conflict with one which is of such vital connection with fundamental researches.

Visual Acuity

If the normal subject has differentiated groups of retinal receptors with marked differences in their spectral sensitivity, and the colour-blind has lost the response of some group, wholly or partly, then it should be possible to detect some corresponding defect in the visual acuity of colour-blind subjects. Also, we might expect to find smaller acuity in monochromatic light than in white light for a normal subject.

Careful work by H. E. Roaf [15] on normal eyes has shown a maximum acuity in white light; monochromatic red and green are about equal, but inferior to the white, and blue is lowest. The observations relate to the same field brightness. The conclusion is reached that the differences observed must be attributed to retinal conditions. Observations of this kind are a little difficult to interpret, since the acuity determinations are often made with " test types " of the Snellen variety, and the question of the " minimum separabile " is not wholly unconnected with the resolving power of the optical system of

the observing eye. By far the greatest retinal sensibility to position difference is shown in tests of "contour acuity," where a sensitiveness of less than ten seconds of arc in the visual field is not uncommon. This implies a "local sign" discrimination corresponding to a fraction of the visual angle represented by the subtense of the separation of the centres of contiguous foveal cones. The writer hopes to be able to make some experiments on contour acuity in monochromatic illumination, when the interpretation of the results should be more free from considerations involving the action of the optical system. It appears probable that the fine-grained acuity of which the human eye is capable can hardly be explained except in connection with correspondingly fine-grained effects of induction and inhibition, which are able to accentuate very slight differences in the relative illuminations of neighbouring receptors.

Hecht [16] finds that colour-blind eyes showing red-green confusion have a maximum acuity in white light comparable with that of the normal eye; in monochromatic light the eye showing a shortening of the spectrum loses acuity in the red of the spectrum, but shows an increase in the blue just about compensating for the decrease in the red.

The fact that a dichromat may possess a normal visual acuity in white light shows, then, that he has no group of receptors entirely out of action; possibly some receptors have changed their spectral sensitiveness. Suppose for a moment that there are normally groups of red-, green-, and blue-sensitive cones; then it is possible that some of the red cones have assumed the spectral properties of the blue ones. Hecht finds this assumption difficult to reconcile with the idea that the visual brightness of a sensation is proportional to the number of cones active. On the whole, the evidence from "visual acuity" so far is extremely disappointing It may be taken as proved, for example, that there is no spectral radiation which stimulates one group of receptors only (supposing such groups to exist), but that red monochromatic light of sufficient intensity can excite all the retinal elements. Studies of visual acuity have, then, thrown little light on the trichromatic hypothesis, but do support to some extent the supposition of the presence of receptors with varying spectral responses. More work is required in this field.

Time Relations in Colour Vision

Recent years have also witnessed the more detailed examination of a number of other visual phenomena. The first is the chromatic responses elicited by suitably periodic black

and white stimuli, as in Benham's top. Piéron [17] has analysed the colours in terms of the time variation of response for hypothetical red, green, and blue primary sensations.

An allied inquiry is the examination of the periodic colour changes in the after-images produced by white and coloured stimuli, which has been the subject of special study by Prof. Peddie and his pupils [18]. Peddie's work [19] on Colour Vision drew timely attention to the magnitude of the theoretical work of Helmholtz, and gives an original discussion of some aspects of the unique colour phenomena which await theoretical explanation, and which must be vital in the final elucidation of these problems. While the mathematical treatment may appeal to physicists, the book illustrates some of the fundamental troubles which await those who seek for secure foundations among the sands of contradictory observations, and have moreover to deal with recondite conceptions, like that of "sensation," to which no external measuring-rod can be applied. This is said in a spirit by no means critical, but rather to call attention to the great necessity of well-authenticated evidence on all fundamental experimental questions.

For example, a typical beginning of a mathematical inquiry is as follows (Peddie, p. 176): "We may postulate that fatigue of a centre occurs at a rate which is proportional to the sensation value instantaneously associated with its action; and that defatigue of that centre goes on at a rate which is proportional to the sum of the sensation values which are at the same instant effective at the other two centres." It is then necessary to assume the "sensation" values to be typified by such expressions as r/ro, derived from the integration of the equation of the Fechner hypothesis of the form $ds = kdI/I$. The expression of the above conceptions takes the form

$$\frac{d}{dt}\left(\frac{r}{ro}\right) = -\rho\left(\frac{\dot{r}}{r}\right) + \lambda\left(\frac{g}{go} + \frac{b}{bo}\right),$$

with two other associated equations. If these can be taken to apply to externally unstimulated variations, we can obtain (on solution of the equations) a consistent account of the "exponential + oscillation" type of decay characteristic of the positive after-image. Miss Smith reaches the conclusion that "the phenomena of after-images, like those of direct vision itself, may be explained by known physical laws in relation to a system consisting of interdependent parts which exhibit in their action three independent freedoms only." It is only by mathematical methods that we can see a possible connection between the oscillatory phenomena of the after-image and such a "postulate" as the above, and nothing can

take the place of such theoretical discussions ; but all through work of this kind we cannot help feeling how loose is the connection between our somewhat hazy and uncertain visual observations, and the operations of the mathematical treatment with their hypothetical " sensations " and the like.

Frank Allen [20] has applied the " critical flicker frequency " to a novel and suggestive investigation of the effects of temporal and spatial induction. The older investigations of the badly named " fatigue " phenomena were practically confined to qualitative observations, and therefore tended to be inexact and contradictory. What is the real effect, for example, on the sensitiveness of the retina, of exposure to orange-yellow light, in regard to vision of the colours of the spectrum? The early investigators duly stimulated their eyes with orange-yellow light, examined the spectrum, and reported what they saw. But how true and how illuminating for workers in colour-vision is that Japanese proverb, " We see with our hearts " !

The critical flicker frequency is a more or less definite indication of the sensitiveness and state of adaptation of the retina for the flickering stimulus. The effect of initial stimulation of the retina by light from the extreme red of the spectrum (·680 μ) is found to lower the sensitiveness of the visual mechanism, so that the curve representing the critical flicker *period* (reciprocal of frequency) is elevated for a range covering the whole red of the spectrum, but ending there. Stimulation by light of $\lambda = \cdot 589\ \mu$ (orange-yellow) produces two elevations, one in the red and another in the green, but leaving the orange-yellow (curiously enough) unchanged. Stimulation with $\lambda = \cdot 570\ \mu$ leaves the whole curve practically unchanged, and the same is true of $\lambda = \cdot 470\ \mu$. These " transition colours " are most curious, since on this basis they do not seem to lower the sensitiveness of the retina. There are two others, viz. ·660 μ and ·420 μ. Allen puts forward the view that the results strongly support the three-components theory, and traces connections between transition colours and the intersection points of the sensation curves of König and of Abney.

The effects of simultaneous stimulation of one retinal area upon another part, and the effect of the stimulation of one eye on the sensitiveness of the other, can also be tested by a similar method. In these cases both depression and *enhancement* of sensibility for different spectral regions may be found. Thus where the left eye is " fatigued " with red, $\lambda = \cdot 687\ \mu$, and the right eye is examined, the critical-period curve shows depressions (enhancement of sensitiveness) at all points except ·48 μ and ·67 μ. When the left eye is light-adapted, and the right eye " fatigued " with red (·687 μ) and examined, the

curve shows depressions on each side of 0·48 μ, but an *elevation* beyond ·67 μ. Allen has suggested that " every ray of light " produces two general effects on the retina—a " direct " and a " reflex." The direct action fatigues either one or two of the primary sensations red, green, and violet, according as the colour stimulus is simple or compound in its nature ; the reflex effect causes the excitation of all three sensations in such a manner as to enhance their luminous response. He also suggests that this reflex action is responsible for the production of the white light underlying all sensation of colour. In the case of the equilibrium colours, the direct and reflex effects balance or neutralise, so that even after prolonged exposure of the eye to their influence, all other colours of the spectrum are seen without diminution or enhancement of brightness.

Not everyone will fully admit the interpretation which Allen has placed on his results, but their great interest and vital significance in the study of colour vision cannot be denied. There are certainly close relations between the inductive effects, in the enhancement and depression of sensitivity, and the reflex actions of the nervous system studied in detail by Sherrington.

Parsons observes that there is a vast field of research opened in the study of the persistency curves for colour-blind subjects, and it cannot be doubted that in the phenomena of the transition colours we have another of those unique phenomena, like the " hue sensibility " maxima, which challenge our ingenuity to explain them. It seems at present as though they do lend support to the three-component theory.

Further Physiological Questions

Physiological studies on the " all or none principle " were mentioned at the beginning of this paper. It was hoped by the electrical studies of the nerve currents caused by the stimulation of the retina by light (Chaffee and Hampson [21]) to elicit some information as to a differential type of response for spectral lights of different colours. It is remarkable that the forms of the electrical pulse for all colours show no noteworthy difference. The investigators say that " it is probable that to obtain an insight into the true nature of colour sensation, some method must be devised for further analysing the response curves into the finer pulses arising from a single rod or cone." So far as it goes, then, this seems to imply the presence of retinal receptors of different hue sensitivity and different response, of which the individual effects have always been masked in the methods so far used.

Are we to take the " duplicity theory " of the functions of

the rods and cones as definitely settled? In his latest book Parsons [22] discusses the general characteristics of perception on a basis which contemplates a dual mechanism; the older, primitive, or "dyscritic" mechanism, out of which or upon which an "epicrite" mechanism has appeared. He examines the conditions of scotopic and photopic vision, finding in them much to support the thesis that these modes of vision represent the dyscritic and epicritic stages. Is the phenomenon of colour, essentially a feature of the epicritic stage, mediated by the cones, while rods are responsible only for the achromatic vision characteristic of twilight?

Perhaps it is as well to keep the gate open, and allow that the rods may have some chromatic functions in photopic vision. Kundt has shown that the absorption maximum of visual purple dispersed in different media (as in the rods and cones) may vary somewhat, and Hecht [16] has examined the possibility of building up a set of trichromatic mixture curves which shall be *deliberately* made to explain most of the main "unique" phenomena set forth above. The maxima of these curves *lie surprisingly close together.* Considerable success is possible in the discussion of most points except the profound alteration in the relative spectral luminosity curves of colour-blind subjects.

The great necessity of present research is more exact information. Experimental methods have been improved, and a great deal more common ground exists for co-operation between physiologists and physicists. The foregoing pages have dealt with the discussion of the subject mainly with reference to the use of the Young theory. This is at present the most pointed chisel which we can take to penetrate the armour of this intractable problem, but nothing need hinder our recognition of the existence of indications that the trichromatism of our sensations may be only a phase of the question. The significance of the persistence of yellow in colour fields, and the continuance of yellow-blue differentiation for colour-blind eyes showing red-green confusion, known from descriptions given in acquired colour-blindness [23] (and in cases where one eye is colour-blind, while the other is normal) cannot be minimised, but we have not yet fully learned how to take a theory like the "Ladd-Franklin theory" and use it in a quantitative manner. The development of chemistry and physics may one day put better tools into our hands.

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POPULAR SCIENCE

THE ROMANCE OF SCIENCE IN BYGONE LONDON (*Concluded*)

By H. G. WAYLING, M.Sc.

A RAPID transition from city to country is possible by leaving Parliament Square for St. James's Park, traversing Great George Street meanwhile. The very names before us suggest a rural atmosphere: Birdcage Walk, Spring Gardens, The Mall; with a splendid stretch of water in sight alive with swimming birds. Cromwell Mortimer (died 1752), a Secretary of the Royal Society in the first half of the eighteenth century, mentions deer and cows grazing on its pastures, and he himself fetching milk from the Vineyard Gardens; not for human consumption, however. As a physician acting temporarily in the capacity of a veterinary surgeon he was investigating the recent outbreak of foot and mouth disease, then called the cattle plague, and was engaged in testing the milk and flesh of the affected animals. Although uncertain as to the cause of the scourge, he seemed to think that bleeding the beasts and giving them whale oil, treacle, and crocus metallorum would bring about some improvement in the milder cases. The faith in the curative properties of the last-named remedy was everywhere so strong that the contemporary German alchemist, Hoffmann, painted on his carriage, "No one knows all the virtues of Antimony."

To the west of St. James's Park lies the Green Park, whose surface, unbroken by any breadth of water, was considered eminently suitable for those exhibitions of fireworks that were given in view of Buckingham Palace when it was deemed appropriate to celebrate the visit of foreign potentates to London, or the signing of Treaties with Continental Powers. On the occasion of the consummation of the General Peace of Aix-la-Chapelle in November 1748, a magnificent display of pyrotechnics was turned to some account by Benjamin Robins (1707–51), who was interested in finding the height to which the rockets would rise, and applying such information to naval and military manœuvres. Robins, who was employed at

Woolwich laboratory, is still remembered for his researches in gunnery and ballistics. Having commissioned a friend who lived in King's Street, Cheapside, to observe with a carefully graduated quadrant the greatest angle of elevation of the scintillating crest of the last great flight of some six thousand rockets terminating the display, he was informed that the maximum angle was 8¾ degrees above the horizon, representing an altitude of 615 yards. The most powerful rockets would thus permit co-operating armies to communicate fifty miles apart in level country. As such an expenditure of combustible material might lead to unintentional conflagrations in the immediate neighbourhood, it was necessary to safeguard the superstructure supporting the blazing artillery. One suggestion as a protection against such an eventuality was that made by the Reverend Stephen Hales (1677–1761), who was not only solicitous for the spiritual good of his flock at Teddington, but mindful of the bodily welfare of mankind at large. Among other things, he was concerned with the ventilation of prisons and the aeration of drinking water supplied to seamen. His plan for preventing these incidental outbreaks of fire was to cover the floors of contiguous buildings with a layer of soil; a remedy which he found successful in practice. Two days after the pyrotechnic demonstration had been given in the rejoicings of 1748, Dr. Cromwell Mortimer went over the skeleton fabric, and was greatly pleased to see that the engineers had resorted to the agency recommended by Dr. Hales, besides having coated the wooden walls with a kind of whitewash.

Overlooking both parks stands Buckingham Palace. Before it attained its present proportions, it was nevertheless considered large enough to be provided with lightning conductors, as other commodious buildings were, towards the close of the eighteenth century. The pointed tip advocated by Franklin, Cavendish, and John Canton was not accepted without dissension from other natural philosophers, who recommended that the top of the lightning rod should terminate in a knob. There appeared to be a justification for this preference, since some premises fitted with points had been struck during thunderstorms. Hence, when it was proposed to equip Buckingham Palace with these safeguards, George III took an interest in the type of rod to be supplied. He seemed to favour the rounded form so volubly appraised by the artist Benjamin Wilson. In order, however, to obtain a final opinion on the perplexing matter, he sent for the President of the Royal Society, Sir John Pringle. When the King found that the Scotsman recommended points, he was rather displeased, nor was his annoyance allayed when Pringle expressed his regret at being unable to alter the laws of nature to oblige

His Majesty. The attendance of Scottish scientists at Buckingham Palace on other occasions has not always been accompanied with the happiest of memories to the Royal household. The question of the genuineness of the Koh-i-nor exercised the mind of Queen Victoria when doubts from certain quarters were thrown on its intrinsic value. In order that a determination of its specific properties might be arrived at, the leading authority on optics, Sir David Brewster (1781–1868), was summoned to the Palace by the Prince Consort, to examine the diamond. The Scottish physicist had recently invented an instrument called a lithoscope, which, by means of polarised light, was able to distinguish glass from gem. Many ladies in the land had submitted their jewels to the test, only to be regretfully informed that their treasures were coloured paste.

The result of the test applied to the " Mountain of Light " was not reassuring, since from its size, shape, and optical properties, Sir David told Their Royal Highnesses that they were not in possession of the gem originally owned by the Great Mogul. A few years previous to this incident, a Scottish chemist, Lyon Playfair, was asked to examine the sanitary arrangements belonging to Buckingham Palace. The investigation proved to be so unsatisfactory that the authorities dared not publish his report. A main sewer ran through the courtyard, and the whole house was in an untrapped connection with it. To prove this, Playfair painted a small room on the basement floor with white lead, and showed that it blackened next morning. The kitchens had defective flues, and unhealthy fumes went up to the Royal nurseries. By exploding pastilles containing gunpowder in the kitchen, Playfair brought down, as he anticipated, the High Court officials to discover the cause of the smell pervading all the rooms. His ruthless inquiry was soon succeeded by the necessary architectural alterations.

From the Palace to Hyde Park is only a matter of a few minutes' walk, and one attended with a feeling that the tourist is in the City but not of it. One of the chief attractions of Hyde Park is the fine expanse of water, the Serpentine, so much appreciated of late for its bathing and boating facilities. Not always, however, has it been a place for aquatic recreation. Soon after Lyon Playfair had improved the sanitation at Buckingham Palace he had to report on the insalubrity of the Serpentine, fouled at that time by Bayswater sewers. The latter nuisance was stopped, and the vile mud in the stream removed. A century earlier, John Smeaton (1724–92), of Eddystone Lighthouse fame, experimented on the Serpentine with a ship's log, which was a simpler form of machine than one recently invented by de Saumarez and tested on the Thames. Smeaton states that in May 1751 he procured a boat

on the Serpentine river in Hyde Park to try how the turns of the instrument would be "consistent with themselves." Over a measured course, which on account of contrary conditions took longer to cover in one direction than in the other, he found in his type of machine that the variation in the speed of the boat made little material difference to the total number of revolutions recorded by the spindle. Hyde Park has always been plentifully endowed with streams and springs, and, before the modern methods of water-supply, with conduits. A remnant of one of the latter is near the west side of Hyde Park Corner. From such a source, John Woodward, Professor of Physic at Gresham College, carried out experiments on the growth of vegetation with phials, in some cases containing nothing but water from a Hyde Park conduit. Being very dubious of the teaching of Van Helmont and his school, that water could be transmuted into earth and other natural things, he decided to put the doubtful question to the test. An investigation of the Belgian alchemist, in which a willow tree had been grown for several months in a weighed quantity of soil and fed only with water, had resulted in nearly as many pounds increase in the weight of the tree as the soil itself had lost in ounces. This seemed to substantiate the foreigner's assertion of the transmutation of water into solid matter. With the idea of probing such a doctrine, Woodward placed plants of spearmint in phials containing samples of conduit water and allowed the plants to grow for fifty-six days. Since all natural waters hold little or much of earthy stuff in solution, some of this would be available for plant food. Although the Gresham professor covered the glass vessels with parchment in such a way as to prevent direct evaporation from the surface of the water, it was necessary, during the course of the experiment, to use copious quantities of fresh liquid to maintain a definite level around the lower parts of the plants. In no case was less water supplied than fifty times as much as the final increase in the weight of the mint, and in some instances as much as seven hundred times the ultimate increment was used. Thus the plants had ample opportunities of extracting sufficient earthy material from the water to account for the gain in mass. Woodward was convinced that the greater part of the water absorbed by the mint found its way through the leaves and into the air, an observation which credits him with being the discoverer of plant transpiration. The professor's quarrel with the celebrated Dr. Mead has been often related ; but kindlier thoughts of him will be recalled as the donor of a valuable collection of fossils to the University of Cambridge and an endowment of a lectureship in Geology.

Before leaving this most fashionable of London's open

spaces, we may as well point out the existence of a smaller piece of water near Kensington Palace, known as the Round Pond. This prosaic basin has been made historic by experiments performed on its surface by no less a person than Michael Faraday. In January 1832 he was still occupied with researches on the induction of electric currents by magnetic forces. He had succeeded in obtaining current electricity by spinning a suspended magnet around its vertical axis. Then he turned his thoughts to that biggest of all natural magnets, the Earth, and asked himself if the rotating planet does not generate induced currents as it turns on its axis from west to east. Accordingly he went to the Round Pond in Hyde Park, stretched a piece of copper wire 480 feet in length north and south over the lake, causing plates soldered to the ends of the wire to dip into the water. The copper wire was severed at the middle, and the free ends connected with a galvanometer. As no change was observed in the testing coil, he tried the effect of running water during the ebb and flow under Waterloo Bridge ; but without any satisfactory results. Still he urged, " Theoretically, it seems a necessary consequence that where water is flowing, there electric currents should be generated." Maybe Faraday afterwards put this failure on his list of " unsucceeding experiments."

If we look at an old map of London, we shall see that a fairly straight road connects St. James's Park with Chelsea, the greater part of the thoroughfare being called King's Road. Its name is significant. In bygone years it was kept in repair at the King's expense, as it formed part of the journey from Whitehall to Hampton Court Palace. Chelsea was then a village of some natural beauty. The fine reach of river on its southern side, its view of glorious sunsets which in later years inspired the artist Turner, and its quiet rural atmosphere provided a change to gentle-folk oppressed with the cares of the City and Court. Then there was the walled Apothecaries' Garden, like a miniature Eden, as well as the Royal Hospital, many of whose officers were well known in society. Prince Rupert (1619–82), who had private rooms at Whitehall, also had an experimental glass-house, on a site now covered by the Royal Hospital, where he manufactured his " Drops," which from that day to this have been a source of curiosity to novices in science. However, the excessive amount of coal required to keep his fires ablaze and the resulting clouds of smoke and grit became a public nuisance, and prevented the Royal Society from letting the ground on which a Royal College then stood. Then there was Robert Boyle, who lived with his sister Lady Ranelagh, in Pall Mall. He, too, had a laboratory in Little Chelsea, to which distinguished foreigners came to watch him demonstrate

his pneumatic engine. Hans Sloane, whose name is perpetuated in a well-known square, lived in Chelsea manor-house, and his statue in the Apothecaries' Garden is intended to testify to his liberal benefactions in the cause of botanical science.

Another name associated with the Physic garden, and remembered by students of Physics, is that of Dr. Ingenhousz. The Dutch physician had lodgings in Pall Mall Court, and as we have before suggested, would be a likely visitor to the village of Chelsea. In 1782 he conducted experiments in the Botanical Gardens, proving that plants grown in inverted glass globes completely filled with water, produced oxygen, or as it was called dephlogisticated air. Sir Joseph Banks spent his boyhood in the riverside hamlet, and when he later resided in Soho Square he frequently had quarters here to ruminate at ease. A touch of romance accompanies the next name, which is that of James Edward Smith, the founder of the Linnæan Society. Coming from Edinburgh to London as a young medical practitioner, he carried letters of introduction to Sir Joseph Banks, then President of the Royal Society, at a time when the distinguished knight had had the option of purchasing the extensive collection of botanical and geological specimens amassed by the Swedish naturalist Linnæus. Banks turned the offer over to young Smith, who obtained the necessary funds from his father, a prosperous Norwich merchant. James Smith took rooms in Paradise Row, now called Royal Hospital Road, which borders, and in some parts overlooks, the Apothecaries' Garden. When the bulky cargo of specimens arrived in England, Banks and Drysander helped the fortunate owner to rearrange and tend the collection. During 1785 Smith investigated the problem of the irritability of plants in the Physic gardens, using a barberry bush for experimentation. Benjamin Franklin knew Chelsea well. He lodged for some time at the house of the Reverend David Williams in Lawrence Street. Williams is remembered by impecunious authors as the founder of the Royal Literary fund. Sir Joseph Banks used to attend his chapel in Margaret Street, Langham Place; albeit in rather a surreptitious way. The future American Ambassador, on one occasion swam from Chelsea to Blackfriars, a feat that so impressed some of his friends that they recommended him to tour the Continent and gain a livelihood by giving exhibitions in the art of natation.

Before leaving this romantic spot we must recount a curious experiment that was communicated to the *Philosophical Transactions* by John Wilkinson, M.D., F.R.S., for determining the exact weight of human and other bodies in fluids. After having estimated practically how much lead various cubes of cork could sustain in fresh water, he

turned his attention to discover the approximate weight of cork that would be required just to support a man placed in river water. For this purpose he procured an average-sized person who could swim (in the event of anything untoward happening), stripped him naked, and tied 10 ounces of cork in the form of a collar about his neck. The place where the investigation took place was from the river-bank in Chelsea Reach, at a spot where the water was about a foot deeper than was required for total immersion. This weight of cork not being enough to keep the man afloat without his own unaided efforts, another ounce of cork was added to the original form of lifebuoy. This still being insufficient, a further half-ounce was found to give practically a perfect adjustment. According to a previous experiment, this 11½ ounces of cork would keep afloat 63½ grains of lead in a similar medium. Wilkinson was cautious enough to observe, that the same quantity of cork which supports a fat or very plump person in water, will not suffice to buoy up a lean individual, though their weights out of water be equal.

Now that we have reached the river's edge we may as well take our promised trip afloat, and record some of the scientific researches that have occurred on and near the noble stream with which London is inseparably connected. Between Chelsea and Westminster the land on either side was swampy, and only partly cultivated for many centuries. The name of Lambeth Marsh still persists ; high tides inundated the land on which the Tate Gallery now stands. Coming in sight of Westminster Bridge, we must make reference to the structure which previously spanned the river here, and on which Sir William Watson performed his experiments on the conduction of electricity. Following on the researches of Stephen Gray (1670–1736), the Charterhouse pensioner, Watson, and some other prominent Fellows of the Royal Society, were curious to discover the speed of the electric fluid. Gray, in the confines of a country house, had shown that it was able to pass several yards along a thread connecting a rubbed glass tube and an ivory ball. Sir William Watson, who was part of his time physician to Westminster Hospital, might have considered the local bridge an appropriate support for a longer conducting wire.

Accordingly, on July 14, 1747, several members met there to assist the physician to perform the experiment. Among his helpers was Dr. Bevis, who brought lightning from the clouds with an electric kite which he flew from St. John's Gate, Clerkenwell, where Dr. Samuel Johnson worked for Cave, the printer. However, to return to our original account. A line of wire was laid along the bridge, not only through its whole

length but likewise turning at the abutments and reaching down the stone steps on each side of the river, low enough for a helper to dip into the water an iron rod held in his hand. Thus it was possible to construct a circuit, part on the bridge and part through the river, with a charged Leyden jar held by a person (also in the electric chain) posted on the Surrey shore. When the jar was discharged, the circuit was closed, but the shock was experienced simultaneously by both observers. Before concluding that the speed of the electric fluid was too great to be estimated, Watson repeated his trials in the neighbourhood of the New River, Islington, and finally with miles of wire near Shooter's Hill, in Kent. The sight of several men acting as connecting screws in a gigantic electric circuit and all jumping simultaneously when the Leyden jar was discharged, would be worth going a long way to see. On the east side of Old Westminster Bridge was Manchester Stairs, one of the many landing-places for ferries and rowing-boats. In midstream hereabouts, a new form of ship's log, called a Marine Surveyor, was tested by Henry de Saumarez in 1780. By means of a rotating Y-shaped piece of apparatus which recorded its revolutions on a dial in the ship, the inventor, at a time of full tide, drew up a table showing the alteration of the speed of the tide and the changes in the depth of the river. He says that, "Were the same thing done in the Channel and on the sea coast, it would be of no small use to commerce." The variation in the depth of the stream was recorded every fifteen minutes. During an ebb-tide, lasting nearly 8¾ hours, the water was flowing at its maximum speed 1¾ hours from the turn of the tide, and that at a rate of approximately 1⅜ miles per hour. During a neap-tide, which lasted 7½ hours, the greatest velocity occurred 1¼ hours after the tide changed. In the opening years of the nineteenth century, it was discovered by William Hyde Wollaston that a mirage could be seen on the Thames; especially during the hottest days of summer. Wollaston was in practice as a doctor in Cecil Street, since built over, but just west of Savoy Hill. Having more leisure than was lucrative from a professional point of view, he improved the shining hour by combining research with recreation. One very hot day, he placed his eye a foot or so above the surface of the water, and noticed that the oars of some bargees were so distorted as to appear like giant hockey sticks with the convex bend away from the sides of the boat. On returning home, he reproduced similar effects of atmospheric refraction. Making a poker red-hot and viewing through the stratum of warm air that rose from it some lines he had made on the wall, he found the marks raised appreciably above their original position.

We now come into line with King's College, which

forms the eastern extension of Somerset House. When George III's museum was opened in this building in 1843, an interesting experiment was performed by Professor Wheatstone, who used one of his electric telegraphs to make communication between that institution and a lofty shot tower on the opposite bank of the river. This was done by laying a wire along the parapets of Somerset House and Waterloo Bridge and thence to the top of the tower, where one of his instruments was placed. The wire then descended, and a plate of zinc soldered to its extremity was plunged into the mud of the stream, while a similar plate on the other side of the river was employed to conduct the electricity from the intervening Thames. Thus history repeated itself: Wheatstone was adding refinement to the cruder experiment done a century earlier by Watson. Just east of Old Somerset House was the town residence of the Howards, Earls of Arundel, and the Dukes of Norfolk. Arundel Street and Norfolk Street give evidence of the former demesne. For a time succeeding the Great Fire, the Royal Society held its meetings at Arundel House, whose gardens extended around it and down to the river shore. In those days of primitive invention, the construction of a speaking trumpet was considered a great improvement on the powers of broadcasting possessed by the human voice. The originator of this instrument is believed to be Sir Samuel Moreland (1625–95), who demonstrated with it in 1670. A modification of his pattern was exhibited before the Royal Society from the terrace of Arundel House in the last quarter of the seventeenth century. The object itself was something like a brass euphonium, provided with a lateral mouthpiece. Instead of the sound from the voice taking a direct path from mouthpiece to the exit, it traversed an inner tube, thus impinging on a concave surface at one end of the interior, and thence along an outer jacket into the open air. On its public trial, this instrument successfully delivered words from one side of the river to the other, and that against the wind which was then strong. The message was written down by a person sent over to the opposite shore for that purpose. The advantages claimed for this trumpet were that it could send words farther than the Moreland pattern, besides being able to shield the original sounds from disturbing noises from the land. The few steam-boats that ply with the laudable object of giving sightseers a view of the City from the river, have a cleaner stretch of water on which to travel than was the case in the middle of the last century. The river then was little better than an open sewer. At low tide the water was an opaque brown fluid. Faraday purposely took a trip from Blackfriars to Hungerford Bridge one day in July 1855 to draw public attention to its vile condition. With his usual resource-

fulness at demonstration, he tore up pieces of white cardboard, and then moistening them so that they would easily sink, dropped some of them at every landing-place that his boat touched. Before the fragments had sunk an inch below the surface, they were indistinguishable, though the sun shone brightly at the time. When the pieces fell edgeways, the lower part was hidden from sight before the upper part was under water. Near the bridges the filth rolled up in dense swirls. Finally, in addressing these remarks to the Editor of *The Times*, he said, " I was glad to enter the streets for an atmosphere sweeter than that on the river." Faraday had disinfected the Millbank Penitentiary, following a fatal outbreak of fever, with chlorine, but the Thames was an Augean stable of too vast a scale to be dealt with single-handed. In most of our accounts we have tried to associate scientific labours with places still in existence. The habitations of men change from year to year, yet in our itinerary we have dealt with sterner stuff than dreams are made of.

Before we disembark from our fairy boat, we must pay a call farther downstream at Deptford, once known as West Greenwich. It is common knowledge that from the time of the Stuarts, down to comparatively recent years, a Royal Dockyard has existed here. Peter the Great came to its yards to learn shipbuilding ; Pepys and Evelyn visited its busy quays. In 1681, Fellows of the Royal Society were experimenting with a diving bell, in which their curator remained half an hour under water. The submerged object was made of lead and let down with a strong cable. The construction of the bell was the practical corollary of Boyle's researches with the air pump, and the tube in which he discovered the law so closely connected with his name. At a depth of 33 feet the density of the enclosed air in the diving bell would be doubled. The use of this engine, as it was then sometimes called, was further explored by Halley. Being Astronomer Royal at Greenwich, he was conveniently placed for experimenting in Deptford Creek. He suspended his bell from the mast of a ship by means of a sprit, which was directed by braces to carry it clear of the vessel. His diving bell had a glass window at the top, and attached to the leaden truncated cone, the shape given to the bell, were two empty barrels with their bung-holes downwards. By fastening a piece of hose-pipe to the top of a barrel, and having sufficient length of it, he could leave its lower end at such a depth under the bung as to allow the imprisoned air to escape into the bell at will. With the assistance of two men on a platform the tubs could be hauled up alternately, and a fresh supply of air delivered into the enclosed compartment. In later experiments, Halley made himself a cap of lead so that he might walk

on the bed of the river, his headgear being a miniature diving bell itself. We have now come to the end of our journey. Although London grows and changes greatly each decade, there still remain associations with the past that can tell us much of the romance of science in bygone days.

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NOTES

The Physicist to the Rescue (P. E.)

The *Sixty-ninth Bulletin of the National Research Council of the U.S.A.* contains twelve papers concerning the present relations of molecular physics to biology. In an introduction, which is the most interesting (though not therefore the most useful) part of the publication, Lecomte du Noüy develops the thesis that since the colloidal condition is responsible for so many of the properties distinctive of living things, it is to the physicist, not the chemist, that we must look for help in understanding the tricks of protoplasm. From all branches of biological work he gathers examples of problems the solution of which is likely to require purely physical methods ; and on the other hand, makes reference to certain recently evolved physical techniques so far not applied to biological work, but clearly possessing great potential value. After this introduction, the interest of which is very inadequately conveyed by the above summary, one is just a little disappointed by the papers which follow. The oxidation-reduction potential of protoplasm is dismissed in two pages, and work on membrane equilibria in three. The two subjects together occupy just half the space allotted to a description of the Meiostagmin Reaction, which is a clinical method for diagnosing malignant tumours.

Nevertheless the *Bulletin* is sure to interest a wide range of readers, if only on account of the valuable contributions of Leonor Michaelis concerning " molecular sieves "—membranes of such small and constant pore diameter as to be capable of sifting glycerol away from acetone—and very thorough treatment of surface tension and allied phenomena by N. E. Dorsey. In the first of these two articles Michaelis ventures the attractive speculation that the now familiar glass electrode depends for its action on the circumstance that certain kinds of glass may act as sieves, permeable only to the smallest cation of all—the hydrogen ion.

Prof. Osterhout has contributed an article on " Permeability and Bioelectric Phenomena," summarising the work of his school on the conductivity and certain other electrical

properties of living cells exposed to a variety of conditions. Prof. Osterhout's studies have been confined in the main to the cells of certain simple water plants, but the information obtained as to the "permeability" of cell membranes living, dying, and dead has doubtless a very general value. As a complement to this contribution there is a summary by W. Seifriz of our knowledge of the viscosity of different types of protoplasm, and an account of the ingenious methods that have been employed for measuring this property in living cells. Dr. Seifriz' own work in this field is well known.

Surface Protection by means of Schoop's New Homogeneous Pistol (Dr. Alfred Salmony, Berlin)

Exactly twenty years have passed since the Swiss inventor, Dr. M. U. Schoop, of Zurich, carried into effect the basic idea of his ingenious invention of the metal-spraying process. At that time probably nobody would have thought of the manifold possibilities of application of this new process.

The essential feature of the metal-spraying method is characterised by the fact that finely spread metal in a fused state is thrown with great vigour, by means of gas pressure, against the parts to be metallised where the metal particles tend to fill up the unevennesses and pores of the surface. The soldered-on metallic layer so produced ensures a durable and safe closing of the surface. By untiring energy Schoop has succeeded in raising the technics of metal spraying to its present standard of utility. Two years ago he succeeded in finding a method which enabled him to use lead, the uniform coating of which had caused much difficulty. For this purpose he carried out the spraying operation in three phases, namely, fusing, atomising, and spraying. In these operations it is essential that the metal should not come into contact with atmospheric oxygen or directly with the flames. For this reason he heats the metal indirectly with high-temperature neutral gases, carbon dioxide being most suitable.

The apparatus used is known as the homogeneous spraying pistol. Fine metal dust (*e.g.* of zinc, tin, nickel, tungsten steel or silver) is forced by compressed carbon dioxide through openings, allowing only the passage of the finest particles, in a chamber heated by an oxyacetylene flame. The gas pressure is about 7 or 8 atmospheres, and the particles attain a speed of about 8 m/sec.

It is a characteristic feature of this improved spraying process that substances of any nature can be treated by it, that is to say, not only metals but also combustible, inflammable, or even explosive substances such as cement, concrete, gypsum,

wood, paper, textiles, and celluloid. It is possible to prepare extremely thin metal coatings which adhere uniformly. Schoop has, for instance, coated paper with aluminium of a thickness of 0·002 mm., and he is of opinion that this paper could be very well used for banknotes. Alloys of bronze or white metal can also be applied in a very fine coating. Such coatings should prove important, for instance for the cable and telephone industry, as well as for loud-speaker membranes. This extremely thin metal paper conducts the electric current in the diagonal from one corner to the other, which is a proof of its metallic continuity.

Quite new is his process for the manufacture of "metallowood," a new raw material which unites the specific advantages of wood to those of iron. The metallowood is manufactured in two different finishes, namely, with ground and polished metal coatings and with a coating showing the characteristic grain of the wood. This wood may be bored, sawed, patinated, etc. The packing of the joints also does not present any difficulty, as it does with wood coated with metal by other methods. A wood prepared in this way is resistant and waterproof. It forms a good insulator against heat and cold, as well as against sound.

The finest textile fabrics and even laces have been sprayed, and one has obtained in this way a uniform metal coating or, by means of patterns, very beautiful metallic effects. As mentioned above, the metal particles tend to produce a uniform surface, and a coating with metal of concrete and cement pipes and similar material can be easily effected internally as well as externally by the homogeneous spraying pistol. A short time ago the *Schweizer Wasserwirtschaftsverbandes* of Zurich carried out testing operations in order to verify the uniformity of the deposit on concrete plates metallised in accordance with the Schoop process. The water permeability of these plates was tested by high-pressure apparatus ; no oozing at all could be detected. Roofs of concrete are frequently leaded, for which operation fifteen minutes are required per square metre. The handling of the homogeneous spraying pistol is so simple that a fairly skilled workman can be trained to use it in two or three days.

Another field of application for Schoop's metal-spraying process is the so-called cold-welding which will be particularly welcome to the motor-car owner, as it cuts down the time required for repairs as compared with autogeneous or electric welding.

In the aeroplane industry the improved homogeneous pistol should be of the greatest value. In order to save weight, all the dimensions of the metal parts are made as small as possible.

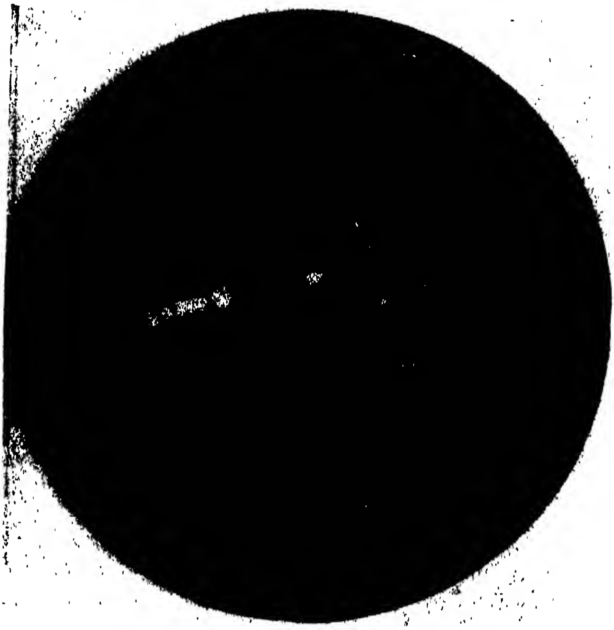

FIG. 1.—Front view of Schoop's homogeneous spraying pistol, oxyhydrogen gas.

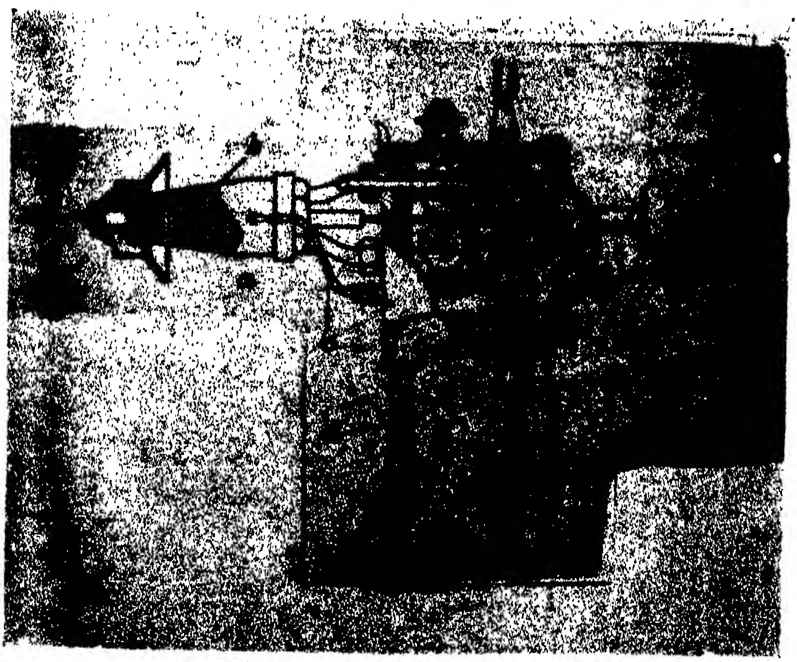

FIG. 2.—Diagram of the pistol.

Oxidation and corrosion of the metal wires, struts, mountings, and stays are liable to become very dangerous. In this connection the process presents again the advantage that aeroplanes, without being dismantled, can be protected against premature destruction at the places liable to become defective. Wooden propellers at their extreme ends and at their edges may be protected, by a sprayed-on metal coating, against mechanical damage through grass, sand, and the splashing of water. For flying boats and the floats of hydroplanes the process is of importance, in the respect that it affords a protection for the duralumin against galvanic decomposition in the sea-water.

Agricultural Research (J. H.)

"Agricultural Research in 1929" (*Royal Agricultural Society of England, London*, 1930, pp. 182, price 1s. 3*d*. post free), which is the fifth volume of the series, contains a summary of recent work by well-known authorities. While written primarily for the farmer with the object of keeping him in touch with the recent advances of science as applied to his industry, it is also of interest to the scientist, for it summarises many important papers which have appeared in publications off the beaten track of the scientific journals, and references to the original papers are given.

In the section on Crops and Plant Breeding, Engledow, in addition to a summary of the development of winter washes for fruit trees, varieties of sugar beet, and the control of rust and smut diseases of cereals, has provided an interesting account of the effect of weather on crops. By restricting wheat to six hours of daylight all except the earliest maturing varieties are prevented from producing any ears at all, but go on tillering and become "grassy." Oats need less sun and more rain than wheat, and the distribution of these crops in Great Britain is in accordance with this. The effect of rainfall on the yield varies with the stage in the development of the plant at which it occurs ; the weather is of importance mainly at certain critical periods in the development of the plant. This may prove useful in forecasting the yields of crops. The water balance and drought resistance in plants are also dealt with.

Mackintosh, in the section on Dairy Husbandry, refers to the extra grazing obtained in the early spring and late autumn by the use of nitrogenous manures. Work on the anatomical differences between the beef and milk types of cattle and studies on the daily activities of the cow (time spent in standing, lying, eating, ruminating, etc.) are reported. High-yielding cows show higher pulse and respiration rates than low-yielding ones ; these functions may be taken as an approximate measure

of the rate of metabolism. The methods and accuracy of milk and fat records, the effect of the calving interval in affecting the lifetime yield of a cow, and the effect of gland pressure and the method of milking on milk secretion and the composition of milk are also discussed.

In a section on Agricultural Economics Orwin deals with the utilisation of surplus milk, which arises mainly on account of the seasonal variation in the supply, and other problems of economics in farm management and marketing. The causes of, and probable effects of policies on, the present agricultural depression are outlined. Owen, who writes on Agricultural Engineering, in addition to sections on the different agricultural implements, devotes one to the use of electricity in agriculture, a matter which is likely to become of increasing importance in this country in the near future.

A brief review of the late Prof. T. B. Wood's extensive and valuable contributions to animal nutrition research is included in Crowther's section on Animal Nutrition. Feeding standards for dairy cows, the food requirements of pigs, the value of sugar beet pulp as a food for cattle and pigs, and recent work on mineral deficiencies, including iodine, in feeding stuffs are among the many subjects discussed. The use of ultra-violet radiation in safeguarding the supply of vitamine D has been of use in intensive poultry management.

Soils and Fertilisers are dealt with by Russell; he refers to the establishment of Empire research stations and to the main outcome of the Second International Congress of Soil Science—a practicable basis of soil classification. Methods of land reclamation at present in progress in Holland and Denmark, as well as extensions of cultivated land in the British Empire, are described. Interesting accounts are also given of the effects of fertilisers on the composition and quality of the crop (barley and potatoes) and the effect of weather on the action of manures.

Minett, in the section on Veterinary Science, discusses the relation between the bacillus of contagious abortion in cattle and that of undulant fever (Malta fever) in man. The control of contagious abortion by vaccination and the eradication by the diagnosis and disposal of infected animals are also considered. The progress of eradication schemes for tuberculosis in the United States and Canada by the "Accredited Herd" and "Area" plans are reported; these show considerable success. Canine distemper and methods of combating the warble fly are also discussed.

Many of these problems arising from applied science suggest new lines of work in pure science; frequently also they bridge the gap which often exists between many of the pure sciences as they are at present organised.

Cattle and Protein (J. H.)

In the "Minimum Protein Requirements of Cattle" (*National Research Council, Bulletin No. 67, Washington*, 1929, pp. 84) Mitchell has brought together the results of numerous experiments made in the United States, and particularly at the Missouri Agricultural Experiment Station. A review of the whole subject, together with a bibliography, is included; it is discussed under various headings—requirements for maintenance, growth, fattening, pregnancy, milk production, and muscular activity.

The results are worked out in terms of animal expenditures or storages of protein and not, as protein requirements have hitherto been expressed, as digestible crude, or true, protein. They are thus minimal in the truest sense of the word in that they represent digestible protein possessing a biological value of 100. The results show a much lower minimal requirement of protein than that obtained by other investigators; for maintenance 0·19 gm. protein per 1,000 gm. body weight instead of 0·6 gm. were found to be sufficient, and for growth the estimates are 50 per cent. or more lower than Armsby's standards. This fact, and the variations which appear when experiments run on similar lines, but at different institutions (Armsby's co-operative experiments), are compared, would seem to indicate that there are many factors, as yet incompletely realised, which affect the utilisation of protein by the body. The expression of protein requirements in terms referable to the animal rather than to its feed emphasises this, and the investigation, apart from its other uses, will form a basis for determining what these factors are; for example, the biological value of the proteins in farm feeding stuffs and the effects of minerals and vitamines on protein utilisation. In addition there are a number of interesting observations contained in the account. The use of average digestion coefficients for proteins with rations containing considerable amounts of starch and sugar will always overestimate the content of digestible proteins. The "deposit-protein" in the tissues is only eliminated after a considerable period of nitrogen-free feeding. The percentage composition of an animal is more closely related to the weight attained than to the age of the animal, *i.e.* the weight of a steer is a better indication of its stage of growth than is the age: the more rapidly an animal increases in weight on a given ration the more rapidly will it fatten. The deposition of fat in adipose tissue involves cellular proliferation, and therefore fattening possesses its own nitrogen requirement distinct from that of growth: the fattening even of mature animals increases the protein requirements of the animals. The change from a prolonged regime on an inadequate diet,

that has induced a stunted and unthrifty condition, to an adequate ration, will tend to occasion a rapid resumption of growth that, for a time at least, will exaggerate the nutritive value of the second ration. These, and numerous references to work done on other species, make the publication one of general interest to those who are working on problems of animal nutrition and growth.

The Drumm Battery (S. K. Lower)

The *Electrical Review* for August 8, 1930, contains the first official statement to the Press of the advantages claimed for the Drumm battery. Hitherto, published information had been scanty and somewhat misleading. The statement, issued by the Minister for Industry and Commerce of the Irish Free State, concerns the applicability of the battery to electric traction. Stupendous claims have been made from time to time, and a revolution in the battery world appeared to be inevitable, but most of the information has been without authoritative foundation. Two articles by R. N. Tweedy in the *Electrical Review* of August 22 and September 19 have done nothing to inspire confidence in the battery, and subsequent correspondence (*Elec. Rev.*, Oct. 3 and 24) indicates a feeling of uneasiness.

The facts, as far as they can be ascertained, are that the battery, which was designed by Prof. James Drumm of Dublin, consists of positive plates almost identical with those of the Nife cell, negative plates practically indestructible, and an alkaline electrolyte which need be renewed but once a year. The resistance of the battery is so low that it can be charged and discharged at very high rates without overheating. A battery can be recharged completely in less than an hour, sometimes even in eight minutes. It is free from corrosive fumes, and its construction enables it to withstand vibration without damage. These advantages render it particularly suitable for traction purposes, and the present indications are that only in this direction is the Drumm battery likely to supersede existing methods.

Public trials of the battery fitted to an experimental coach on the Inchicore-Hazelhatch section of the Great Southern Railway were made during August. Apparently some degree of satisfaction was obtained, but the tests cannot be regarded as conclusive. The life of the battery is still to be determined. The energy efficiency is stated to be 75 per cent. under practical conditions, and the current efficiency 95 per cent. It is believed that a completely new electrochemical phenomenon is involved. Theories have been propounded, but the subject is still under

investigation. Meanwhile, patent applications have been filed by Prof. Drumm and Celia Ltd., and the development of electric traction in the Irish Free State remains in the balance.

The Spahlinger Vaccine (T. H.)

The committee controlling a demonstration in Norfolk of the efficiency of Mr. Henry Spahlinger's vaccine for the immunisation of cattle against tuberculosis have reported their observations to representatives of the daily press. A copy of the document submitted to the press has been forwarded to us by Sir Frederick Maurice, a member of the committee.

The committee of seven, including three farmers and three general practitioners, two veterinary and one medical, tell an interesting story. From 1916 Spahlinger performed five experiments, a summary of which is contained in the document, on cattle and calves in Switzerland. His claim that he had produced a simplified and cheap vaccine of dead germs which immunised cattle against tuberculosis impressed the committee.

On December 21 and 31, 1929, and March 21, 1930, the committee inoculated eighteen calves intravenously, each with a single dose of varying strength of the undefined Spahlinger vaccine. The calves, of unrecorded age, origin, and state of health, were fed during the experiment on cow's milk, which may or may not have contained living tubercle bacilli, and were housed under the conditions of Captain R. G. Buxton's farm. On July 12, 1930, each of the eighteen calves was inoculated intravenously with a " massive " dose (15 milligrammes) of the undefined virus, *viz.* " extremely virulent living bovine tubercle virus." As controls four unvaccinated calves were inoculated, two intravenously and two subcutaneously, with unrecorded doses of virus ; the former two died on August 11 and 16, 1930, the latter two died on September 7 and 10, 1930. The two veterinary practitioners conducted the post-mortems and, on ascertaining from " independent laboratories " that the organs contained large numbers of tubercle bacilli, certified that the four control calves died of acute generalised tuberculosis. On November 7, 1930, one calf which had received " the smallest dose of vaccine " (0·05 c.c.) died and " its autopsy and microscopic examination revealed that the cause of death was tuberculosis." Under date November 23, 1930, the committee reported that " the remaining seventeen vaccinated calves are alive and in good health, and consequently that it is proved that " the Spahlinger simplified bovine vaccine confers on calves an extremely strong resistance against tuberculosis, even when the virus is administered intravenously in such massive doses as to kill vaccinated calves in about a month."

The committee are to be congratulated upon the masterly drafting of their general impressions and rapidly drawn conclusions which have persuaded a number of reputable people into feeling convinced "of the great value to humanity" of Spahlinger's work, and that "the moment has arrived when the results of his recent demonstrations should be made known to the world." Possibly a minute fraction of the world might desire to know the formula and technique by which Spahlinger prepared the vaccine and virus, also the subsequent history of the seventeen vaccinated calves; but the publication of a second instalment is not promised in the document.

From answers in the House of Commons it would appear that the vaccine will be tested upon the orders of the respective Ministries of Agriculture in England and Northern Ireland. It is conceivable that in 1934 there will be forthcoming some reliable evidence upon which the statements in this interesting document may be evaluated.

An Attempt that Failed (R. R.)

My brother Charles Ross was born March 10, 1864, and was the fifth son of Major-General Sir C. C. G. Ross, of the Bengal Army. He entered the Norfolk Regiment in 1884 and was then attached to the Egyptian Army for a time, 1893–4; and to the Staff College, 1897–9, where he was Instructor. He was Company Commander at Sandhurst 1905–8, and served in South Africa 1900–2 (despatches). During the Great War he commanded a brigade and then a division (despatches). But his principal work was to warn the British public during the South African War of the evident intention of the German Emperor to attack this country; and this view was carefully and correctly published in his book *Representative Government and War*, followed by later books. It was amusing to read reviews of that work in the British press, and I do not mind saying that I have never read such ridiculous reviews of any previous work. The commercial gentlemen who decide the affairs of this country seem to think that my brother was trying "to fry fish of his own." As well known, the whole attempt to instil brains into the modern Briton was an utter failure even when made by Lord Roberts, and the result was that this country was subjected to a great war in which it lost millions of lives and many millions of money. I speak with some heat, because I have had a precisely similar experience in connection with the prevention of malaria, and consequently possess a rather low opinion of the British commercial gentlemen mentioned and also of the politicians whom they admire so much. They are naturally so proud of their merits that they are not willing to learn anything.

General Ross married Marion, daughter of the late Rev. J. E. L. Schreiber. Mrs. Ross is a poetess of distinction. General Ross died at his house in Shawford, Hants, on December 21 last.

A Correction (R. R.)

I have always taught that the amount of malaria in a locality must depend upon the number of mosquitoes to each person there. But of course this assumes that all the mosquitoes are feeding on human blood. Recently, however, *The American Journal of Hygiene*, January 1931, published an article by L. W. Hackett and A. Missiroli which proves that *Anopheles maculipennis* feeds not only on human beings but often more frequently and easily on domestic animals. This will of course reduce the proportion of these mosquitoes which are biting men, and must therefore modify the equations obtained by Miss H. P. Hudson and myself on Pathometry—on which a special book is being brought out by myself very shortly, being a reprint of an article in three parts published many years ago by the Royal Society.

Notes and News

The New Year Honours list included the following names of interest in scientific circles : *Baron :* Sir Ernest Rutherford. *Baronets :* Sir John Rose Bradford and Sir Richard Gregory. *K.C.B. :* Dr. F. E. Smith, secretary to the Committee of the Privy Council for Scientific and Industrial Research. *K.C.M.G. :* Dr. A. W. Hill, Director of the Royal Botanic Gardens, Kew. *Knights :* Dr. E. G. Graham-Little, Dr. R. W. Livingstone, Vice-Chancellor of Queen's University, Belfast. *C.B. :* Mr. R. L. Hobson, keeper of Ceramics and Ethnography, British Museum. *C.B.E. :* Prof. Sidney Russ, professor of Physics, Medical School, Middlesex Hospital ; Mr. J. J. Shaw, secretary to the Seismological Investigations Committee of the British Association. *O.B.E. :* Dr. W. M. Aders, recently economic biologist, Zanzibar ; Mr. R. S. Capon, Superintendent of Scientific Research, Royal Aircraft Establishment, Air Ministry ; Mr. J. S. Corbett, secretary of the Empire Forestry Association, *M.B.E. :* Dr. V. E. Wilkins, Assistant Principal, Ministry of Agriculture and Fisheries.

Prof. Hans Fischer of Munich was awarded the Nobel prize for chemistry for the year 1930, and Dr. Karl Landsteiner, of the Rockefeller Institute of Medical Research, the prize for medicine. Dr. Landsteiner discovered that human blood is of four different types, which do not necessarily mix one with another.

We have noted with regret the announcements of the deaths of the following men, well known in the world of science, during the last quarter: Mr. A. B. Basset, F.R.S., mathematician; Sir Otto Beit, F.R.S.; Dr. H. Borns; Prof. A. A. T. Brachet, For. Mem. R.S., of Brussels, embryologist; Dr. J. W. Evans, F.R.S., geologist; Prof. E. Goldstein, of "canal" rays fame; Dr. W. Haffkine, bacteriologist; Prof. E. W. Hyde, mathematician; Prof. H. Kniep, director of the Institute of Plant Physiology, Berlin-Dahlem; Mr. R. G. Lunnon, physicist of Armstrong College, Newcastle-on-Tyne; the Rt. Hon. Lord Melchett, F.R.S., chairman of Imperial Chemical Industries, Ltd.; Prof. J. Munro, Emeritus professor of mechanical engineering in the University of Bristol; Sir F. Ogilvie, at one time Director of the Science Museum, South Kensington; Sir Charles Parsons, inventor of the steam turbine; Capt. Otto Sverdrup, the Scandinavian explorer; Prof. J. H. Teacher, pathologist, of the University of Glasgow; Prof. F. Wald, chemist, of Prague; Prof. T. Wibberley, of Cork, agriculturist.

The Gold Medal of the Royal Astronomical Society has been awarded to Prof. W. de Sitter, who has also been invited to deliver the George Darwin Lecture this year. A Jackson-Gwilt Medal has been awarded to Mr. Clyde W. Tombaugh of Lowell Observatory, Flagstaff, Arizona, for his discovery of the planet Pluto.

Dr. C. E. P. Brooks has been awarded the Buchan prize by the Royal Meteorological Society. Mr. R. G. K. Lempfert has been re-elected President of the Society.

Prof. R. Ruggles Gates has been elected President of the Royal Microscopical Society.

The La Caze prize of the Paris Academy of Sciences has been awarded to Henri Abraham for the whole of his physical work.

Dr. T. A. Stephenson, who was a member of the Great Barrier Reef Research Expedition, has been appointed to succeed Mr. L. Hogben as professor of Zoology in the University of Cape Town.

Dr. Wilhelm Schmidt has been appointed to succeed the late Prof. Exner as professor of Geophysics in the University of Vienna.

Dr. W. F. G. Swann, who is now director of the Bartol Research Foundation of the Franklin Institute, has been elected President of the American Physical Society.

Einstein has accepted an invitation to become Cecil Rhodes Memorial Lecturer at the University of Oxford, and will spend the summer term in Oxford.

Prof. A. J. Dempster has been awarded the annual prize of the American Association for the Advancement of Science

for his demonstration of the wave properties of protons. The prize of $1,000, awarded annually for an outstanding paper read at the winter meeting of the Association, was given for a paper by Messrs. Tuve, Hafstad, and Dahl of the Carnegie Institute, dealing with high-voltage tubes. Voltages in excess of 5 million volts have been obtained with a Tesla coil having a tuned spark in the primary circuit. Pyrex vacuum tubes have been constructed to stand 2 million volts, and by running such tubes at 1·4 million volts, cathode rays with velocities within 1 per cent. of that of light have been obtained. The experiments are being continued.

The University of Bristol has received a donation of £25,000 from Mr. W. M. Wills to endow the chair of theoretical physics in the Wills Physical Laboratory. The chair is now held by Prof. J. E. Lennard-Jones. The University has also received a gift of £50,000 from the Rockefeller Foundation as a contribution to the cost of present and future research in the Physical Laboratory.

The Pilgrim Trust (founded by E. S. Harkness, of New York) made a grant of £16,000 to the Royal Institution to complete the sum required to pay for the modernisation of the Royal Institution building. The total cost of the reconstruction has exceeded £90,000. The famous theatre is now provided with proper exits, and the pillars, which used to support the gallery, have been removed.

The Council of the Royal Society has decided that, in future, seventeen new fellows shall be elected each year. Since 1848 the number has been only fifteen and, if that number was satisfactory over eighty years ago, it can hardly be disputed that the increase of two is conservative!

The centenary meeting of the British Association will be held in London during the period September 23–30. General Smuts will be president. The sectional presidents are as follows: Section A (Mathematical and Physical Sciences), Sir J. J. Thomson; Section B (Chemistry), Sir Harold Hartley; Section C (Geology), Prof. J. W. Gregory; Section D (Zoology), Prof. E. B. Poulton; Section E (Geography), Sir Halford Mackinder; Section F (Economic Science and Statistics), Prof. E. Cannon; Section G (Engineering), Sir Alfred Ewing; Section H (Anthropology), Prof. A. R. Radcliffe-Brown; Section I (Physiology), Dr. H. H. Dale; Section J (Psychology), Dr. C. S. Myers; Section K (Botany), Prof. T. G. Hill; Section L (Educational Science), Sir Charles Grant Robertson; Section M (Agriculture), Sir John Russell. In celebration of its centenary the Association is endeavouring to raise a substantial endowment fund to place its finances on a more certain basis. The meeting is the first to be held in London and, in place of

the usual booklet description of the town and neighbourhood in which the meeting is held, it is intended to issue a book containing an historical survey of science in London during the last five or six centuries.

The meeting has been arranged so that the delegates and members can join in the functions arranged to celebrate the centenary of Faraday's discovery of electromagnetic induction. These include an exhibition at the Albert Hall designed to show the hundred years' development of Faraday's discoveries.

June 13 next will be the centenary of the birth of James Clerk Maxwell. It is proposed to celebrate the event by appropriate meetings and addresses at Cambridge on October 1 and 2 immediately following the Faraday and British Association centenary celebrations in London. Among others, Einstein, Planck, and Sir J. J. Thomson will take part.

The Cambridge University Press is publishing, on behalf of the Royal Irish Academy, a collected edition of the mathematical papers of Sir W. R. Hamilton. The first volume, *Geometrical Optics*, will probably have been released before this note appears. Three other volumes will follow.

The Cambridge University Press will also publish very shortly a new popular book by Sir James Jeans entitled *The Stars in Their Courses*, based on the talks which he gave for the B.B.C. last autumn, and a new *Text-book on Spherical Astronomy* by Dr. Smart.

The Society of Chemical Industry will celebrate its Jubilee by holding an exhibition in the Central Hall, Westminster, during the week July 13–18. The Department of Scientific and Industrial Research, the British Chemical Plant Manufacturers' Association, and various Research Associations will co-operate to make the exhibition as interesting and attractive as the Achema Exhibition in Frankfurt.

The Wellcome Foundation, Ltd., is about to erect a new medical and chemical research building at the corner of Gordon Street and Euston Road on the site, 225 × 135 feet, now partly occupied by their Bureau of Scientific Research. During many years the Foundation has maintained medical and chemical research laboratories, but recent developments have made it necessary to co-ordinate and extend these activities. The new building will furnish the additional accommodation required, and be provided with the most modern research equipment. Mr. Septimus Warwick, F.R.I.B.A., is the architect.

Abstracts of Dissertations for the Degree of Doctor of Philosophy, Vol. III, published by the Clarendon Press (price 5*s.*) for the University of Oxford, contains abstracts of fifteen of the successful theses presented to the University during the

session 1929–30. Eight of these belong to the arts group, five to the physical sciences (including mathematics and chemistry), and two deal with biological subjects. In his thesis, entitled *Some Physical Properties of Single Metal Crystals*, Fraser dealt with the electrical conductivity of aluminium, and found that to within the accuracy of the experiment, namely, 1 per cent., there is no correlation between the resistivity of a single crystal and its orientation, though such crystals have a conductivity 1 per cent. less than that of the annealed aluminium from which they were made.

Millikan described his 1930 experiments on cosmic rays in the *Physical Review* (December 1930 and February 1931), and in his address as the retiring president of the American Association for the Advancement of Science (*Science*, January 2; *Nature*, January 31). If the rays consisted of high-speed electrons they would "spiral round the lines of force of the earth's magnetic field and thus enter the earth more abundantly near the earth's magnetic poles than in lower latitudes." To test this point experiments with an improved electroscope were made at Churchill, Manitoba, 730 miles from the magnetic pole, and at Pasadena, with the result that the mean intensity of the radiation was, within the limits of experimental error, the same at both places. The rays therefore enter the earth's atmosphere as either waves or photons. Further, somewhere in the atmosphere below a height of 15·5 kilometres, the intensity of the ionisation in a vessel exposed to the rays goes through a maximum, and this implies that the rays have not passed through an appreciable amount of matter in their path from their place of origin to the earth. The conclusion is inevitable, that the rays originate in interstellar space. Using the Klein-Nishina formula to connect energy and penetrating power, the spectral bands of the cosmic-radiation correspond roughly to the energy released when helium, oxygen, silicon, and iron are formed out of hydrogen. Bowen's discovery, that the "nebulium" lines arise from such elements distributed in interstellar space, lends support to this hypothesis of the origin of the rays. Stellar energy must be supplied by the annihilation of atoms in the intensely hot interiors of the stars. To complete the hypothetical cycle, hydrogen atoms must somewhere be built up from the radiant energy!

Bulletin No. 76 of the National Research Council, Washington, is a handbook of the scientific and technical societies and institutions in the United States and Canada. The arrangement is alphabetical, and under each heading is given the address and name of the secretary of the Society, its history and object, the qualifications for membership, times of meetings, research funds, and publications. The total number of societies

in the United States falling within the scope of the *Bulletin* is about 800, and in Canada 91. The oldest American society would appear to be the Academy of Natural Sciences of Philadelphia, instituted in March 1812, and incorporated in 1817. The interests of this society are now mainly biological. Its total membership is 1,819. The American Association for the Advancement of Science was founded in 1848, and is now divided into sections on the lines of the British Association. The total membership is over 18,500. The National Academy of Sciences, Washington, was incorporated by an Act of Congress approved by the President, Abraham Lincoln, in March 1863. The membership is limited to 300, which includes a maximum of 50 foreign associates. The New York Academy of Sciences was organised as the Lyceum of Natural History in 1817 and the Smithsonian Institution in 1846. The Nova Scotian Institute of Science, the first of the Canadian societies, was formed in 1862. The Royal Society of Canada dates only from 1883.

The *Annual Report of the Director of the Bureau of Standards*, Washington, D.C., for the year ending June 30, 1930, contains, in addition to statistics relating to personnel, salaries, fees, etc., a brief account of the multifarious activities of the Bureau. Selecting a few items at random from the fifty odd pages in the *Report*, we note that a new material for length standards is being sought for, and a stainless steel, containing 14 per cent. chromium and 0·5 per cent. carbon, seems likely to satisfy the requirements of polish, homogeneity, hardness, thermal expansion, and resistance to corrosion. The Waidner-Burgess standard of light (*i.e.* an opening 1 square centimetre in area in a black body at the freezing-point of platinum) has been studied thoroughly, and developed as a reproducible, convenient, and inexpensive standard. It gives 58·84 international candles. For the determination of the acceleration due to gravity at Washington a number of fused quartz pendulums have been designed. For pressure measurements up to 1·5 metres of mercury in gas thermometry, a manometer in which the mercury level can be determined to 0·0001 mm. has been constructed. The Structural Materials Testing section has issued a handbook for house owners, instructing them how to carry out house repairs, and the Textile section is preparing a white stainless mineral oil for use in knitting machines. The freezing-point of platinum was found to be $1773° \pm 2°$ C. on the international scale. A service for the calibration of X-ray dosage meters has been established, and a vast amount of research has been carried out in connection with aeronautics.

The December issue of the *Bureau of Standards Journal of Research* contains Paul R. Heyl's account of his redetermina-

tion of the constant of gravitation G. His final result is $6{\cdot}670 \pm 0{\cdot}005 \times 10^{-8}$ cm.^{3} gm.$^{-1}$ sec.$^{-2}$. The value obtained by Boys and by Braun was $6{\cdot}66 \pm {\cdot}01$, so that the agreement is good.

The time of swing method recommended by Braun was employed, *i.e.* the time of swing of the suspended system was observed when the line joining the centres of the large attracting masses was (*a*) along the rest position of the suspended system ($T = 1{,}754$ sec.), and (*b*) at right angles to this position ($T = 2{,}081$ sec.) (see plan, Fig. 1). The attracting masses were two cylinders of tool steel 28·5 cm. long, 20 cm. diameter, and

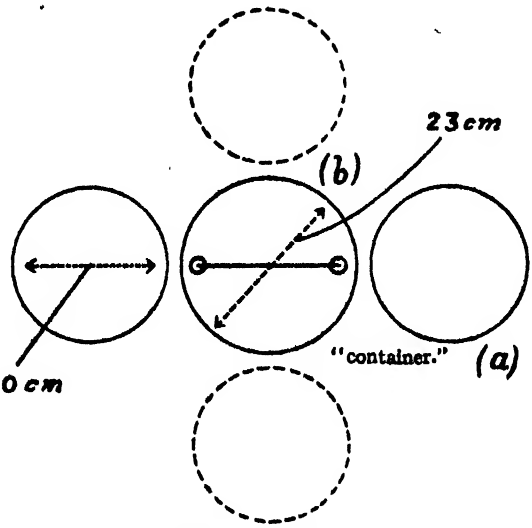

Fig. 1.—Plan.

about 66 kgm. mass. These were forged from an ingot 30 cm. in diameter so as to ensure the absence of blow-holes, etc. The small masses were spheres of three different materials, gold, platinum, and glass, and in each case the mass of a sphere was about 50 gm. Two such spheres (*e.g.* of platinum) were suspended by tungsten lamp filament from the ends of a horizontal aluminium rod 20 cm. long (mass 2·4 gm.). The rod, strengthened by a truss of fine copper wire, was suspended by a piece of the same tungsten lamp filament 1 metre long. The rod carried also a small mirror, which reflected light from a scale 3·5 metres away in an adjoining room into a telescope near the scale. The total mass of rod, mirror, spheres, etc., was *c.* 102 gm. The whole of the " torsion pendulum " was placed in a container in which the air pressure was reduced to 2 mm. of mercury. The lower part of this container was made of

wrought iron (in the later experiments) to shield the slightly para-(or dia-)magnetic spheres from the earth's magnetic field, distorted by the presence of the steel cylinders which were placed with their axes vertical just outside the cylindrical container. The beam from which the cylinders were hung by steel rods was itself supported above the airtight container, and could be turned about a vertical axis coinciding with the axis of the tungsten filament.

One set of time observations involving twenty-four transits of the suspended system through its central position for positions (*a*) and (*b*) of the cylinders occupied thirteen hours, and during this period the observer remained in position in order that the results might not be affected by alteration in the position of his mass.

The formula for the attractive force exerted by a finite cylinder at an external point is given for the first time. The solution is a series of zonal harmonics which converge so rapidly that 7 terms only are required for an accuracy of 1 in 100,000. Further, the gravitational field produced by the cylinders in which the spheres move is not uniform, and use of the simple formula applicable to a uniform field would give a value of G 60 per cent. too great.

The values of the gravitational constant obtained with the gold spheres had rather a large spread (6·683 to 6·672) (? because the gold had absorbed mercury vapour), and are less reliable than the others. The values for platinum lay between 6·667 and 6·661, for glass 6·678 to 6·671. The reason for the difference between the values with platinum and glass could not be ascertained, but experiments with an Eötvös balance showed that the difference was not due to any variation in G.

From a recent article in *De Visscherij Courant*, it appears that the slipper limpet is persistently multiplying in the waters of Zealand in Holland. Frequent warnings by the coastal fishery authorities at Amsterdam have been issued, pointing out the necessity of exterminating these parasites in their early stages, and the Dutch fishery inspectors have been instructed to collect specimens. In the month of November, 184 specimens were sent in by one Dutch firm. The breeding powers of this pest are unusually great, and occasionally seven or more specimens have been found attached to a single oyster. As the food of the slipper limpet is the same as that of the oyster, it is feared that a great multiplication of the pest would have a serious effect upon the quality of the much-esteemed Zealand oyster, and Dutch oyster-breeders and merchants have been enjoined to take all possible measures towards its destruction.

The Ministery of Agriculture and Fisheries desires to

impress upon all persons who import Dutch or other foreign oysters for the purpose of relaying, the very great importance of taking every precaution to avoid laying down oysters with slipper limpets attached to them. With this in view, every oyster should be examined for slipper limpet spat before it is laid down. The slipper limpet is believed to have been introduced into England about 1880 with a consignment of American oysters intended for relaying, and, starting from Brightlingsea, its spread has been continuous. The circumstances most favourable to the growth of oysters are those also in which the slipper limpet is most likely to flourish. Where, therefore, the pest has reached oyster beds, it has multiplied rapidly, owing to the abundance of suitable food and other favourable conditions.

When once the slipper limpet has become established on an oyster ground, it is practically impossible to eradicate it, and it behoves all those who are interested in the welfare of our oyster fisheries to see that further stocks are not introduced to our grounds on imported oysters.

Paper No. 62, issued by the Safety in Mines Research Board, contains an account of further work by H. J. Burgess and R. V. Wheeler, on the ignition of firedamp by the impact of hand picks against rocks. It is shown that firedamp can be ignited when a pick is struck in a particular manner against sandstones, especially those quartzitic sandstones which are easily crushed by the impact of the pick point. The dangerous stroke is one which produces a localised yellow flash at the moment of impact. Showers of sparks obtained, *e.g.*, by a blow on iron pyrites, are not dangerous.

Paper No. 63 deals with the propagation of combustion in powdered coal. It appears that the oxidation of coal which results in spontaneous combustion proceeds in two distinct stages. Firstly the slow oxidation which is a suitable environment may produce sufficient heat to raise the temperature of the coal to a point at which the second stage, active low-temperature combustion or smouldering, commences. The conditions under which this smouldering, once started, will permeate a heap or train of coal dust have been studied at the Fuel Research, Greenwich, by H. E. Newall and F. S. Sennatt. The combustion will spread through a mass of fine coal at temperatures as low as 130° C. The rate of propagation may be as slow as 5 inches per hour, but it increases with the percentage of oxygen in the atmosphere and with atmospheric temperature. The evolution of hydrocyanic acid is appreciable, especially (in the cases examined) when the temperature ranges from 500° to 600° C.

The third of the very excellent booklets published by the

Safety in Mines Research Board under the general heading, *What every mining man should know*, is entitled, *How Some Firedamp Explosions are Prevented*, and costs threepence at H.M. Stationery Office. It deals with lighting (including, of course, the Davy safety lamp), and with the casing of electrical machinery to prevent sparks from igniting the mine gases. In connection with the latter is a series of photographs showing the spreading of flame in enclosures of various shapes, a description of the method used to observe the rise of pressure in an enclosure during an explosion and of the method of lessening the force of the explosion inside a casing by permitting the escape of the heated gases through a very narrow gap between the two flanges on its upper and lower halves. The name of the writer of the book is not given, but he is to be congratulated on the altogether admirable manner in which he is presenting the results of the research paid for from the Miners' Welfare Fund, in a form which should be appreciated by the miners themselves.

CORRESPONDENCE

To the Editor of SCIENCE PROGRESS

SIGNING REVIEWS

From R. A. FISHER, Sc.D., F.R.S.

DEAR SIR,—About 110 pages of SCIENCE PROGRESS are devoted every quarter to reviews, notices, abstracts and, best of all, essay-reviews, dealing with recent scientific work, and I am writing to solicit your attention to the proposition, upon which you are, perhaps, in a position to sound the opinions of other readers, *that all reviews without exception should carry the signature of the reviewer.* Several points seem to me to deserve consideration in this regard.

To the general reader the interest of a review is greatly enhanced by a knowledge of its author, for this allows him to compare the opinions expressed with others which he associates with the same name. The interest of an onlooker in any branch of science is not concentrated only on the opinions under discussion, but on opinions in relation to personalities. Even if his reading is desultory and occasional, he will like to know also whose opinions are being sought upon each particular topic.

To the more persevering student it is not only of interest, but also of some importance, that the reviewer, on whose opinions he may have to rely, should not be anonymous. There are many statements which we should accept from a well-known authority in whom we have confidence, but which, as anonymous opinions, carry not the slightest weight. On the other hand, since no editorial care can avail altogether to exclude ignorant or prejudiced statements, the name of the reviewer will often serve as a timely warning to his readers. A name may obviously serve, on some points as a guarantee, and on others as a warning.

I believe these considerations have, in recent years, received increasing recognition among writers on Science. Anonymity has its origin in popular journalism, where, perhaps, it is still a convenience, although some contemporary scientific journals do still admit an undue proportion of unsigned

material. The review of Prof. Gates's book by β_{10} is a case in point; for, on different views of the identity of the reviewer, it might appear either as a searching and penetrating criticism, or as an attack based upon a personal prejudice against genetical methods.

Yours faithfully,
R. A. Fisher.

Rothamsted Experimental Station,
Harpenden, Herts.
August 12, 1930.

HEREDITY IN MAN

From Prof. R. Ruggles Gates, Ph.D.

Dear Sir,—I think it will be clear from my previous letter in your January number (p. 515), that I have no quarrel with biometricians or the legitimate use of biometric methods in the study of heredity, so long as they do not affect to ignore the experimental Mendelian results, which must always be on safer grounds than any merely mathematical analysis. Each method has its limitations, although these are more serious in the case of biometrical treatment, because there is not the same facility in checking conclusions by facts as with the experimental method.

The experimental or observational and the mathematical methods are really complementary. This statement applies not only to biology, but also in other sciences, for example astronomy, where physics and mathematics co-operate in, let us say, the elucidation of stellar structure. But it is a universally recognised principle that physical observations and the deductions from them are to be used as the basis for mathematical analysis whenever they are available or obtainable. Between biology and mathematics the same principles of co-operation apply. Biometrical methods are often useful in determining the cogency of the conclusions to be drawn from certain experimental results; and in cases where, as in some forms of quantitative inheritance, the individuals cannot be classed in categories on the basis of measurements, mathematical treatment may nevertheless make it possible to draw certain useful conclusions. But it needs to be remembered that mass statistics are subject to innumerable errors, and a purely mathematical treatment of them may lead to a result which is not analytical in any biological sense.

The aim of my book on *Heredity in Man* has been to apply genetical conceptions and methods to the fields of anthropology and medicine, but there was no desire to exclude any biometrical

methods or results which could aid in the elucidation of this great theme. The vast possibilities in the application of genetics to anthropology are only beginning to be realised. This is particularly true of racial crossing, which is now beginning to be studied in an analytical way in many parts of the world. It is to be hoped that anthropologists will realise that anthropometric indices, however useful they may be for certain purposes, can only serve to a very limited extent as the basis for a genetical analysis of racial differences.

In the province of medicine, the introduction of genetical conceptions is of at least equal importance. The medical literature shows great improvement in this respect even in the last five years. It is obvious that every medical student and practitioner should have enough genetical knowledge to be able to understand the different forms of Mendelian inheritance, and to compile a pedigree showing how any abnormal condition with which he comes into contact has been inherited in the patient's ancestors and collaterals. This by no means excludes the use of the biometric method in those cases where individuals cannot be classified by inspection, but even in such cases a genetical background is essential if progress is to be made in the analysis of inheritance.

Scientific societies for the study of human heredity, composed partly or wholly of medical men with some knowledge of genetics, are now being established in various countries. An international committee for the purpose of correlating such investigations has been formed by the International Federation of Eugenic Organisations. The study of the inheritance of abnormalities and defects in man will fall to an increasing extent in future within the province of preventive medicine. The field of racial crossing and the inheritance of racial differences is, on the other hand, primarily within the domain of anthropology, and it is greatly to be hoped that the present generation of anthropologists will introduce genetical methods and conceptions into their studies. All over the world exist countless cases of interracial crosses, awaiting analysis by genetical and pedigree methods, recognising the recently proven fact that genetic segregation takes place in the inheritance of racial differences, as in that of abnormalities.

R. Ruggles Gates.

King's College, London,
January 29, 1931.

ESSAY-REVIEWS

THE AVAILABILITY OF INFORMATION. By H. F. BARNES, B.A., Ph.D., being a review of: (1) **The Plant Quarantine and Control Administration,** by GUSTAVUS A. WEBER [Pp. x + 198] (Washington: Institute for Government Research of the Brookings Institution, 1930. Price $1.50); (2) **The Bureau of Entomology,** by GUSTAVUS A. WEBER [Pp. xii + 177] (Washington: Institute for Government Research of the Brookings Institution, 1930. Price $1.50); which are Service Monographs of the United States Government, Numbers 59 and 60.

IN the economy of plant and animal life the availability of food is of paramount importance. In like manner information should be available to educationalists in easily assimilable form. In the United States this was realised after President Taft had told Congress that an inquiry was being made under his direction into the efficiency and economy of the methods of prosecuting public business, namely, into the vast organisation of the United States national government. The Institute for Government Research of the Brookings Institution undertook the preparation of a series of monographs giving a detailed description of each of the distinct services of the government. One can understand how diverse and far-reaching are these services when it is realised that up to the present over sixty of these service monographs have been published.

The Brookings Institution is preparing these monographs with a view to producing " documents that will be of direct value and assistance in the administration of public affairs." It claims that " to executive officials they offer valuable tools of administration," " to members of Congress the monograph should prove of no less value " and, to the public, " knowledge of the organisation and operations of their government."

We have before us two of these monographs, those dealing with Plant Quarantine and Control Administration and the Bureau of Entomology. Each volume, as are all in the series, is of uniform plan, giving the history and development of the establishment, its activities and its organisation. In addition there are a compilation of the laws and regulations governing its operation, financial statements, and a full bibliography relating to the service and its operations, followed by an adequate index.

To those interested in organisations these books cannot fail to appeal, whether solely for the sake of the information they contain, or, to those more intimately connected with the subjects, for the means whereby improvements can be more readily obtained. To entomologists throughout the world these books must be of extraordinary interest because they set forth in such clear language the growth of the entomological services of a nation which leads the world in this subject, at any rate in its more applied directions. The sections on the activities of the bureau of entomology and the plant quarantine and control administration are perhaps the more interesting portions of the volumes.

We think that the importance of this series of monographs to the general public, claimed in the foreword, is liable to be underestimated. It is odious to make comparisons, but in what other country are similar sources of information regarding the activities of comparable organisations available? Not the least useful part of the books are the lists of publications of the Brookings Institution.

NATURAL SCIENCE AND MODERN LOGIC. By J. H. Woodger, D.Sc., being a review of **A Modern Introduction to Logic,** by L. S. Stebbing, M.A. [Pp. xviii + 505.] (London: Methuen & Co. Ltd., 1930. Price 15*s*. net.)

It is a curious fact that of all the doctrines bequeathed to us by Aristotle the one which has persisted longest with least change has been his logic. There appear to have been two main reasons for this. In the first place, professed logicians, not only in the Middle Ages, but also during the succeeding centuries, right up to the nineteenth, never succeeded in overcoming the narrow limits imposed upon the science by the fact that the only form of proposition which had been studied by Aristotle happened to be the subject-predicate form, with the consequent limitation of deductive inference to the type embodied in the syllogism. On the title page of the above book the author has placed a quotation from Professor Whitehead: "A science which hesitates to forget its founders is lost. To this hesitation I ascribe the barrenness of logic." But, in the second place, pure mathematics developed independently of Aristotle's logic, and later proved sufficient for all the requirements of physical science, so that the latter was not in any way hampered by the restrictedness of logic.

But all this has now been changed. Not only have the narrow bounds of Aristotle's logic been successfully overcome, and a renaissance of logical investigation well started, but the breach between pure logic and pure mathematics has been

filled in, with important results for both sciences. For these sciences are now seen to be special branches of one general science—the science of abstract form or order. In consequence of this it is now difficult to draw any sharp boundary between them, and one school of thought at least believes that the whole of mathematics is deducible from premises which would ordinarily be regarded as "purely logical." This, however, is still a matter of debate. The important point is that both logic and mathematics have now received a great extension in the sense that restrictions formerly thought to be insuperable have now been removed. Pure mathematics is no longer "the science of number and quantity," but one of much more general scope; and modern logic is capable of helping us in dealing with problems which were utterly beyond the possibilities of the older logic. Moreover, the belief, for which J. S. Mill was largely responsible (and which is still common among men of science), that pure mathematics is an empirical inductive science, also disappears, and with it is opened up the possibility of a better understanding of the relation between the empirical sciences, on the one hand, and mathematics and logic, on the other. Similarly, the modern doctrine that mathematical propositions are analytical and not "synthetic" *a priori*, as Kant thought, removes the necessity for the Kantian doctrine of time and space as "pure *a priori* intuitions" and all its consequences.

Now, for this revolution we are indebted very largely to mathematicians themselves. For, apart from the early attempts of Leibnitz and Lambert, the whole movement is traceable to the work of Boole and Schroeder in the middle of the last century, which was enormously deepened and extended by the epoch-making work of Frege and Peano towards its close, culminating, for the present, in the publication of the *Principia Mathematica* of Whitehead and Russell (1910–1925). But contemporary with this line of development, pursued by mathematicians interested in the foundations of their own science, there have also been the important contributions of logicians and philosophers such as C. S. Peirce and his school, de Morgan, Venn and Jevons, and, more recently, W. E. Johnson, G. E. Moore, C. I. Lewis, the late F. P. Ramsey, and the extremely important study of language (in the most general sense) by L. Wittgenstein in his *Tractatus Logico-Philosophicus*.

Now, this whole movement is likely to have the most important consequences for the future development of natural science, particularly for those branches—such as biology—which have so far made but little use of "ordinary mathematics." One of the most important directions in which the

older logic has been extended is that marked out by the logic of relations, as it is developed, for example, in *Principia Mathematica*. This provides us with an intellectual tool of great power and range for the analysis and expression of complicated ideas and is in no way restricted in its uses to "numerical data."

But, until recently, there has been no book to which anyone unacquainted with the subject could turn for a readable general introduction to all these exciting new discoveries in their relation to the old logic. The traditional textbooks usually ignore them. Now, however, this serious defect has been remedied by L. S. Stebbing's *Modern Introduction to Logic*, in which the scientific reader who wishes to obtain an insight into the nature of the intellectual tools he uses intuitively will find everything he requires to prepare him for a study of mathematical logic. The book is divided into three parts. Parts I and II deal (roughly speaking) with the topics which used to be called "Formal Logic" and "Empirical Logic" respectively, but here the traditional doctrines are placed in their proper setting within the vastly extended science which modern research has given us. Of the twelve chapters of Part I, the Aristotelian type of proposition and the syllogism occupy the major part of four, the remaining chapters being devoted to expounding the consequences of introducing relational propositions and the mathematical notions of the variable and function, and to the extension of logical symbolism and much else that is important relating to language in general. Modern research has shown to what an enormous extent ordinary language conceals the true logical form of propositions, and so has led to the confusion of fundamentally different types. The quotation from de Morgan, which heads chapter x: "Every science that has *thriven* has thriven upon its own symbols; logic, the only science which is admitted to have made no improvements in century after century, is the only one which has *grown no symbols*," might be taken to heart also by some of the more backward empirical sciences.

Part II is chiefly occupied with Induction and Causation and their relation to scientific enquiry. Here the reader will find an account of the work that has been done in this department of logic since the time of Mill. There is also a chapter on the special problems presented by the historical sciences, including a section on the methodology of the social sciences.

Part III, which is the smallest—occupying rather less than a quarter of the book—deals with topics which are perhaps chiefly of interest to the philosophical logician, such as the theory of definition, the nature of abstraction, and the relation between logic and pure mathematics. But these questions are

also of importance for any man of science who happens to be interested in the logic of scientific procedure. The book concludes with a brief historical survey of the development of logical theory.

It is inevitable that, even in an introduction, such a book as this will bristle with debatable points, since it brings the reader within reach, as it were, of the " growing point " of the science. With a view to future editions it may be useful to call attention to some obscurities which have caught the reviewer's eye. On p. 36 it is stated that " A fact is not an event. It does not occur *at* a time, although some facts are facts with regard to a particular time." But surely the fact which makes the proposition expressed by " Charles I was defeated at the battle of Naseby " a true proposition and not a false one (to use the author's own example) was and is an event, and one which extended during a particular period of time. The fact which was the actual defeat of Charles I was a time-extended event with vague spatio-temporal limits. If, in the history of the world, there has been no such occurrence, or if what happened at Naseby was a victory for Charles I and not a defeat, then the above proposition is false.

On p. 53, the logical distinction between particular and universal, on the one hand, is contrasted with the ontological distinction between substance and universal, on the other, and it is said (although the author does not hold this view) that " a substance . . . is regarded as that which persists through a period of time, whereas a particular may be momentary." What is meant by saying that a particular may be momentary? Does this mean not extended in time at all? If not, is there any fundamental distinction between persisting for a " long " time and only persisting for a " short " time? From the logical point of view (as the author explains later) the important distinction seems to be that between what is represented by " x " and " y " in such propositional functions as ϕx and xRy, and what is represented by ϕ and R. On p. 176 the reviewer was startled to find the statement that " Mathematical propositions are verified inductively in so far as physics, deduced by means of these propositions, is true." This assertion seems to be so far out of harmony with the whole doctrine of Part I that what it says cannot be what is meant. Surely mathematical propositions cannot possibly be " verified inductively " for the simple reason that they say nothing about the world at all. As Wittgenstein says: " We use mathematical propositions *only* in order to infer from propositions which do not belong to mathematics to others which equally do not belong to mathematics." It is the *physical* proposition that is verified, and

this might still happen even if the proposition were reached by faulty deductions. Whereas to say that a mathematical proposition is " verified " merely means that it follows from certain premises. The fact that, by giving " physical meaning " to the variables in a mathematical proposition we obtain a significant proposition which says something about the physical world which is true does not constitute a verification of the *mathematical* proposition.

On p. 302 we read that " A scientific theory that is incapable of experimental testing is valueless." But in discussing the historical sciences on p. 376, the author says : " History is the record of what *has happened*. The primary aim of the historian is to ascertain what exactly did happen. . . . This essential reference to time is the distinguishing characteristic of historical knowledge ; those sciences are *historical* which cannot omit the time-direction. Consequently, astronomy, geology, biology are to some extent historical sciences ; wherever the concept of evolution is applicable there is an historical element. The distinction between the exact and the inexact sciences corresponds to the distinction between those sciences which can and those which cannot omit the time-direction."

Now, all historical theories are, from the nature of the case, incapable of *experimental* testing. Therefore, according to the above view, *either* (1) such theories are not scientific, *or* (2) they are valueless. The important point seems to be the distinction between historical theories based on genuine *historical* data (*e.g.* documents and fossils, which have survived from the historical period in question) and inductive causal theories which are the outcome of experimental investigation of processes now going on. These two types of theories are fundamentally different, and each has its own canons of " evidence " and " verification." But they have often been confused, and pseudo-historical theories have been invented by merely extending inductive generalisations " backwards " in time to any desired " historical " period. When the period in question is one in regard to which we have, and can have, *no* genuine *historical* evidence, such theories cannot in *any* sense be verified. It is such bastard theories which are *both* unscientific *and* valueless. It is not any " essential reference to time " as such, or an inability to " omit the time-direction " which distinguishes the historical sciences, but the fact that they undertake to deal with *particular* occurrences *in their particularity* or " Einmaligkeit." And it is precisely because the inductive theories do *not* do this that they cannot give us genuine historical information. That is why the doctrine of natural selection cannot tell us how the giraffe got his long neck. Moreover, inductive knowledge is,

from the nature of the case, incomplete, and, in consequence, is always liable to be upset by "surprises" resulting from further investigation. This is well illustrated by the way in which speculations regarding the age of the earth have changed from one generation to another. We can never know (when genuine historical data are not available) how far back in time we are justified in extending an inductive generalisation.

Also, it does not seem to be desirable to equate this distinction between historical and inductive (or experimental) science with that between "exact" and "inexact" science. For the latter distinction is one which can be drawn *within* the inductive sciences. The expression "exact science" is used in a number of different senses. In one sense only pure mathematics and logic are exact sciences, when exactness refers to exactness of inference. But when the empirical sciences only are in question it is accuracy of observation that is meant. We then have the greatest exactness where measurement of spatial magnitudes is possible, because these happen to be the things which can most accurately be *compared*, and thus underly all accurate experiment. But the really important distinction seems to be between those empirical sciences, on the one hand, which have succeeded in discerning what exact order systems are exemplified (approximately) by their data, and so are able to *use* the abstract investigations of such order-systems which are furnished by pure mathematics, and those empirical sciences, on the other hand, which have not yet taken this step, but still work with unanalysed ideas and the language of everyday life with all its defects from the scientific point of view. And it is just those sciences which belong to the latter class which are most likely to find help in the new logic. This point also touches the author's discussion of Mr. Russell's remarks on the use of the notion of "cause" in natural science. It *is* true to say that the mere collection and cataloguing of "causes" belongs to the early "classificatory" stages of a science. This procedure bears somewhat the same relation to "processes" that classification according to "characters" bears to "things." This is superseded later, for, as the author says on p. 493: "The scientist is concerned with the discovery of types of order by means of which conclusions can be drawn," and "an empirical science as it becomes highly developed tends to become deductive in form, in spite of the demand that it must fit the facts." But there is no need for an "in spite of" so long as the form of the deductive system is also the form of the physical realm under investigation.

It would be desirable to enlarge the index in subsequent editions. A number of important topics which are fully

discussed in the text are not mentioned in the index, *e.g.* " logical form " and " existence theorem."

This book is the only one of its kind, namely, a *modern* introduction to logic, in the English language. It deserves the earnest attention of all men of science who are interested in the foundations of their work and do not restrict themselves to the rule-of-thumb use of the procedure which has been bequeathed to us by tradition. It is particularly welcome at the present time, when the progress of investigation has awakened a renewed interest in the foundations of natural science, and when some eminent men of science are indulging in wild speculations of a pseudo-philosophical character. For modern logic not only provides us with a means of stating what we have to say in clear and precise language, but also enables us to discover more easily whether we are talking nonsense. Anyone who has worked through this book will be able to read Mr. Russell's *Introduction to Mathematical Philosophy* with profit, and from thence to pass on to *Principia Mathematica.*

REVIEWS

MATHEMATICS

Algebraic Equations; An Introduction to the Theories of Lagrange and Galois. By EDGAR DEHN, Ph.D. [Pp. xi + 208.] (New York: Columbia University Press. Price 21*s*. net.)

THIS is a textbook of the more advanced type. It begins with a discussion of polynomials, progresses steadily through the usual elements of the theory of groups to the theory of Galois, and closes with a short discussion of special equations such as cyclic, cyclotomic, and metacyclic equations. An interesting detail is the table which makes a careful comparison between the methods due to Lagrange and Galois respectively.

On the whole, the book is well written, and its subject-matter carefully presented. Bold-face type is systematically used for important statements and results, an excellent feature in a didactic work of this kind. The author has spared himself no pains in the task of making his subject both clear and attractive to the student, while the index at the end has been carefully compiled.

There are occasional touches which seem strange to the English reader. The friendly polynomial is here an integral function or, on occasion, an entire function, while " combination of permutations " is a phrase as unfamiliar as its meaning, when explained, is commonplace and straightforward. On one or two occasions the author's admiration and enthusiasm for his subject somewhat outrun academic discretion and frigid correctness, and then his style is inclined to be bombastic. But such enthusiasm, though it sometimes mar the style, is a good guarantee of the matter.

The binding and printing of the work are excellent, and the numerous subscript characters are all easily read. If we might be allowed one criticism on a purely typographical point, we should like to suggest that the product symbol, which so heavily patterns page 149, is much too thick and is printed a shade too high on the line. The practice, among mathematical printers, of using big, heavy-type product and summation signs is a common one, but this fact merely strengthens one's objection to the resulting disfigurement of the page.

W. L. F.

Studies in the Theory of Numbers. By LEONARD EUGENE DICKSON, Professor of Mathematics in the University of Chicago. [Pp. x + 230.] (Chicago: The University of Chicago Press, 1930. Price 18*s*. net.)

THE book deals with quadratic forms, and chiefly with problems concerning universal forms, zero forms, and the minima of quadratic forms. A form $f(x, y, \ldots)$ is called universal if, for every integer n, the equation $f=n$ has integral solutions. The least number L (if it exists), such that the equation $|f|=L$ has integral solutions not all zero, is called the minimum of f. A form whose minimum is zero is called a zero form.

Among numerous other new results, the book contains the remarkable theorems that every universal ternary quadratic form is a zero form and that every indefinite quadratic form in five or more variables is a zero form.

The book is fairly independent of the older literature on the subject, in which the author points out several serious errors.

The last chapter (which, by the way, does not assume the results of the preceding ones) deals with the number of representations of a given number as the sum of 5, 6, 7, or 8 squares. It gives a very interesting exposition of a method mainly due to Hardy and Mordell, based on the theory of modular functions. The reader, however, need not be familiar with the latter theory.

The book may be recommended to all those interested in the theory of numbers.

T. Estermann.

Contributions to the History of Determinants, 1900-1920. By Sir Thomas Muir, D.Sc., LL.D., F.R.S., C.M.G. [Pp. xxiv + 408.] (London and Glasgow: Blackie & Son, 1930. Price 30*s*. net.)

What can one say of this fresh evidence of Sir Thomas Muir's scholarship and industry which has not already been said of one or other of the four volumes which cover the history of determinants up to 1900? It is still Muir and, as far as an ordinary person may presume himself able to judge, it is still Muir at his best. The style is the same, admirably suited to its purpose; the liveliness of treatment, the clearness of statement, the close linking up of allied papers are all exemplified afresh. And so too is his power of condemning outright with a minimum of words—" an attempt to give an account of the elementary theory in three pages," " the principle of selection is not apparent." But above all the reader still knows, as soon as he has glanced at half a dozen pages, that his historian knows the material backwards and forwards; time and time again one is astonished at Sir Thomas Muir's mastery of detail. We can conceive of other writers who would be able to lay their fingers on every one of the ten rediscoveries of a theorem first stated in 1851, but who, other than Muir, could give chapter and verse in an old textbook (1877) for an odd example from Mathematical Questions and Solutions for 1916. And who could claim for himself one hundred references in a " list of authors " which dealt with twenty years of work on one subject?

The present volume follows the plan of the well-known four-volume Theory of Determinants. Of the author's recent writings dealing with the later developments of special types of determinants, only those on " compounds " and " alternants " have been incorporated in the book. It is to be hoped that the other papers will form part of some book and not be left in the comparatively inaccessible forms (transactions and proceedings of the Royal Societies of South Africa and Edinburgh) in which they were first published.

There is one departure, and that a notable one. The book closes with an index which crowns the work of the five volumes. It is called the " Subject Index to Writings on the History of Determinants," and occupies thirty-five (35) pages. With this before one it is possible t› look up in each of the five volumes everything about textbooks, determinants with trigonometric elements, anything you will. This last great service to those interested in the subject is a fitting conclusion to a labour which has been applauded throughout the world. We can only echo the French critic of a former volume: " Comme travail de référence on n'a jamais rien fait de mieux que cette œuvre de Muir."

W. L. F.

Applied Mathematics for Engineers. By T. Hodgson, B.Sc. Vol. I, Graphical Statics; Vol. II, Dynamics, with an Introduction to the Differential and Integral Calculus. [Pp. Vol. I, vii + 183; Vol. II, vi + 293.] (London: Chapman & Hall, 1930. Prices 9*s*. 6*d*. and 13*s*. 6*d*. respectively.)

There is a great deal to be said for the writing of textbooks on a subject with a special class of student in mind. The necessary but very arbitrary

division of any science into labelled branches has disadvantages for the student in his early years. He tends to regard these as watertight compartments, to overlook all the correlations, and in practice to assign any problem to a particular compartment before he is able to attack it. With more experience the correct perspective is usually gained, but it is a particular advantage of these volumes by the same author that the student may gain the wider outlook more easily. There are at least two other advantages of a work such as this. A wise selection of the material in the different branches can be made. The student who is presented with half a dozen textbooks on the special branches is not usually in a position to do this unaided. Further, the author is in a position when writing for a particular class of student to direct the mathematical technique to problems which the student will constantly have to face in later practice.

The author of these two volumes has done his task well. A third volume shortly to be published will deal with differential equations and their applications. This set should prove very welcome to all students of engineering who want to get a thorough grasp of the mathematical bases of their subject. The volumes contain all the essential material—which is very well arranged—very clearly interpreted, and attractively produced by the publishers. There are plenty of examples for the student. The whole production can be thoroughly recommended.

R. C. J.

Barlow's Tables. Edited by L. J. Comrie, M.A., Ph.D., H.M. Nautical Almanac Office. Third Edition. [Pp. xii + 208.] (London: E. and F. Spon. Price 7s. 6*d*. net.)

Barlow's tables of squares, cubes, square roots, and cube roots were first published in 1814. The revision of the second edition, which has been in use for ninety years, was completed by A. de Morgan in December 1839. Faced by an ever-increasing demand, the publishers decided to replace the old stereo plates by modern type, and the volume now under review is the result. The new tables comprise (*a*) n, n^2, n^3, n^4, $n^{-\frac{1}{2}}$, $n\,!$, $n^{\frac{1}{2}}$, $n^{\frac{1}{3}}$ and n^{-1} for values of n between 1 and 100; (*b*) all these with the exception of $n\,!$ for values of n between 101 and 1,000; (*c*) n^2, n^3, $n_{\frac{1}{2}}$, $(10n)^{\frac{1}{2}}$, $n^{\frac{1}{3}}$, n^{-1} for values of n between 1,000 and 10,000. There is also an additional table giving the values of the powers up to the tenth of the numbers 1 to 100 and the eleventh to twentieth powers of the numbers 1 to 10. The square roots and cube roots are given to eight significant figures with interlinear differences; the square roots of $10n$ to nine figures.

The column in the tables giving $\sqrt{10n}$ for values of n between 1,000 and 100,000 has been added by Dr. Comrie, and is a very great convenience, since, when using the tables to find, *e.g.* $\sqrt{\cdot 02395}$, inspection of adjacent columns will show whether $\sqrt{2395}$ or $\sqrt{23950}$ is to be employed. The print is necessarily smaller than that employed in Milne-Thomson's *Standard Tables of Square Roots* published last year, and the pages vary somewhat in blackness (compare, for example, pp. 48 and 50), but the figures are quite clear and unlikely to give trouble to tired eyes.

D. O. W.

PHYSICS

Band Spectra and Molecular Structure. By R. de L. Kronig. [Pp. x + 163.] (Cambridge: at the University Press, 1930. Price 10s. 6*d*. net.)

This book is based on a course of lectures delivered at Cambridge by Prof. Kronig in May 1929. These have been expanded and arranged in the present form. Prof. Kronig has himself made many valuable contributions to band theory, and this compilation of the theoretical side of the subject should be welcomed by all workers in the field. The approach adopted is exclusively

that of the new quantum theory—or more precisely of wave mechanics—and the reader is assumed to have some knowledge of the appropriate mathematical technique. Electronic, vibrational, and rotational energy levels are dealt with in this way. More detailed problems, such as fine structure and the rotational distortion of spin multiplets, require further knowledge of the perturbation methods of Born, Heisenberg, and Jordan. Other chapters deal with selection rules and intensities in diatomic molecules, also with the problems of molecule formation and chemical binding. A valuable collection of facts and theories relating to the properties of diatomic molecules in bulk is included. Those properties discussed are light scattering and dispersion, the Kerr and Faraday effects, dielectric constants, magnetic susceptibilities, and specific heats. A valuable bibliography of over three hundred and fifty papers is included in the book. These papers are classified, and this feature should be of very considerable reference value.

Prof. Kronig writes on this subject with a very wide knowledge of its development and present position. We consider that his book will be welcomed by mathematical physicists especially, and by all those who wish to keep in touch with the applications of the new quantum theory in the many fields of physical enquiry.

R. C. J.

Modern Physics : A General Survey of its Principles. By THEODOR WULF, S.J. Translated from the German by C. J. SMITH, Ph.D., M.Sc., A.R.C.S. [Pp. xi + 469, with 202 diagrams.] (London : Methuen & Co., Ltd., 1930. Price 35*s*. net.)

WHEN we are confronted with a scientific book translated from a foreign tongue, it is natural to enquire whether there is any real justification for the expenditure of time and money involved. Consequently, our first enquiry is whether its scope is similar to that of any book already published in the English language. In the case of the volume before us, there is no doubt that it is a book of distinction, and that it rightly occupies an important place in the literature of physics. At present, we are thoroughly familiar with two main types of books on elementary physics, which it is as well to bear in mind in an examination of this volume. We have, on the one hand, our sound intermediate textbooks of physics, from which a student may acquire an adequate knowledge of the technical or professional elements of the subject. On the other hand, we have those textbooks, mainly issued by American publishers, which appear designed for a democracy in which every individual is supposed to receive instruction, irrespective of his abilities to appreciate the facts of physics, and for whom the textbook must necessarily be of a lower standard, or at any rate, more akin to a narrative. Thus we have our matter-of-fact guides to knowledge on the one hand, and our easy steps to culture on the other.

The question we must now ask ourselves is whether there exists a book which gives a co-ordinated account of the fundamental results of physics, which satisfies the philosophic cravings of our more mature students, which gives some idea of the historical development of our science, and which gives an appreciation of the position of modern physics? Or, again, is there a book which will enable an intelligent reader, who has learned something of physics in the days of his youth, to gain an adequate conception of the progress achieved in our understanding of the world around us? We may answer that the book before us satisfies these needs.

The book is divided into four parts. In the first part, the fundamental concepts of mechanics and of wave motion are discussed. In the second, our knowledge of the atomic structure of matter is reviewed, and we find excellent discussions of the properties of the various states of matter, of the laws of thermodynamics, and the evidence for the existence of atoms. The tables

of data given in this section are very useful. It is noted, however, that a statement concerning the susceptibility of paramagnetic bodies at low temperatures needs serious revision. It is unlikely, too, that all physicists will be satisfied with the statement that, on the whole, the subject of surface tension has contributed very little towards our understanding of the molecular structure of the material world. In the third part, the laws of electricity and magnetism are set forth, together with the phenomena which appertain to the conduction of electricity by gases and to radio-activity, and this section fittingly closes with a discussion of the internal structure of the atom. The last part is entitled the Physics of the Ether. It is devoted to the consideration of the propagation of light and physical optics, and rightly includes a simple account of the theory of relativity. The final chapter, on the aims of modern physics, gives an outline of the trend of recent developments in wave mechanics.

The translator has done his work, as far as one can judge without reference to the original German text, very well indeed, and the English version is very pleasant to read. We recommend the book very strongly to all teachers of physics who are interested in introductory courses to advanced physics, and to those who wish to keep abreast of the main current developments. We feel, however, that we must also make a recommendation to the publishers. We see no reason for the high price of this book, and would suggest that a reduction by at least thirty per cent. be considered. It is said that a certain piece of physical apparatus of foreign manufacture was priced here at some sixteen to twenty times its actual cost of production, merely because it was considered that the British public would pay. It is to be hoped that the British public will never experience the extension of a similar principle to the sale of books.

L. F. B.

Physical Principles of Electricity and Magnetism. By R. W. Pohl, Professor of Physics in the University of Göttingen. Authorised Translation by Winifred M. Deans. [Pp. xii + 356, with 393 illustrations and 60 examples.] (London and Glasgow: Blackie & Son, Ltd., 1930. Price 17*s.* 6*d.* net.)

Professor Pohl regards the usual presentation of the phenomena of electricity and magnetism in their historical order of development as obsolete. This is, of course, a matter of opinion, but one usually finds it desirable, merely from considerations of laboratory convenience, to start the presentation with a discussion of the properties of magnets, and then to pass on to a discussion of electrical measuring instruments, postponing the treatment of electrostatics for some time later in the course. Professor Pohl, however, does not trouble to discuss in detail the behaviour of the small magnet, but he plunges as quickly as possible into a description of laboratory instruments. In fact, the magnetometer, with its attendant errors, is practically banished from the book. It is true that its place is taken by a combination of a solenoid and a magnet controlled by a spring, but those fundamental matters which are so helpful in obtaining an idea of the meaning of absolute measurements are not to be found in this book. However, we must be grateful that the author has included an excellent description of the magnetic potentiometer, an arrangement which, although described in the *Philosophical Magazine* many years ago, is still very little known to the teaching profession.

The most important feature of the book is its wealth of illustrations. Many of them are highly original, and all teachers who are interested in the efficient demonstration of lecture experiments would do well to refer to this book. The average English student will appreciate the sound practical basis of the treatment, but he will be a little mystified when he encounters some of the units employed. He is not accustomed to regard air as possessing

an absolute dielectric constant of $8{\cdot}84 \times 10^{-14}$ coulombs per volt-centimetre, or to regard the magnetic susceptibility of a vacuum as $1{\cdot}256 \times 10^{-8}$ volt-seconds per ampere-centimetre, and the reviewer devoutly hopes that he never will be so accustomed. These idiosyncrasies of pedagogical exactitude are not likely to trouble the reader unduly, and if they force him to remember that K and μ have dimensions, presumably they have done all that is required. The author is at his best when he is dealing with the connection between electric and magnetic fields, and his explanations of Maxwell's equations are very masterly. His treatment of electric waves is delightful in its simplicity, and on the mechanism of electric conduction he gives a particularly good account of the present state of our knowledge.

The book is very pleasant to read. There are a few phrases which the translator should have avoided; for example, the reviewer has never yet heard an English student talk of "the fling of a galvanometer." The printing and general production are most excellent, and the book is heartily commended to all who are concerned with serious instruction in physics.

L. F. B.

The Principles of Quantum Mechanics. By P. A. M. DIRAC. [Pp. x + 257.] (Oxford: at the Clarendon Press, 1930. Price 17*s*. 6*d*. net.)

THE lull which has occurred of late in the rapid progress of theoretical physics has made it possible to take stock of and sort out the most important developments, and it is not surprising therefore that, during the last year, several excellent books on the subject have appeared. These for the most part stress one or other of the two main lines of theoretical development, namely, the matrix methods of analysis on the one hand, and the wave mechanics on the other. Both of these lay particular stress on co-ordinate representations of the quantities involved; on the dynamical variables in the case of the matrix mechanics, and on the states of the system in the case of the wave equation treatment. Now, in Dirac's account of the new principles, the most logical account of all perhaps which has so far crystallised out of a rather cloudy mixture of assumptions, postulates and conclusions, it is rather on the fundamental physical concepts themselves, together with the mathematical operations governing them, that principal stress is laid, just as in vector analysis certain quantities are dealt with directly with the help of combinations of certain fundamental operations, instead of by means of their co-ordinate representations. The method employed in the book, like that of Weyl's "Gruppentheorie und Quantenmechanik," is a symbolic one which, starting out from certain fundamental concepts as axioms, seeks only to develop a self-consistent mathematical scheme which will suffice for the prediction of the result of any physical experiment—the ultimate test of success in an account of the working of Nature on a microscopic scale, but which is neither intended nor expected to reveal a background of models based on ideas arising from our knowledge of the macroscopic phenomena of matter. This is undeniably the soundest point of view to adopt, and so it is eminently satisfactory to read a book in which the terms "particle" and "wave" do not continually recur to remind us of things which, for the time being at any rate, are better forgotten, for, it must be remembered, one of the most important points of philosophical interest which has arisen out of the new theory, chiefly as a result of the work of Bohr and Heisenberg, is the recognition of the incongruity of the principle of causality and space-time descriptions of atomic phenomena. A remarkably clear and connected account has been provided and illustrated by many of the most important applications of the new methods of attack on physical problems; difficulties of course remain, particularly in connection with the relativistic aspect of the theory, and gaps remain to be filled with regard to mathematical existence theorems, etc., so that theoretical physicists are not yet threatened

with even temporary unemployment. As Dirac is careful to point out, only long familiarity with the properties and uses of the new axiomatic concepts and operations can provide them with a permanent home in our mental establishment, just as the idea of energy in mechanics has by now ingrained itself in our imagination. This book which, in its systematic exposition, resembles rather a textbook of some long-established subject, should undoubtedly be read and digested as much as possible by all those seriously interested in theoretical physics. J. W. FISHER.

Quantum Mechanics. By E. U. CONDON, Ph.D., and P. M. MORSE, Ph.D. [Pp. ix + 250.] (London: McGraw-Hill Publishing Co., Ltd., 1929. Price 15*s*. net.)

Atoms, Molecules and Quanta. By A. E. RUARK, Ph.D., and H. C. UREY, Ph.D. [Pp. xvii + 790.] (London: McGraw-Hill Publishing Co., Ltd., 1930. Price 35*s*. net.)

THESE two books are the first of a new international series in physics. Such a series is certain of a warm welcome, particularly if subsequent books maintain the standard set by these under review. It is, however, to be hoped that an attempt will be made in later volumes to co-ordinate more effectively the various contributions to the series, as it would considerably add to their value and incidentally reduce their cost if duplication could be avoided. Thus the second of these volumes, while concerned mainly with advances on the experimental side, includes several chapters on wave and matrix mechanics, and deals with these subjects so extensively (nearly 200 pages) as almost to render unnecessary the book by Condon and Morse.

Quantum Mechanics may be regarded as a good practical guide to the present form of the quantum theory, and will be found to be of great value to any young research student of physics who desires a working knowledge of the subject. It not only sketches some of the main features of the theory, but works out in considerable detail many examples of an important character. Numerous features of the book indicate that the authors combine the art of the teacher with the knowledge of the specialist. The main argument is illumined by many an apt illustration and enlivened by interesting exercises designed to give stimulus to the reader.

After introducing the wave equation in an easy way and formulating working rules for its derivation in general cases, detailed solutions are given of a number of standard problems, such as the linear oscillator and the hydrogen atom. The intensity of radiation emitted by such quantised systems is worked out and detailed selection rules deduced. All this is done so skilfully that the theory occupies not more than three short chapters, and can be read without any previous knowledge of the subject.

Further chapters deal in a most useful way with approximate methods of solving the wave equation and include an account of perturbation theory. These are used to elucidate the structure and vibrations of diatomic molecules, a field of work to which the authors have themselves made important contributions.

Atoms, Molecules and Quanta falls naturally into three broad divisions. The first of these deals with the older quantum theory, gives a sketch of Hamiltonian mechanics, describes in detail the theory of optical and X-ray spectra on the basis of orbits, and follows the usual historical and stereotyped methods.

The second provides a long and detailed account of atomic and molecular spectra and bears the mark of the expert in his own field. It gives an excellent summary of recent work on infra-red spectra and electronic band systems. Actual examples are worked out, and the reader is shown clearly and concisely how the experimentalist has brought order out of chaos in complicated band systems. It includes a further excellent chapter on other

topics of spectroscopy such as the polarisation of resonance radiation and the Raman effect. In these rich experimental fields the authors write in their happiest vein.

The third division gives an account of wave and matrix mechanics, and covers very much the same ground as the book by Condon and Morse, though greater attention is given to matrix mechanics and its relation to wave mechanics.

The book is intended for those who are approaching the subject of atomic and molecular structure for the first time, and also for research workers in need of an up-to-date account of the laws of quantum theory and the most recent experimental results. There can be no doubt that the second class is well catered for. The book is a veritable mine of information, and is likely to prove a boon companion to those who pursue the cause of physics either in the study or the laboratory. The reviewer does not consider it so successful for the beginner. The book is too comprehensive and not sufficiently selective. Is it necessary for all successive generations to follow the devious ways of their predecessors and to wander for forty years in the wilderness before reaching the Promised Land ? Some parts of the theory, though of value historically, might well now be omitted from such a textbook as this. However, those beginners who read the book under expert guidance, will be brought to the very boundaries of knowledge in many branches of physics, and those familiar with its contents will be prepared for further advances into the unknown.

J. E. LENNARD-JONES.

A Treatise on Light. By R. A. HOUSTOUN, M.A., Ph.D., D.Sc., Lecturer in Natural Philosophy in the University of Glasgow. Sixth Edition. [Pp. xi + 494, with 335 figures in the text and 3 plates.] (London: Longmans, Green & Co., 1930. Price 12*s*. 6*d*. net.)

HOUSTOUN'S *Light*, first published in 1915, still remains the only textbook published in the United Kingdom which attempts to cover the whole of the subject from the pin-hole camera to relativity and the Raman effect; a fact which is in itself a testimony to the excellence of Dr. Houstoun's work. It might be possible to write and publish a better book for the very reasonable price at which this is issued, but it is a possibility which requires experimental proof, and it is not a very hopeful experiment.

The work is so well known that any detailed description would be superfluous. The new edition contains six more pages than that published in 1924, but, by excising old matter and diagrams, there are in fact twenty pages of new material and twenty-three new diagrams. In the next impression Dr. Houstoun might perhaps find room for a reference to the reflection echelon which has now proved its worth, and to the remarkable results obtained by Siegbahn in the short-wave region by using a concave grating at grazing incidence in the manner so long advocated by A. W. Porter.

D. O. W.

CHEMISTRY

Stereochemie. By GEORG WITTIG. [Pp. xi + 388, with 127 figures.] (Leipzig: Akademische Verlagsgesellschaft M.B.H., 1930. Price M. 25.)

THERE have not been many books devoted solely to stereochemistry; Werner's *Lehrbuch der Stereochemie*, published in 1904, and the second edition of Stewart's *Stereochemistry*, published in 1919, were almost identical in outlook, since in the fifteen years between the publication of these books little real progress had been made in the subject. The last ten years, however, have seen a remarkable change, and in but few branches of organic chemistry have such notable advances been made. As regards the general arrangement

of atoms in space, X-ray analysis, though not so productive of results in organic chemistry as was at first anticipated, has been an important factor, and has done something to revive interest in the neglected study of crystallography. The important advances in our knowledge of optical isomerism can be traced to a tardy realisation that molecular asymmetry is the conditioning factor for the existence of this type of isomerism. Although this was emphasised by le Bel, the unfortunate expression " asymmetric carbon atom " of van't Hoff seems to have prevented the appreciation of the wider possibilities until recently.

Geometrical isomerides have also been more thoroughly investigated of late, but here the results, especially as regards the determination of configuration, have not been so conclusive. The most interesting discovery in this direction is undoubtedly the stereoisomerism of the decahydronaphthalenes due to the non-planar configuration of the rings.

The author accordingly has a large quantity of new work to summarise, and he has dealt faithfully with the material, producing a volume which is a valuable survey of the present position of stereochemistry. In addition to the treatment of the stereochemistry of carbon and nitrogen, chapters are devoted to that of other elements, to stereochemistry and crystal structure, and to the stereochemical influences at work in the kinetics of chemical reaction.

One would have preferred the author to have approached the subject of optical isomerism from the general aspect of molecular asymmetry rather than dealing first with the particular instance of a carbon atom with four dissimilar groups attached thereto, and later with optical isomerides which do not readily conform to this type. The author references are not satisfactory; frequently in the case of joint work only one author is mentioned, and never more than one initial is given, why the second is chosen is difficult to understand, O. Forster and F. Thorpe appear strange to English eyes.

In the section dealing with the isomerism of the diphenyl compounds far too little credit is given to Turner and his collaborators. The method of determination of the configuration of aldoximes is not satisfactorily described; the reaction employed is not a dehydration but the elimination of, for example, acetic acid from an acetyl derivative under the influence of aqueous alkali, the misapprehension that β-aldoximes lose water more readily than α-aldoximes dies hard. Finally, the author seems to dismiss the difficulties in the determination of the configuration of geometrical isomerides rather too lightly.

These matters, however, are of minor importance, and those interested in the subject should be grateful for a valuable and up-to-date collection of the more important results in this interesting field.

O. L. B.

Artificial Organic Pigments and their Applications. By Dr. C. A. Curtis. Translated from the German by Ernest Fyleman, B.Sc., Ph.D., F.I.C. [Pp. viii + 291.] (London: Sir Isaac Pitman & Sons. Price 21*s*. net.)

One of mankind's early technical achievements was the discovery of how to convert certain colouring matters of vegetable origin into insoluble substances which could be used as pigments. On account of their exceptional fastness and satisfactory shades, some of the old colour lakes derived from vegetable dyes are still in demand to-day. Dr. Curtis cites Persian berry and madder lakes. But to a large extent they have been replaced by pigments derived from coal-tar dyes; indeed, these have begun to challenge the position held by the mineral pigments, especially as concerns yellow and red.

The manufacture of artificial organic pigments constitutes one of the most important developments of the coal-tar dye industry—one concerning which

the available information seems hardly commensurate with its vastness and moment. If the chemistry of the coal-tar dyes themselves is complex, that of the coal-tar pigments is at least doubly so; and although the best conditions for pigment formation are, in general, well known to manufacturers, the reactions which take place and the laws governing pigment-formation are very imperfectly understood.

So far, we have nothing comparable to *The Colour Index*, which tabulates the coal-tar dyes and inorganic pigments so precisely, dealing with the artificial organic pigments. But, at any rate, Dr. Fyleman's excellent translation of Dr. Curtis's informative work is very welcome, especially in view of the large amount of practical information which it contains.

The artificial organic pigments may be conveniently divided into two main groups, lake colours and pigment dyestuffs. "Pigment dyestuffs are insoluble organic colouring matters, whilst lake pigments are obtained by the treatment of soluble organic dyestuffs with salts, acids, soaps, and so forth. They are thus precipitated on to so-called mineral bases, carriers, or substrates, which may be compared to the textile fibre in dyeing processes. Some substrates show chemical affinity to dyestuffs, which therefore unite with them spontaneously, but in most cases it is necessary to form a salt or complex salt which envelops the particles of the substrate."

The precipitants used for the manufacture of lake colours are of a most diverse character, depending on the character of the dyestuff employed. The acid dyestuffs are mostly precipitated with barium chloride, but alum and other precipitants are also employed. Basic dyestuffs interact with green earth and white earth, which also act as bases, with tannin and tartar emetic and with other materials. The bases employed are equally diverse, blanc fixe (precipitated barium sulphate), aluminium hydroxide, and green earth being amongst the most important.

In the case of green and white earths, their action on basic dyestuffs was formerly considered to be a process of absorption only. Dr. Curtis points out that more recent investigations indicate the formation of complex salts.

The applications of artificial organic pigments are both numerous and important. Those dealt with by Dr. Curtis include the manufacture of decorative colours (lime-washes, water-paints, cement-colours, water-glass colours), oil-paints and enamels, printing inks, sealing-wax colours, crayon and pastel colours, artists' colours, linoleum, and lincrusta.

Much interesting and practical information is given in the book concerning the manufacture of various decorative paints and varnishes, numerous formulæ being included. There is also an extremely interesting account, well calculated to appeal to the general reader, of the various processes adopted for printing in colours. Artists' colours, perhaps, are dealt with rather too briefly, so that the reader is liable to get the impression that fewer pigments are used by artists than is actually the case. The employment of artificial organic pigments in the cosmetic industry is passed over in silence. This is a pity, as the subject, about which much secrecy seems to be observed, is one of interest and moment. For cosmetic use, it is extremely desirable that only those lakes should be employed which are prepared from dyestuffs innocuous in themselves and having the same degree of purity demanded of foodstuff colours, by means of precipitants and bases containing no poisonous metals, such as barium or lead.

In addition to the chapters dealing with the manufacture and application of artificial organic pigments, Dr. Curtis's book contains a big collection of abstracts from recent patent literature, as well as a chapter on "Theoretical Considerations," and some few details concerning the analysis of artificial organic pigments. There is also a valuable Appendix tabulating important details concerning the dyestuffs used in the pigment colour industry, which has been enlarged by quotation of the products of certain leading British firms.

In short, the book constitutes an extremely interesting and valuable account of an industry of leading importance, to which we are largely indebted for making our cities and our homes more beautiful and satisfying that craving for colour which, perhaps, must be included amongst the instincts of mankind.

H. S. REDGROVE.

The Microbiology of Starch and Sugars. By A. C. THAYSEN and L. D. GALLOWAY. [Pp. viii + 336.] (London: Oxford University Press, 1930. Price 25*s*. net.)

IN setting themselves the task of writing a treatise on the Microbiology of Starch and Sugars the authors can have been under no misapprehension as to the formidable nature of their undertaking and the acknowledgment that they examined in detail more than three thousand original papers speaks for itself. Quite evidently no pains have been spared to make the information given as complete as possible, and those in search of facts concerning the constitution of starch, glycogen or insulin or their hydrolysis, or the behaviour of tetra-tri or disaccharide under the action of hydrolytic enzymes of fungal or bacterial origin, are not likely to consult Part I of this book in vain. The second part deals with fermentation of monoses, and opens with a clear account of Kluyver and Donker's interpretation of fermentations as oxidation-reduction reactions. The views of these latter authors may not be generally accepted as yet, but they do at any rate appear to produce some sort of order out of chaos; their classification has in the main dictated the order in which the subject is dealt with in the present volume. A special chapter is devoted to the comparatively little-studied fermentation of pentoses. Part III deals with synthetic activities of micro-organisms, such as fat production, and the two remaining parts are concerned with the applications to various economic and industrial processes, such as cereals and cereal products, grain and milling products, sizing materials and adhesives, baking, diseases of bread, cane, and beet sugars, etc. In this part of the book will be found much interesting information not easily accessible, and the whole forms a very valuable addition to the literature of carbohydrates in the English language.

P. H.

In the Realm of Carbon. The Story of Organic Chemistry. By Prof. H. G. DEMING. [Pp. x + 365, illustrated.] (London: Chapman & Hall, 1930. Price 15*s*. net.)

THERE can be no two opinions as to the interest of this introduction to organic chemistry and its applications, and all boys of whatever age who are presented with a copy of it will soon realise their good fortune. M. M. Pattison Muir, in his *Story of the Wanderings of Atoms*, made a similar, though shorter, attempt to give an outline of organic chemistry and its applications, and the reviewer recalls spending many happy and enthralled hours reading this history of man's attempts to outdo nature and prepare in laboratory and works the indigo and madder that had hitherto been obtained from the soil, and now we have Prof. Deming's larger and even more interesting volume, which combines most deftly the not too unpalatable powder of organic chemical theory with the jam of its everyday applications.

The book sets out to provide for the general reader who wishes to know something of the manner in which organic chemistry grew and developed, and something of its contributions to the comforts and necessities of modern life; its scope is perhaps best indicated by the titles of the three parts: (1) How the Foundations were Laid; (2) The Organic Chemical Industries; (3) The Chemical Activities of Living Cells.

The first section thus shows shortly but clearly how the modern conceptions of organic chemistry were arrived at step by step, through the work and speculations of Dumas, Laurent, Gerhardt, Kekulé, Pasteur, and van't

Hoff, leading by easy stages to the complexities of camphor, indigo, and the alkaloids.

In Part II a good account of the scope of the organic chemical industries is given, touching upon acetylene and its uses, petroleum, coal and its products, the industries based upon cellulose, and so on. In Chapter XVI, "Giants Astride the Earth," some account is given of the enormous economic and political importance to-day of the great chemical corporations of the world, such as the German I.G., the British I.C.I., Du Pont's in the United States, and so on. ". . . New enterprises have been set afoot in all fields of commerce. New industries have sprung up. Coal tar discards its black mantle and assumes the garments of the rainbow. . . . Coal veins are struck with the staff of chemistry and liquid fuels gush forth. The voices in the market place speak familiarly of catalytic processes, nitrogen fixation, vat dyes and synthetic ammonia. . . . Former enemies join hands across vanishing frontiers. Giants spring from the earth and cast their shadows over continents."

Nor, in outlining what we may term the mechanical side of organic chemistry, does the author neglect the chemistry of the living cell, for in the last few chapters we have a very readable account of the chemical activities of the living organism, of bacteria, of fermentations, cellulose, sugar, and the thousand and one products used in daily life. In fact, Prof. Deming's work would form a very fitting supplement to Dr. Holmyard's *Introduction to Organic Chemistry*. The illustrations are in some cases well meant but unconvincing. The best advice one can give about the book is to recommend people to read *In the Realm of Carbon* for themselves.

F. A. MASON.

The Spirit of Chemistry. An Introduction to Chemistry for Students of the Liberal Arts. By ALEXANDER FINDLAY, Professor of Chemistry, University of Aberdeen. [Pp. xvi + 480, illustrated.] (London: Longmans, Green & Co., 1930. Price 10s. 6d. net.)

IT is explained in the Preface that this volume is designed "so as to make appeal to the imagination and intellectual interests of those who are not destined for a scientific career, but who desire to understand something of the intellectual progress of recent years and to gain some knowledge of a branch of science on which much of our present-day civilisation is based." To this class of readers its author is already well known through his earlier book, *Chemistry in the Service of Man*, on which the present volume is modelled and out of which it has grown. The opening chapters describe the aim and method of science, and give a brief account of the historical development of chemistry and of the discovery and formulation of its fundamental laws. Subsequent chapters discuss atomic weights, the periodic law, radioactivity, atomic structure, the three states of matter, combustion, the gases of the atmosphere, matter and energy, fuels and illuminants, solution, ionisation, electrochemistry, metals, alloys, the colloidal state, organic chemistry, stereochemistry, and so on. The treatment follows historical lines, wherever possible, and the text includes some well-written thumbnail biographies of the founders of chemistry. Moreover, the materials and the chemical phenomena of daily life and a number of industrial chemical processes are discussed in ways that are suited to the class of reader for whom the volume is intended. Prof. Findlay's main purpose, however, is to convey something of the spirit of science, of its intellectual honesty in its attempt to understand the phenomena of nature, and of its cultural value. And in this purpose he has certainly succeeded. His book provides profitable reading not only for the "Students of the Liberal Arts," for whom it was written, but also for students of science, who will find it a useful and interesting supplement to most textbooks of chemistry, since it contains much information ordinarily

absent from these. An appendix gives, chapter by chapter, a somewhat formidable list of books for further reading. Besides twenty-two portraits, the volume contains forty illustrations, and upwards of eighty figures in the text. The choice of figures and illustrations, so important in a work of this kind, is excellent. Evidently, no pains have been spared to make the book attractive.

D. McKie.

First Report of the Committee on Photochemistry. By H. S. Taylor and Others. (Reprint and Circular Series of the National Research Council, No. 81.) [Pp. 482–575.]

The report consists of six articles which were originally published in the *Journal of Physical Chemistry* (April 1928, pp. 481–575). The individual articles "were not submitted to the committee as a whole," and differ exceedingly in their outlook and in the method of treatment. There is enough variety to suit every taste.

The first two articles, by G. S. Forbes and H. G. De Laszlo, deal with experimental technique, and contain a large amount of information in a very condensed form. They will be appreciated by investigators in the photochemical field. The next article, by L. A. Turner, is a concise introduction to the physical aspect of light absorption. It is clearly written, but perhaps a few explicative diagrams would have enhanced its value.

In the article that follows, H. S. Taylor shows how the Einstein quantum relation (which states that one quantum of active light is absorbed per molecule of reacting substance) can be successfully applied to selected examples of simple photochemical reactions, such as the combination of hydrogen with the halogens. To explain a few of the more complex reactions, Taylor introduces his favourite theory of chain reactions, in which it is assumed that the initial act of photochemical absorption is followed by a series of "purely thermal" reactions.

W. D. Bancroft's contribution is the most voluminous. It contains a wealth of examples of photochemical reactions of great variety. Prof. Bancroft has little sympathy for the application of the quantum principles to photochemical processes, but gives nevertheless lengthy quotations from Bodenstein and Frank, and from many other writers (two-thirds of the article is between quotation marks). He prefers to explain photochemical changes in terms of the older view that "photolysis is a molecular electrolysis," and discusses the action of "depolarisers," substances whose presence facilitate photochemical reactions.

The sixth and last article is a short contribution by S. C. Lind, comparing the chemical effect of light with that produced by electrons and ions (obtained from alpha particles) on chemical systems.

This Report brings out forcibly the difference in the attitude adopted by photochemists towards the fundamental laws of their science. The Grotthuss-Draper law, that the only radiation which will produce photochemical reactions is that which is absorbed by the reacting system, receives unanimous approval. Indeed, Bancroft goes farther, and states that "all radiations which are absorbed by a substance tend to eliminate that substance." (The snag in this generalisation lies in the indefinite meaning of the expression "tend to.") The Stark-Einstein law (one molecule reacting per quantum absorbed) continues to form a bone of contention. "Fortunately for my peace of mind, it does not come within the scope of this Report to discuss the general accuracy of the Einstein equation," avers Bancroft. H. S. Taylor qualifies the law by adding, "The photochemical yield is determined by the thermal reactions of the system produced by the light absorption."

One aspect of the Einstein equation which was emphasised at the Faraday Symposium in 1926 is ignored in this Report, namely, its limitation to reversible endothermic reactions (*Trans. Farad. Soc.*, vol. 21, pp. 439, 468,

490, 523). It appears that, like most great generalisations, the laws of photochemistry are *defining* laws. In a less ambitious form, in keeping with the more cautious mood of present-day physics, the Grotthuss *definition* could be expressed thus: "Photochemical reactions are chemical reactions in which radiation is selectively absorbed by the reacting system." If Grotthuss's *law* is taken to mean that radiation cannot act catalytically, it becomes an unwarranted restriction upon nature. There is no *a priori* reason why, in chemical changes where light acts like a trigger which starts an exothermic reaction, the radiation initially absorbed should not be re-emitted during the reaction (induced chemiluminescence). Indeed, this may actually take place in some of the reactions where "purely thermal" chain mechanism is now postulated. Chemical changes in which a single quantum causes the oxidation of 10,000 molecules (benzaldehyde), or even of 50,000 molecules (sodium sulphite) are strangely reminiscent of catalytic action. Why postulate different mechanisms—absorption of a light quantum by the first molecule, and transference of kinetic energy, or electronic activation, in the rest of the reacting molecules? It would seem simpler to explain the oxidation of all the molecules by the successive absorption and re-emission of one quantum. This simple view would conflict, however, with the strict interpretation of the Grotthuss-Draper Law. VICTOR COFMAN.

Diatomaceous Earth. By R. CALVERT. American Chemical Society Monograph Series. [Pp. 251, with 70 figures.] (New York: The Chemical Catalog Co., 1930. Price $5.)

THIS book is an excellent addition to the series of scientific and technical monographs which are published under the auspices of the American Chemical Society. It deals with the nature, origin, properties, and commercial uses of diatomaceous earth, otherwise known as kieselguhr and diatomite. It is composed of the remains of the minute siliceous plants known as the Diatomaceæ, and the pure mineral which is present in the earth is a hydrated amorphous silica in a very fine state of subdivision. This property, and its highly siliceous composition, make diatomaceous earth useful in a number of interesting ways. A well-known but now nearly obsolete use is as an absorbent for nitroglycerine in order to make dynamite. The present major uses are for the filtration of raw cane sugar solution, as an addition to concrete, and as a brick for thermal insulation purposes. Diatomaceous earth is also used for the filtration of fruit juices, varnishes, oils, and many other turbid liquids; as an abrasive in soaps, cleansing powders, and polishes; as an absorbent for numerous liquids; and as a filler for rubber, asphalt, paints, etc. There are many other possible uses which are expounded in this book, and Mr. Calvert prophesies a rosy future for the industry.

In successive chapters the author describes the nature and origin of diatomaceous earth, the present status of the industry, the occurrence of the material, its mining and preparation for the market, its physical properties; and, finally, in the eleven remaining chapters, he describes its extremely varied uses.

The book is well written, well produced, and well printed, and is illustrated by numerous excellent photographs and diagrams. While it is more for the physicist and chemist, the book also provides good up-to-date material for the geologist. G. W. T.

GEOLOGY

Economic Geology. By H. RIES. Sixth Edition, revised. [Pp. v + 860, 75 plates, 291 figures.] (New York: J. Wiley & Sons. London: Chapman & Hall, 1930. Price 30*s*. net.)

THAT a new edition of this standard work has been called for within five years of the publication of the last edition is a tribute to its great popularity

and usefulness. The fifth edition was reviewed in SCIENCE PROGRESS, January 1927, p. 545. Three mistakes and misspellings were therein pointed out, but only one of these has been corrected. "P. Brun" is still used instead of "A. Brun" on p. 444; and "porphyry" is still misspelt six times in Fig. 162 (p. 524). In accordance with the plan adopted in earlier editions, the subject-matter, statistics, and references have been brought up to date without materially increasing the size of the book. Seventeen pages have, however, been added, but plates and figures remain the same as before. The bibliography on oil-shale does not contain R. Potonié's recent general work on that subject, nor does that on fertilisers include Prof. J. W. Gregory's recent account of the geology of phosphates published in the *Transactions of the Geological Society of Glasgow.* No mention of Prof. P. G. H. Boswell's extensive work on foundry-sands is made, or of G. V. Wilson's investigations of the Carboniferous bauxitic clays of Ayrshire, or of Dr. R. H. Rastall's book on the *Geology of Metalliferous Ore Deposits.* In fact, the references show insufficient appreciation of non-American work in all the subjects treated in the book. Notwithstanding these omissions the book is a mine of up-to-date information, and maintains its position as one of the best texts on economic geology.

G. W. T.

Elements of Optical Mineralogy. Part III, Determinative Tables. Second Edition. By N. H. and A. N. WINCHELL. [Pp. xii + 204, 3 plates in pocket.] (New York: John Wiley & Sons. London: Chapman & Hall, Ltd., 1929. Price 22*s.* 6*d.* net.)

IN some recent notices of mineralogical works the reviewer has found occasion to animadvert on the doubtful value of some types of determinative tables. These strictures, however, do not apply to the excellently conceived tables which form the third part of Dr. Winchell's new edition of the *Elements of Optical Mineralogy.* These form an almost indispensable part of the entire work, the first two parts of which were respectively reviewed in SCIENCE PROGRESS, October 1929, p. 354, and April 1929, p. 715.

The tables summarise the data given in Part II of the book, and are complete as far as transparent or translucent minerals are concerned; but only a few of the commoner opaque minerals are included, because the ordinary petrographic methods are not well adapted for their study and determination. There are six sets of tables, the first and shortest, "Opaque Minerals," being based on colours in reflected light. Table II is based primarily on birefringence, and secondarily on refractive index. Further properties arranged in this table are colour and crystal system for isotropic minerals; optical sign, cleavage, extinction angle, 2V, crystal habit and system for the anisotropic minerals. Table III is based primarily on colour in thin section, and secondarily on pleochroism, followed by birefringence and refringence.

Table IV is divided into two parts, the first relating to isotropic minerals which are arranged according to refractive index, the second dealing with anisotropic minerals which are arranged according to the mean refractive index or the index of the ordinary ray. Table V is based on dispersion phenomena, and, while it is still incomplete, all available data are included. In this table a property "D" is tabulated, of which no explanation is given in the introduction to this section, or in the list of abbreviations. It appears to be refractive index. A large coloured plate of the Michel-Lévy Table of Birefringences, a special determinative chart of refringence and birefringence for rock-forming minerals, and Wulf's stereographic plate printed on translucent paper, are to be found in a pocket at the back of the book. A short introduction explains the plan of the tables.

It is difficult to express just appreciation of this most valuable work,

which must have involved extraordinary labour in its compilation. The warmest thanks of all mineralogists and petrographers are due to Dr. A. N. Winchell.

G. W. T.

Simple Geological Structures. A Series of Notes and Map Exercises. By J. I. PLATT, M.Sc., F.G.S., and JOHN CHALLINOR, M.A., F.G.S. [Pp. 56, 10 figures, 9 maps.] (London: T. Murby & Co., 1930. Price 3*s*. 6*d*. net.)

THE object of this book is to provide a series of notes on the elementary principles involved in the reading of simple geological structures and history from a geological map. To this end nine illustrative map exercises of much ingenuity are fully worked out and clearly expounded. The book owes its origin to the proved necessity of providing an explanatory companion to the popular series of exercise maps which have already been published by Mr. Platt. Very full and explicit notes on the bedding plane, the bed, unconformity, faulting, folding, igneous rocks, and superficial deposits, interspersed with appropriate map exercises, are given; and the book concludes with notes on the description of a geological map, and on the drawing of geological sections. The whole scheme seems to be admirable in conception and thorough in execution. The importance of "seeing solid," of envisaging the structures in three dimensions although they are represented in two, is insisted upon throughout. We have no hesitation in recommending this well-thought-out practical manual to students of elementary geology as the best possible introduction to an important part of their work.

G. W. T.

Methods in Geological Surveying. By E. GREENLY, D.Sc., and HOWEL WILLIAMS, M.A., D.Sc. [Pp. xvi + 420, 81 figures, 3 plates.] (London: T. Murby & Co., 1930. Price 17*s*. 6*d*. net.)

THE matter in this book is so good that it is a great pity it is so prolix. After reading it the reviewer has the feeling that its value to the student would have been doubled had its length, and presumably its price, been halved. In many parts of the world the geologist finds good topographic maps ready to his hand; in other parts topographic surveyors exercise their function concurrently with, or a little in advance of, the geologist; and while the geologist has occasionally to make his own topographic map, we do not think that this fact justifies at least one-third of this book being devoted to such topics as the construction of topographic maps, the evolution of general cartography, cartographic mathematics, cartographic terms, and other purely general material. All this is done well, but it is scarcely to the point. When the book really gets down to its subject it is excellent except for a certain garrulity in the writing.

The senior author had the advantage of working for a time with Peach, Horne, and Clough, those great masters of the art of geological mapping, and he has been engaged for many years on the detailed survey of Anglesey. Is it possible that the insularity which has crept into Dr. Greenly's strictures on geometrical methods of surveying, as carried out by "calculator-surveyors," arises from this fact. These methods are admittedly of little or no value in such complicated areas as both authors of this book have mainly had to deal with, but they are extremely useful in broad regions of simple geological structure, such as are to be found in many oil-fields and coal-fields. One of the most valuable things in the book is Dr. Williams's too brief exposition of the methods of Cloos in mapping the structures of large igneous intrusions. We should have liked to see more attention paid to the mapping of igneous rocks in general, in which the junior author has done such brilliant work. Dr. Greenly also has a valuable chapter on the cartographic problems

of the crystalline schists. The book was probably compiled too early for reference to be made to the stratigraphical methods based on current-bedding and graded bedding, which have recently been found to be specially useful in orogenic regions of great structural complexity. The two last chapters, "Suggestions," and "Concluding Reflections," make entertaining reading, although we may doubt their entire relevance to the subject of the book, already overloaded as it is, in our opinion, with irrelevant material.

However, the fine art of geological mapping is faithfully and well expounded in these pages. Students, and eke senior geologists, who wish to excel in this art would do well to study the book carefully, although, perhaps, with some judicious skipping.

G. W. T.

Chapters on the Geology of Scotland. By the late B. N. PEACH, LL.D., F.R.S., and the late J. HORNE, LL.D., F.R.S. [Pp. xvi + 232, 18 plates, 27 figures.] (Oxford University Press. London: Humphrey Milford, 1930. Price 10*s.* 6*d.* net.)

THIS book is but a fragment, but it will prove as enduring a monument to Peach and Horne as the stone cairn erected to their memory last summer at Inchnadamph, on the scene of their greatest geological triumph. It is the only finished part of a projected comprehensive *Geology of Scotland*, upon which Peach and Horne collaborated after their retirement from the service of the Geological Survey. Death, however, overtook them long before the great task could be completed. The preparation of the manuscript for the press has been well and faithfully carried out by the editors, Mr. M. Macgregor, Prof. E. B. Bailey, and Dr. R. Campbell.

The first chapter deals with the physical features of Scotland in relation to geological structure, and for that purpose the country is regarded as consisting of four principal geographical divisions—the North-west Highlands, the Grampian Highlands, the Central Lowlands (which are, however, well south of the centre), and the Southern Uplands. The lochs or fiords are regarded as submerged land valleys excavated in the Highland plateau after the Kainozoic uplift, but largely modified by subsequent ice action. In opposition to Prof. J. W. Gregory the authors believe that, while some of the West Highland fiord valleys run along lines of fracture, dislocations have not been the dominant agents in their formation.

Chapters II, III, IV, and V deal with the geology of Scotland north of the Great Glen. They are respectively entitled: Pre-Cambrian Rocks, Lewisian Gneiss; Pre-Cambrian Rocks (*continued*), Torridon Sandstone; Cambrian System, and Later Intrusions and Earth Movements; and Highland Metamorphic Rocks, Moine Series (Eastern Schists). The material in these chapters is largely that of the survey memoirs covering the region concerned, especially the great North-west Highlands Memoir, but new material, relating to the Outer Hebrides, the inner Western Isles, and the Highland Border rocks, is incorporated. A piquant close to Chapter V is afforded by the setting forth of the reasons for Dr. Peach's well-known hypothesis that the Moine Series is but the metamorphosed representative of the Torridon Sandstone, but the antidote is provided by the cautious and dissenting pen of Dr. Horne.

The two final chapters, on Islay and Colonsay, the north end of Jura, Scarba, Lunga, Eilean Mor, and the Garvellachs, have been reproduced from originals supplied by Dr. Peach to Dr. Horne. Only verbal alterations have been made by the editors in this material, although it is known that Dr. Horne disagreed with certain of the major conclusions of his colleague.

The *pièce de résistance* of the book is the daring diagrammatic section drawn by Dr. Peach from N.N.W. to S.S.E. across the whole width of the Highlands, in which he shows the region as a folded complex of Lewisian,

Torridonian, Cambrian, Ordovician, and Silurian rocks, and effects some startling correlations, which are reinforced in a brilliant long coloured section showing the structure of the Highlands from the Firth of Lorne to the Firth of Clyde. Whatever may be the ultimate fate of these ideas, there is no doubt but that they will have to be taken into account by other workers, as, for one thing, they agree in part with recent Scandinavian views on Highland structure in that country.

The book is finely illustrated by plates showing the scenery and structures of the rocks, and by numerous maps and sections in the text. Its production reflects much credit on the editors, and the book is likewise a fine example of the printer's art. Peach and Horne live again in its pages, and after its perusal the reader is left deploring the untoward fate that prevented the completion of the great and much-needed *Geology of Scotland*, which it was the intention of the authors to provide.

G. W. T.

The Structure of Asia. Edited by J. W. GREGORY, LL.D., D.Sc., F.R.S. [Pp. xi + 227, 23 plates, 17 diagrams in text.] (London: Methuen & Co., Ltd., 1929. Price 15*s*. net.)

THIS book is the outcome of an international discussion held at Glasgow in 1928 during the meeting of the British Association. The addresses then delivered have been collected and edited by Prof. J. W. Gregory, who has also contributed an introductory essay which elucidates and unifies the whole book. The great synthesis and interpretation of Asiatic structure made thirty years ago by E. Suess stands in need of revision. Argand, in 1924, introduced revolutionary changes in interpreting the present Asian structures as almost entirely due to the Tertiary paroxysm of earth movement, the continent as a whole drifting southward, its ancient stable blocks at the same time crumpling the intermediate areas into fold-mountain chains. Suess, on the other hand, regarded the ancient blocks as affecting the trends of the mountain ranges by mere passive resistance, and thus thought that the younger Alpine mountains had been influenced and moulded by the remnants of the older Altaid mountains. Prof. Gregory regards Suess's interpretation as fundamentally correct, although it needs correction in several important points, notably in his views as to the structure of Eastern Asia. While the general movement in Asia has been from north to south towards the hollow of the Indian Ocean, the direction of movement is locally reversed in the south-eastern quadrant of the continent, as is shown by the new Russian work in Turkestan and the work of the Anglo-Persian Oil Co.'s geologists in Persia.

A vast amount of fresh work on Asian geology has been published since Suess's synthesis by the Russians in Siberia and Turkestan, by the Chinese and Japanese geologists in their respective countries, by the French in Indo-China, by the Dutch in the East Indies, and by the British in Burma, India, and Persia. The largest single contribution to the present symposium is a "Contribution to the Stratigraphy and Tectonics of the Iranian Ranges," by Dr. H. de Bockh and his colleagues of the A.P.O.C., which occupies a little more than half the book. This is a mine of facts of immense value for Asian tectonics, and geologists will be grateful to the A.P.O.C. for releasing this mass of new information. Another important paper is that by Prof. F. E. Suess (the son of E. Suess) on, "The European Altaids and Their Correlation to the Asiatic Structure." Prof. Suess differs from his father in believing that the crystalline schists of the ancient blocks are of Palæozoic ages, and have been largely produced by the intrusion of great masses of granite. Prof. D. I. Mushketov, the Director of the Russian Geological Survey, writes on, "The Tectonic Features of the East Ferghani-Alai Range," and Prof. G. B. Barbour of Peiping on, "The Structural Evolution of Eastern

Asia." The book also includes three shorter papers by W. D. West on the North-West Himalaya, C. P. Berkey on the Gobi region, and H. A. Brouwer on the East Indies.

The book is illustrated by a splendid series of maps, sections, and diagrams, which have, however, added considerably to its cost. Prof. Gregory is to be congratulated on his team of contributors, on the general high standard of their work, and on the way he has welded their different contributions into as coherent a whole as the nature of the subject would permit.

G. W. T.

Impurities in Metals. Their Influence on Structure and Properties. By Colin J. Smithells, M.C., D.Sc. [Pp. xiii + 190, with 181 illustrations.] (London: Chapman & Hall, 1930. Price 18*s.* net.)

In some ways this book is disappointing. It is too short, and the available space has not all been used to the best advantage. There is too little in it about impurities in metals, and too much about other subjects. Impurities are harmful minor constituents which are commonly present in metals and alloys because of the difficulty of getting rid of them. But many alloys contain minor constituents which are added deliberately, and for good reasons, and which form an essential part of the alloy. The book deals with minor constituents in general, not merely with impurities, and the title should be altered accordingly.

It would be difficult to compress into a book of this size all the data, or even all the important data about minor constituents, and there should be no room in the book for the first four chapters. These chapters (and part of the last) might be described as an introduction to modern metallography. They are well written and interesting, but they should either be made much fuller or omitted. As they are not really an essential part of the subject, the latter course would be preferable. The remaining five chapters deal with the effect of minor constituents on the structure (two chapters), mechanical properties, electrical properties, and corrosion of metals and alloys. As far as they go they are excellent, and since nothing more comprehensive is available, they are extremely valuable. In these chapters, the author has gathered together data from a great number of sources, and has succeeded in making much of the data easier to grasp, by showing how they fit into general theories. Although the author has merely outlined the data, he has given copious references to the original papers from which fuller particulars can be obtained.

The book is clearly written, well printed, and beautifully illustrated. Of the 181 figures, 114 are half-tone reproductions; 16 of these are X-ray patterns, and the others photomicrographs. All the illustrations have been selected with care, and they add considerably to the value of the book.

M. S. Fisher.

GEOGRAPHY

East Yorkshire: A Study in Agricultural Geography. By S. E. J. Best, B.Sc., Ph.D. (Leeds), F.R.G.S. [Pp. 1–189.] (London: Longmans, Green & Co., 1930. Price 16*s.* net.)

Dr. Best is to be congratulated in presenting a regional survey, usually regarded and treated as a dull and uninspiring subject, in a live and attractive form which rivets attention to the practical value of this method of geographical study. The peculiar interest lies in the attempt to associate the effect of environment on the points of view, hopes, and aspirations of the people of the East Riding. A study of the distribution of soils, crops, and population, together with their inter-relations with one another, provides the basis for approaching the human aspect of the subject. The relatively small area of 750,000 acres has rendered it possible to make the enquiry more detailed.

Geologically, the most important deposits in the East Riding are glacial and post-glacial, together with large areas of chalk with and without flints on the wolds. Soil types are very diverse, and may vary considerably on any one farm or even in one field, a fact which renders the task of preparing a soil map of the area one of considerable magnitude. None of the usual systems of geological, chemical and physical, or agricultural soil classification meet the case fully. A hybrid and general classification has therefore been evolved which has proved adequate as a starting-point from which to work, and a generalised soil-regions map has been built up on that basis and discussed in relation to the crops and natural vegetation associated with different soil types. Natural and artificial warps are very characteristic of the East Riding, and warping has apparently been carried out for an indefinite period in the past.

Distribution maps of the chief crops show a concentration of potatoes in the south-west, beans in the south-east, that of bare fallow following the latter. Wheat, barley, oats, turnips, and swedes and permanent grass are more generally distributed, but also show definite areas of concentration, varying with the crop. Similar maps for stock indicate a scarcity of sheep in the south and a more even distribution of cattle for milk than of cattle for beef.

The agricultural history of the East Riding for the last three hundred years shows that on the whole it has been a prosperous agricultural area from quite early times. A chronological list of the chief events affecting agriculture since 1800 throws considerable light on the changes in farming policy during the last century. The comparatively small variations in the agricultural population are overwhelmed by the rapid increase in the urban population, largely due to the growth of Hull since 1871. A most interesting series of graphs indicates the percentage variation in the two types of population since 1821 in each of the thirteen soil regions outlined, and suggestions are put forward to account for the changes. Market towns and countryside alike react to the fluctuations of agricultural prosperity, and the recent history of Driffield provides a good illustration of the trend of events. The foregoing survey leads up to a description and discussion of the East Riding of to-day, with an account of the distribution of population, towns, and villages, the methods of transport, and present-day agriculture. The author's sympathetic feeling for the East Riding farmers yet allows him to admit that their great fault is their conservatism, and he advocates schemes of co-operation carefully worked out on a sound and acceptable basis. In the space of a review it is not practicable to outline the suggestions or arguments put forward in this connection, but a perusal of the pages would well repay the reader interested in this problem. The final note is that of the value of education, and the importance of helping the farmer and the farm labourer towards a more liberal education which will enable them to adapt themselves to rapid changes under modern conditions.

W. E. Brenchley.

BOTANY

The Dispersal of Plants throughout the World. By H. N. Ridley, M.A., F.R.S. [Pp. xx–744, with 22 plates.] (Ashford, Kent: Lovell Reeve & Co., 1930. Price 63*s*. net.)

The subject of dispersal is always a fascinating one, and the great diversity of means by which this is attained is largely responsible for its complexity. The author of this work rightly emphasises the fact that the reproductive bodies of the same species may owe their dispersal to more than one agency, as when the wind-borne seed after it has reached the ground is carried still farther by the action of rain-water.

The large mass of data here summarised, which include not only published

observations but a number of the author's own records, constitutes an important work of reference in a subject of which the literature is both voluminous and scattered.

The so-called mechanisms of dispersal are treated very fully under the headings of Wind-dispersal (pp. 1–162), Dispersal by Water (pp. 163–334), Dispersal by Animals and Man (pp. 335–659), and Mechanical Dispersal (pp. 660–74). There are, in addition, chapters on Island Floras, Dispersal of Orders and Genera, and a bibliography of some 500 references to the more important papers cited. Under each dispersal agency a large number of examples are described which are classified according to the morphological nature of the presumed adaptation and the affinities of the species concerned.

The plates contain a large number of figures of seeds and fruits without, unfortunately, any indication as to the scale of representation. There is a copious index which, however, having regard to the character of the work, might with advantage have been more complete.

The work as a whole constitutes an important and valuable work of reference, in which the reader can find data respecting most aspects of the subject—from the weight of the dust seeds of *Monotropa hypopitys*, which are under ·00005 of a grain, to the time taken for the seed swallowed by a bird to pass through the intestine.

It may be noted that the numerous references to Krakatau lose much of their force since the pertinent indictment of the evidence regarding its colonisation; that the number of pores in the capsule of *Antirrhinum* is three, and not four as stated; and that vegetative propagation in *Linaria vulgaris* is probably far more responsible for its distribution than its wind-borne seeds.

We cannot always agree with the author, as when he states that in his opinion there is not more than one genuine species included in *Taraxacum dens-leonis* or when he sees purposeful adaptation in most of the structures which he describes. But these are minor points in a work that provides a wealth of data and abundant descriptions of a diversity of dispersal mechanisms which will serve as a valuable work of reference to the general reader and specialist alike.

E. J. S.

Australian Rain-forest Trees (excluding the Species confined to the Tropics). By W. D. FRANCIS. [Pp. 14 + 347, with 1 map frontispiece and 226 figures.] (Brisbane, Australia: Government Printers, 1929. Price 10*s*. net.)

THIS is a descriptive account of the species of trees which characterise the Australian rain-forests developed in the areas where the rainfall is usually more than 60 inches in the year and there is no pronounced dry season. This type of vegetation commonly attains a height of from 80 to 160 feet, and is characterised by an uneven profile and the great variety of the constituent species. Usually, even in limited areas, a very large number of species are involved, and many of these have buttresses. The southern rain-forests comprise a smaller number of species, and in this region a single tree may become dominant, as is the case with *Ceratopetalum apetalum* on the coastal highlands of Dorrigo. The main part of the text consists of descriptions of over 280 tree species. These descriptions are of diverse length, but mostly contain details not only of the reproductive organs and foliage, but also of the type of trunk and the bark characters. An outstanding feature is the profuse illustration, with reproductions of photographs of the bole and of the flower and fruit of a considerable proportion of the species described.

The descriptions aim at avoiding as far as possible technical terms, and identification is facilitated by the provision of keys, both artificial and generic.

The work should prove of considerable value both to the technologist and botanist, and the action of the Commonwealth Government Committee in securing its publication sufficiently vindicates the wisdom of placing funds at their disposal for such purposes.

E. J. S.

Fungous Diseases of Plants in Agriculture, Horticulture, and Forestry. By Dr. JAKOB ERIKSSON. Second Edition. Translated from the German by W. GOODWIN, M.Sc., Ph.D. [Pp. viii + 526, with 399 illustrations.] (London: Baillière, Tindall & Cox, 1930. Price 35*s*. net.)

THIS excellently printed and well-illustrated work on the more important fungous pests of the cultivated plants of temperate Europe is a useful guide not merely to their identification but also their control. Further, the bibliographical references following the description of each disease enables the student requiring more specialist knowledge to extend his reading.

A great merit of the work is that the descriptions, though quite lucid, are concise, and the non-specialist is not overwhelmed by a plethora of detail.

The last few pages contain some data with regard to losses due to fungous diseases which will appear startling to the lay mind. Thus the yearly loss of wheat crop due to rust in the world is estimated at 250 million dollars, whilst 2,500 million dollars is the estimated annual loss from fungal diseases of the vine. Such figures emphasise the importance of increased knowledge respecting causes and remedial measures, and it may confidently be anticipated that the present volume will become one of the standard works of reference on this subject.

E. J. S.

Comparative Morphology of Fungi. By E. A. GÄUMANN. Translated and revised by C. W. DODGE. [Pp. xiv + 710.] (London: McGraw-Hill Publishing Co., 1928. Price 37*s*. 6*d*. net.)

THIS excellent and comprehensive handbook has as its basis Gäumann's *Comparative Morphology of Fungi*, but, as the preface to the American edition states, the translation does not closely follow the German original, and certain sections of the book (notably the sections dealing with the orders of the Basidiomycetes) were in great part rewritten in order to incorporate later views on the classification of these organisms, and the results of new work published after the appearance of the German edition.

The field of work is very wide, and, treated with such a wealth of critical and illustrative detail as the author and translator have seen fit to employ, the subject would assume impossible dimensions were the discussion not curtailed in some of its less important aspects.

In order to keep the book within moderate compass, the historical aspect is not considered; but where divergent views are held as the result of contemporary research, evidence *pro* and *con* is most fully put forward and critically discussed.

The opening chapters give briefly a generalised account of the thallus, the reproductive organs and the problems of sexuality. These chapters constitute, in the main, a dictionary of terms and terminology—each new term as it is introduced being emphasised by special type and clearly defined.

The great body of the book is occupied with a study of the morphology of the groups Archimycetes, Phycomycetes, Ascomycetes, and Basidiomycetes, and closes with a brief chapter on *Fungi imperfecti*. Bacteria and Myxomycetes are not considered. The chapter on the Laboulbeniales, in the preparation of which the authors have incorporated the results of Thaxter's work on this group, constitutes almost a brief monograph in itself.

A final chapter gives a review of Fungus Classification.

That the book is produced by the McGraw-Hill Publishing Company is sufficient guarantee of the excellence of the type and illustrations; the text is remarkably free from typographical errors.

The work can be unhesitatingly commended; it is indispensable to the serious student of the fungi, and its indispensability is further emphasised by the excellent bibliography of more than a thousand headings which accompanies the book. N. F.

ZOOLOGY

Histological and Illustrative Methods for Entomologists. By H. ELTRINGHAM, M.A., D.Sc., F.R.S. [Pp. xi + 139, with 21 figures.] (Oxford: at the Clarendon Press, 1930. Price 7*s.* 6*d.* net.)

THE entomologist, who has had no opportunity of acquiring training in laboratory methods, will be indebted to the author of this book of one hundred and forty pages for his lucid and concise account of the essential materials and processes employed in the study of insect anatomy and histology. Here he will find the necessary instructions on how to prepare his specimens for section-cutting and staining and, incidentally, how to avoid those exasperating mistakes, which are the bane of all beginners in the field of microscopy. Because of their intractable chitinous exoskeleton, insects are particularly trying subjects for section-cutting, but with the gathering of experience in the use of the proper fixatives and embedding materials, many of the difficulties can be successfully overcome.

The manipulation of the smaller species of insects in the preparation of whole mounts is not generally considered an easy matter, and even the more experienced entomologist will find the suggestions contributed by Mr. H. Britten in a special chapter of great assistance. The method which he recommends is at once simple and effective, and beautiful mounts can be readily obtained.

The training of no entomologist is complete who neglects the uses of illustration, and the author has been at no small pains to show that the lack of artistic ability can be readily discountenanced by the appropriate use of the many mechanical aids that are readily available in the preparation of diagrams. Hints are also furnished on the making of simple models and the colouring of lantern slides and photographs, so that there should be no valid excuse why the entomologist should not develop into an interesting lecturer upon the subject, which he has chosen either as a hobby or profession.

A. E. CAMERON.

Principles of Animal Biology. By LANCELOT T. HOGBEN, M.A., D.Sc. [Pp. xxi + 332, with 125 illustrations.] (London: Christophers, 1930. Price 8*s.* 6*d.* net.)

ZOOLOGY and biology are terms of such enormous content that they may be used to cover a multitude of sins, and when in actual practice the task of devising an elementary course in either is essayed, no two people will agree as to the ground it should cover. It is true also that in Britain at any rate there has been a tendency, in some instances strongly marked, to overweight the morphological aspect of the subject. Attempts have been made at various times to break away from this tradition, and the present book is one of them. It does not follow that the new courses suggested in this or any other book are better than the old, but they are different. The present work, in our opinion, swings over to the other extreme and places the weight upon the physiological aspect of zoology, including such a minimum of morphology as will help to make the physiological work intelligible, and may perhaps serve as a sop to the older traditions and to those who favour

the "polysyllabic aspects of embryology." To this have been added introductory genetics and other interesting topics. Apart from the latter, a book of this type—not, of course, containing the same facts, for many of them were not known then—if it had been written twenty or twenty-five years ago would in all probability have been entitled *Elementary General Physiology* or something similar.

The stress on the functional aspect is so great that simple morphological facts are sometimes obscured. It is disturbing to find on p. 219, for example, the Crocodilia defined as having an incomplete ventricular septum. But the obscurity sometimes goes farther and affects physiological questions also. On p. 97 we find a diagram of the circulatory system of the frog which ignores the cutaneous circulation, yet surely the fact that in most frogs the skin is at least as important a respiratory organ as the lungs is of considerable physiological interest. Again, on p. 92 we find the term *truncus arteriosus* used for *conus arteriosus*; the matter may seem merely one of nomenclature, but one of the principal differences between them is that the former contains rhythmically contractile cardiac tissue and the former does not. Leuckart is persistently spelled Leuckhart. Apart from such blemishes the book contains a number of suggestions which are well worthy of the serious consideration of all those biologists, and they are in the majority, who have at heart the provision of a satisfactory introduction to zoology and biology. It would be possible, by treating this book in the critical manner adopted by the author in his "Foreword," to pass a number of adverse comments on it, but it is perhaps better to accept it in the spirit in which it is offered. To quote: "I do not claim that my own way of teaching is an ideal one, but I do venture to believe that at a time when methods of teaching zoology are undergoing considerable changes it is worth while that those of us who are trying out new methods should place them at the disposal of others."

C. H. O'D.

BACTERIOLOGY

Physiology and Biochemistry of Bacteria. By R. E. Buchanan and E. I. Fulmer. Vol. II [pp. xvii + 709, with 57 illustrations], and Vol. III [pp. xv + 575, with 2 illustrations]. (London: Baillière, Tindall & Cox, 1930. Price 34*s*. net per volume.)

The first volume of this work, which appeared in 1928, was reviewed on p. 163 of Vol. XXIV of this Journal. Vol. I dealt with growth phases, composition and biophysical chemistry of bacteria and their environment, and energetics.

Vol. II is subdivided into three sections. Section A is a short but admirable description in twenty-nine pages of the mathematics and physical chemistry of reactions in homogeneous and heterogeneous systems. Section B, of some 170 pages, deals with the effects of *physical* environment upon micro-organisms. The effects of temperature on the growth and death of micro-organisms, and on the rates of metabolism, of light rays of different wave-lengths, and of radium emanations are exhaustively treated. The effect of various physical environments such as pressure, mechanical motion, and surface tension are also touched upon. Section C, which constitutes the greater part of this volume, traces the effects of *chemical* environment upon micro-organisms. This section opens with a general discussion on the effect of chemical environment on the rates of growth, of death, of metabolism, and of movement, and then develops into a detailed treatise on the effects of different types of chemical compounds on micro-organisms. For this purpose chemical compounds are divided into inorganic compounds and their ions, non-nitrogenous organic compounds, *e.g.* alcohols, acids, saccharides, etc., and nitrogenous organic compounds, *e.g.* amines, amino-

acids, amides, peptides, proteins, and, somewhat arbitrarily, of course, vitamins, auximones, and bios.

Vol. III, which is essentially a continuation of Vol. II, opens with a very short section of twenty-six pages, devoted to certain special physiological inter-relationships of micro-organisms, such as antibiosis and symbiosis. Whereas Vol. II deals mainly with the effect of chemical environment on micro-organisms, almost the whole of Vol. III deals with the effect of micro-organisms on their chemical environment. Accepting the view that chemical changes induced by micro-organisms are brought about through the effect of a series of enzymes, the authors review all the enzymes which have been demonstrated in micro-organisms and then pass on to deal with the different types of chemical reactions brought about by these enzymes.

The reviewer wishes at once to congratulate the authors on the almost Teutonic thoroughness with which they have carried out their task, and to recommend the whole work, in spite of its price, to all who are engaged in the teaching of, or research in, microbiological chemistry. While it is probably true to say that certain aspects of the subject have been equally well dealt with by previous writers, there is, in the reviewer's opinion, no book in any language which deals so exhaustively with the subject as a whole as does this work. The authors will doubtless be partly compensated for their tremendous labours by the large sale of the book, but their greatest reward will be the undoubted fillip which the book will give to progress in the subject.

H. RAISTRICK.

An Index to the Chemical Action of Micro-organisms on the Non-nitrogenous Organic Compounds. By E. I. FULMER and C. H. WERKMAN. [Pp. xiii + 198.] (London: Baillière, Tindall & Cox, 1930. Price 20s. net.)

THIS unusual book is further evidence of the rapid strides which microbiological chemistry has made in recent years. It, together with Buchanan and Fulmer's book *The Physiology and Biochemistry of Bacteria*, indicate that microbiology is now, in common with other biological sciences, tending to develop more quickly on the chemical side than on the purely descriptive side. In other words, the interest in micro-organisms is tending to shift from the point of view of what these organisms look like to what they are capable of doing in a chemical sense. Here the reviewer wishes to offer a word of warning to any chemist contemplating work in this field, since it seems to be but little realised that microbiological chemistry is something more than chemistry with a smattering of microbiology. This point of view is very well expressed by the authors of this work. "It is unfortunate that occasionally investigators will execute a very careful piece of research from a chemical point of view, and fail to describe the organism with which the work was done, merely referring to it as a 'bacillus.' Frequently a description of an organism has been given which may be adequate for purposes of classification, but no name has been attached. The authors wish to emphasise the importance of a proper use of bacterial taxonomy in chemical studies of fermentation." An excellent example of this type of error is afforded by a large number of papers which have appeared, dealing with some aspect of the metabolism of *Penicillium glaucum*. It has been customary in the majority of instances to give this name to any green *Penicillium*, in spite of the fact that there are dozens of morphologically distinct species of *Penicillium* which are green in colour, and which are incidentally a cause of despair even to an experienced mycologist.

This book does not claim to be a critical review of the extensive literature on microbiological chemistry, but rather a summary, in tabular form, of the chemical substances which have been reported as produced by the action of

bacteria, yeasts, and moulds on various non-nitrogenous compounds. Judged from this point of view, there can be no doubt that the authors have carried out their unenviable task in a thoroughly praiseworthy manner, and while most of the information is already available in Buchanan and Fulmer's work previously referred to, it is not presented there in such a readily accessible form.

The book consists of three tables of about fifty pages each and a list of references of some thirty pages. Table 1 is an index to micro-organisms, in which micro-organisms are arranged in alphabetical order. The substance used, the product obtained, and the authority are then given in the order indicated, under each micro-organism in turn. In Table 2 the same information is given in a different way, since here the substrates are arranged in alphabetical order, followed by the product, organism, and authority, thus enabling the reader to gain at a glance an idea of the various products elaborated from a given substrate by the micro-organism involved. Finally, in Table 3 the order is products (arranged alphabetically), substrate, organism, and authority.

It will be obvious from the above description that this book is not one to read but rather to consult. To many, including the reviewer, who have spent weary hours searching through a very widely scattered literature, it will be a positive boon in spite of what appears to be its unnecessarily high price.

H. RAISTRICK.

Laboratory Manual in Bacteriology. By S. THOMAS. [Pp. ix + 154.] (McGraw-Hill Publishing Co., Ltd., 1930. Price 8*s.* 9*d.* net.)

THIS unpretentious laboratory manual, which is intended by the author as a companion volume to his *Bacteriology*, sets out to provide " a certain number of laboratory exercises that every beginner must perform in order to learn the necessary fundamental principles of the science, and obtain practice in laboratory technique. The work should be covered in a semester of six hours laboratory time a week."

Twenty-three standard cultures, partly pathogenic and partly non-pathogenic, all of which may be obtained from the American Type Culture Collection, form the organisms for study. A series of thirty-three simple exercises is then given, the choice of which appears to have been wisely made. The book closes with an appendix dealing with the cleaning of laboratory glassware, sterilisation, and the preparation, reaction, titration, and buffer content of culture media.

A curious feature of the book is that, of its 154 pages, a total of 34 pages are completely blank, while a further 15 pages are blank to the extent of a half-page or more.

H. RAISTRICK.

A Compilation of Culture Media, for the Cultivation of Micro-organisms. By M. LEVINE and H. W. SCHOENLEIN. [Pp. xvi + 969.] (London: Baillière, Tindall & Cox, 1930. Price 67*s.* 6*d.* net.)

THIS book does not claim to be a critical review of the numerous media which have been proposed from time to time for the cultivation of micro-organisms, but, to quote from the preface, " It was felt that the collection and orderly arrangement of such culture media would be of assistance in the future development of the science of Bacteriology." Judged from this point of view, there can be little doubt that the book will meet a very marked need, and, in conjunction with Bergey's *Manual of Determinative Bacteriology*, will be as useful to the bacteriologist as Richter and Beilstein are to the organic chemist.

About 7,000 culture media were collected from the literature and are arranged and classified into approximately 2,500 quite distinct media. A number and a distinctive name are assigned to each medium, which is then described under the following headings: Constituents, Preparation, Sterilisation, Use, Variants, Literature References. This part of the book occupies some 830 pages, and is followed by three indexes: a medium-name index, which is an alphabetical list of all media described; a constituents index, in which is listed every medium in which a particular substance was employed; and an author index.

The media are divided into two main groups, liquid media and solid media, and then subdivided as follows into seven groups on the basis of their physical state.

Liquid media	Group I
Solid media:	
(*a*) Initially liquid, reversibly liquid and solid, liquefiable by heat:	
Solidifying agent, Agar-agar	Group II
,, ,, Gelatine	Group III
,, ,, Other materials	Group IV
(*b*) Initially liquid; irreversible:	
Solidifying agent, organic, *e.g.* starch, serum, egg albumin	Group V
Solidifying agent, inorganic, *e.g.* silicic acid, gypsum .	Group VI
(*c*) Initially solid, *e.g.* potato, carrot, filter paper . .	Group VII

Individual media are identified by assigning to them a number, the name of the author who first described the medium, and a specific name which includes the chief carbon and nitrogen source, *e.g.* "273 Ayers' Glucose, Ammonium Phosphate Solution." In solid media the solidifying agent is also specified, *e.g.* "1553 Grieg Smith's, Sucrose, Peptone, Agar."

The book is strongly recommended to all who are interested in the question of microbiological media of all types, *e.g.* bacteriologists, mycologists, biochemists. The directions are so sufficiently detailed as to make reference to the original literature unnecessary, though very full references are given throughout.

H. Raistrick.

MISCELLANEOUS

Identity and Reality. By Emile Meyerson. Authorised Translation by Kate Loewenberg. [Pp. 495.] (London: George Allen & Unwin, 1930. Price 16*s*. net.)

M. Meyerson's *Identité et Réalité* first appeared in Paris in 1908. In his preface to the English edition the author recalls the fact that I directed the attention of English readers to his book on its first appearance in a long review which was published in October 1908. That was M. Meyerson's first book, and he was quite unknown to me. My strong commendation of the book to the attention of English men of science was due entirely to its intrinsic merits. Since that time M. Meyerson has published other important works (notably *L'Explication dans les Sciences*, 2 vols., 1921, and *La Déduction relativiste*, 1926); the *Identité et Realité* reached a third edition in 1926; and his views have been praised by such distinguished thinkers as Bergson, Einstein, and the late Lord Balfour.

The problems discussed by M. Meyerson are on the border-line between science and philosophy. During the nineteenth century science and philosophy were not exactly on friendly terms, but the present century has shown a marked tendency to restore them to the more friendly relations of earlier centuries. Twentieth-century philosophico-scientific literature is already considerable, and M. Meyerson's contributions to it are among the most

valuable. As a practical chemist in his earlier years, M. Meyerson approached his problems without any philosophical bias, and this makes his views all the more interesting.

The book before us begins with a discussion of the view, so fashionable in the nineteenth century, that science is not concerned with explanation but only with description, and the discovery of apparently regular sequences between phenomena. The history of science, however, shows conclusively that at all times science sought, and still seeks, causal explanations. Moreover, this search for causes is not a form of aberration; it is the natural expression of man's mentality. Thought as such looks for identity amid variety, for persistence amid change; and the search for causes is an expression of this tendency. The urge to do so is so strong that at times it appears as if men of science were indifferent about *what* exactly remains the same so long as they could maintain that *something* persists amid change. This is illustrated by the history of the atomic theory, from the minute marbles, etc., assumed in early times to the electrons and protons of the present day. And the readiness with which scientists accepted the principles of the conservation of matter and energy, though the experimental evidence was very slender, affords another illustration of the same tendency. What these principles really came to was the plausible assumption that "there is something (as M. Poincaré put it) which remains the same." To the human mind, only those phenomena of which this seems true appear to be "rational."

The reality of change, however, cannot be ignored. In one form or another it has to be faced and recognised. Perhaps its most striking recognition on the part of science is to be found in the principle of Carnot, and its emphasis on the irreversible direction of the flow of heat. Such irreversible changes cannot be expressed under the conception of identity, and therefore also not under the notion of causality. They can only be described in formulæ expressing more or less regular sequences or laws, and are apt to appear "irrational."

Science, according to M. Meyerson, is thus made to pursue two different paths. It seeks causal explanations wherever it can trace identity or persistence; but is content with mere "laws" or regularities of sequence in the case of phenomena of irreversible changes in which no identity or persistence can be traced.

An adequate account of M. Meyerson's book would need a lot of space. But enough has perhaps been said to explain the title of the book, and to indicate some of the profoundly interesting speculations with which it deals. M. Meyerson throughout shows a rare knowledge of the history of science, and his explanations and criticisms are always illuminating and penetrating. The book is most welcome. The translation of it must have been no easy task, and the translator deserves praise for the success with which it has been executed.

A. Wolf.

Pioneers of Electrical Communication. By Rollo Appleyard. [Pp. ix + 347.] (London: Macmillan & Co., 1930. Price 21s. net.)

Mr. Appleyard has produced a book which is of outstanding merit, and those of us who are interested in the historical development of physical science are heavily in his debt for the ungrudging labour bestowed upon this most interesting work, which, in the course of some three hundred and thirty octavo pages, traces the lives and achievements of Maxwell, Ampère, Volta, Wheatstone, Hertz, Oersted, Ohm, Heaviside, Chappe, and Ronalds.

Some of these names may be new, even to the professional physicist; some, possibly, are *nomina et praeterea nihil*; and there are among us some who may ask why certain outstanding names—Faraday, for example, and

Kelvin—are absent. But if a choice must be made, the choice should be left in the author's hands. And it must be confessed that the author has chosen well.

Faraday we know, and Kelvin we know, but Ohm and Oersted are something of shadows, and, as far as the reviewer is aware, this is the first book which contains anything approaching an adequate estimate of Oliver Heaviside's arresting and complex personality.

Mr. Appleyard concerns himself rather more with the men than with their achievements—and wisely so; for after all, the story and history of electrical communication are to be found in a score of textbooks and treatises. But Mr. Appleyard, while giving an adequate review of their work, has made it his chief task to show up the personality of each of his subjects against the background of his time; and he has succeeded, mainly because he has read widely and searched deeply, has visited the scenes of the labours of the pioneers, has gone straight to the fountain head wherever possible, avoiding second-hand authorities—has, in short, embarked upon, and carried out, his work in a very real spirit of research.

The book can be thoroughly recommended.

But is Mr. Appleyard *quite* sure that the interesting photograph of Wheatstone and his friends, which is reproduced on page 105, is not a "composite"?

A. F.

A Catalogue of British Scientific and Technical Books. Third edition compiled by DAPHNE SHAW. [Pp. xxi + 754.] (London: A. and F. Denny, for the British Science Guild, 1930. Price 20s. net.)

THE British Science Guild continues year by year to be of material assistance to scientific workers in a large number of ways. Not the least important of its activities is the production of this Catalogue, which now appears in its third edition in a much enlarged form. The committee of experts responsible for the production is to be congratulated on the obvious care which it has devoted to a piece of work which must inevitably have presented the greatest difficulties. Anyone who has been so unfortunate as to find himself obliged to attempt the compilation of a selected bibliography will know that it is a task formidable enough to make the stoutest quail. So admirably, however, has the work of selecting the fourteen thousand entries in the present volume been done, that librarians in charge of general scientific libraries might well be advised to see to it that a copy of every work in this catalogue is on their shelves.

But while the selection itself deserves nothing but praise, there are one or two points in the arrangement of the entries which may justifiably be criticised. The headings "Library Science" and "Bibliographies and Library Catalogues" appear for the first time in this edition. The latter of these groups contains only five entries, and does not include the "Catalogue of Current Mathematical Journals, etc., compiled for the Mathematical Association," London, 1913, nor R. T. Leiper's "Periodicals of Medicine and the Allied Sciences in British Libraries," published by the British Medical Association, London, 1923, while the invaluable "World List of Scientific Periodicals" appears among "Directories and Year Books." The Geological Society's "List of Geological Literature," and the Geologist Association's "Catalogue" are entered under the heading "Geology," in spite of the fact that each of these works is the catalogue of a library. On the other hand, R. A. Peddie's "Engineering and Metallurgical Books" is one of the five works brought together as "Bibliographies and Library Catalogues."

This is an inconsistency in classification to which the editors might well devote their attention before the next issue of the book is produced. Whether they decide to place these special bibliographies with other works dealing with

the same branch of science or to bring them together in the group of bibliographies, they should, in any case, move the entry for the "Catalogue of Scientific Papers, compiled by the Royal Society," out of the group of "Collected Scientific Papers." This heading should be confined to the collected writings of individual scientists.

The volume is admirably printed and produced, and is provided with indices of names and subjects. If the entries in the Catalogue were numbered so that references could be made in the indices to numbers instead of to pages, the gain to users of the book would be obvious.

J. WILKS.

A History of Science and its Relations with Philosophy and Religion. By W. C. D. DAMPIER-WHETHAM, M.A., F.R.S. [Pp. xxi + 514.] (Cambridge: at the University Press, 1929. Price 18s.)

NEARLY a century has elapsed since Whewell, of Trinity College, Cambridge, published his *History of the Inductive Sciences*, and followed it up in due course with other important works on the *Philosophy of the Inductive Sciences* and kindred works. These books stimulated considerable interest in the history, methods, and principles of science. The way had been prepared to some extent by Whewell's friend Herschel, whose brilliant little *Introduction to the Study of Natural Philosophy* (1830) was a masterpiece of its kind. These books, in course of time, prompted John Stuart Mill to write his *System of Logic* (1849), in which he gave a full and persuasive account of scientific method. Mill's book passed through many editions, and eclipsed the works of his predecessors. This was largely due to the fact that Whewell was an apriorist philosopher and a theologian, whereas Mill was an empiricist and a naturalist, and therefore more in harmony with the temper of the nineteenth century, especially in England. Even in Cambridge, as the late Lord Balfour has informed us, Mill ousted Whewell from favour. Whewell's apriorist and theological fight was taken up again, first by the Oxford idealists, and then by a number of Cambridge philosophers, including the late Prof. James Ward and the Earl of Balfour. So far, therefore, as the philosophy and logic of science are concerned, Whewell's work may be said to have been very stimulating. It was different, however, with the history of science. Whewell's initiative in beginning this new department of academic study bore no obvious fruit. The study of the history of science has only just made a start in British Universities. But the heritage which Whewell left behind him, and the example which he set, have evidently not been wasted. And it is particularly gratifying to find that another distinguished member of Whewell's old college has now taken up the torch and carried it a stage farther.

The task of writing a general history of science has grown enormously in difficulty since Whewell attempted it. This is obvious from the fact that Mr. Dampier-Whetham has found it necessary to devote nearly two-fifths of his book to this period. The attempt to write the general history of science single-handed, and to take account also of relevant philosophical and religious problems, is one that obviously calls for wide knowledge, deep insight, and high enterprise. That it should be completely successful is not to be expected. For one thing, it necessitates drastic selection and rejection of material, and probably no two people would make quite the same selection. Moreover, difficult as is the problem of correctly describing so many different kinds of scientific discoveries and philosophic views, it is even more difficult to adopt a standpoint that may satisfy everybody. Anyone who ventures on such an enterprise certainly lays himself open to all sorts of possible criticisms. Mr. Dampier-Whetham is fully aware of all this, and seems to avert all criticism by saying that he has written the volume now under review chiefly for his own satisfaction and amuse-

ment, though he hopes that his labours may be useful also to others. Let me say at once that I do not know of anybody who single-handed could have written a better history of science. No doubt the time will come when a larger history of science will be written on co-operative lines after the manner of the *Cambridge Modern History*, etc. But in the meanwhile Mr. Dampier-Whetham's book is most welcome, and is sure to be very useful to more than one generation of readers, if these are not so obsessed with a respect for the printed word as to believe uncritically all that is contained in a book composed by an author of repute and printed by a famous press.

Of palpable mistakes in the matter of details the book is comparatively free, though it does contain some statements which are open to criticism. On p. 38, for instance (also on p. 66), Platonic "Ideas" are referred to as if they were ideas in the mind of man, *i.e.* mental concepts. On p. 47 Aristotle's work in connection with the principles of the lever (in his *Mechanica*) is entirely ignored, and Archimedes gets all the credit. On p. 104 Scholasticism is credited with the spread of the belief in the uniformity of Nature—surely a strange allegation in view of their supreme faith in miracles! On p. 148 Descartes is saddled with the idea of a "direct apprehension of God," in spite of his laborious attempts to formulate three separate proofs of the existence of God. On p. 189 it is pointed out that Newton regarded action at a distance as absurd, yet on p. 195 there is a quotation from Newton in which he appears to accept the reality of action at a distance, and no explanation is vouchsafed of the apparent discrepancy. On p. 207 much is made of Locke's toleration, without any indication of its severe limitations. Perhaps it was natural for Mr. Dampier-Whetham to overlook these limitations, as he himself sometimes writes about religion as if Christianity (or even the Church of England) were the only religion concerned (rather like Parson Thwackum in *Tom Jones*). And this brings me to the main blemish of Mr. Dampier-Whetham's *History of Science*. A really great historian does not cast his shadow across his pages. But this *History of Science* betrays its author's prejudices all too clearly. In several places, for instance, it betrays what is sometimes called the Nordic prejudice (pp. 75, 106), or what Nietzsche described as the "race-swindle." But more serious from the standpoint of science is the author's theological or supernaturalist prejudice. In this respect, no doubt, Mr. Dampier-Whetham is following the example set by Whewell, whose theological prejudices blinded him to the significance of the work of Darwin and Lyell. Moreover, it seems to be the fashion just now among some scientists to coquet with the Churches. Those who write for the "general reader" are always tempted to play to the gallery, and sometimes they succumb to the temptation. But the historian of science should never forget that science began in antiquity with a rejection of supernatural mythology in favour of naturalism, that when later on supernaturalism reasserted itself, science ceased to be, and was lost in the dark ages, and that the revival and development of science in modern times kept pace with the return and growth of naturalism. Naturalism, of course, is *not* the same as materialism, and leaves room for the religion of a reasonable man. But such religion is a different thing from the Churches, which have never been anything but hostile to science and the spirit of science, but are ready to exploit for their own sectarian ends any pronouncements by men of science in favour of religion. Scientists ought to exercise great care not to encourage a new wave of supernaturalism which may prove a serious stumbling-block to further scientific progress.

No doubt there are some readers to whom what I regard as faults will appear to be special merits of Mr. Dampier-Whetham's *History of Science*. In any case my criticisms are not meant to be captious, and are only intended

to stress the need of critical discrimination on the reader's part. Mr. Dampier-Whetham's book as a whole is quite admirable, even if it is not free from those personal predilections of which mere mortals find it so hard to divest themselves.

A. WOLF.

The Theory and Design of Illuminating Engineering Equipment. By L. B. W. JOLLEY, J. M. WALDRAM, and G. H. WILSON. [Pp. xxxi + 709.] (London: Chapman & Hall. Price 45s. net.)

A GOOD idea of the aim of this most impressive treatise may be given by the following quotation from its paper jacket: "The Authors have attempted to attack the subject of illuminating engineering from an aspect not yet treated in detail in any published book, so far as they are aware. Thus this is not a treatise on the science or art of illuminating engineering, but on the design of the equipment which must be employed to attain good practice in lighting."

One of the most noticeable of post-war developments is the very increased importance which is attached to artificial illumination in modern civilisation. Indeed, many of the activities of modern life with its efforts to change night into day, both in work and in play, would probably be attended with deleterious consequences to national health if it were not for the fact that modern science has provided during the last twenty years enormously increased possibilities of light production. The discoveries of the scientist have been followed by the appearance of a new type of specialist engineer—the illuminating engineer, whose function is to devise means of controlling and directing the light emitted by "raw" light sources in accordance with the principles of good illumination in so far as these are recognised at any particular moment.

It is with the control and direction of light that the present book deals, and in so doing it fills very effectively the hiatus referred to by the authors. Much of the information given in the book has been collected from the numerous original papers dealing with particular problems, but a considerable portion is connected with work of very great interest and importance carried out by the authors themselves.

The book, however, not only deals with the actual design of light-producing apparatus, but also with physiological aspects, the physical properties of materials and methods of calculation in so far as these have bearings on design problems. It also deals with special auxiliary equipment used in connection with light sources where it is not described in detail in other places. Indeed, much of this auxiliary equipment is of very recent design, and particulars of it have hardly got into the literature as yet.

The book is divided into five parts. Part I deals with a number of physiological aspects and photometric considerations. Part II deals extremely well with the physical properties of light sources. Part III—the *raison d'être* of the book—deals with the design of equipment and includes more than half of the book. It is divided into eight sections dealing with: (i) Types and Uses of Lighting Equipment, (ii) Mechanical Design of Lighting Equipment, (iii) Reflecting Media and the Design of Reflecting Equipment, (iv) Diffusing Media and the Design of Diffusing Equipment, (v) Refracting Media and the Design of Refracting Equipment, (vi) Characteristics and Design of Optical Projecting Systems, (vii) Colour Media and (viii) Special Lighting Equipment. Part IV deals with auxiliary equipment, such as cable systems, constant current transformers, dimmers, sign flashers, automatic control, etc. Readers will find it very helpful to have these topics treated with particular reference to lighting equipment, and it is very convenient to have them in the same book. Part V consists of numerical data

concerning light sources and materials, together with calculation tables and extracts from British standard specifications.

The book is extremely well produced, although it is rather heavy and could conveniently have been divided into two volumes. The illustrations, of which there are nearly six hundred, are well drawn, and the numerous half-tones extremely well selected to illustrate the authors' points. The price of the book is rather high, but it undoubtedly represents very good value for the money. Taken as a whole, the book is really an encyclopædia on the whole subject of lighting equipment, and will receive a hearty welcome from all illuminating engineers, who will be ill-advised if, in the consideration of their problems, they do not first of all consult Messrs. Jolley, Waldram, and Wilson.

H. B.

Mind at the Crossways. By C. Lloyd Morgan, D.Sc., LL.D., F.R.S., Emeritus Professor in the University of Bristol. [Pp. xi + 275.] (London: Williams & Norgate, 1929. Price 10*s*. 6*d*. net.)

In this book Prof. Lloyd Morgan describes the evolution of mind in two different ways, which he holds are both applicable and quite distinct. The first is the *scientific* explanation, which deals with the relations between events and is not concerned with the actions of agents (to make this quite clear the author even avoids the use of the word "cause"). The second method of description is one in terms of agency—what he calls the *dramatic* explanation. This kind of explanation becomes necessary at a certain stage of evolution, that at which reflective consciousness occurs. Up to this point the scientific explanation is complete (except for the assumption that the whole of evolution is due to a Divine Agent), but when reflective consciousness appears, we get personality and agency, and then science can only abstract from the complete story. The whole story can only be told by involving a dramatic account as well. Both methods of description cover the whole of history; up to the appearance of man only the agency of God need be considered, but afterwards the agency of man must be included as well, and this is not possible in a purely scientific account.

Three levels of emergence in mind are distinguished by Prof. Lloyd Morgan—percipience, perception, and reflection, which "emerge" in this order. He does not think that they "emerge" from something merely physical—the gap is too great, so he uses the term "other than physical."

There are two vital stages in the evolution of mind. The first occurs at the change from percipience to perception, or, in other words, at the formation of the conditioned reflex. At the percipient stage, only mere sensation due to the stimulus of various organs occurs. When these get linked together so that, for instance, a touch produces the right movement of the eyes in the direction of the touch, or the sight of an object produces movements of grasping, then a "cross-over" has occurred and the perceptive level emerges. We pass from a world of crude sensation to one in which former experience plays a part, and a given stimulus produces a conditioned response. It is only after this stage that Prof. Lloyd Morgan thinks the word "mind" can really be used. The second time that mind is "at the crossways" is at the change from perception to reflection. With reflection comes the possibility of guidance and choice.

The whole of evolution is the expression of Divine Purpose, although scientifically it can only be described as the result of "emergence" or "just-comery." The complete story requires both aspects—"If one is justified in scientific belief to the end of interpretation, may one not be justified in dramatic belief for the purpose of explanation?" (p. 271).

Prof. Lloyd Morgan holds, of course, as the basis of his doctrine of emer-

gence, that new properties appear in the world which could not be inferred from the structure of the world up to the moment of emergence, even with the fullest knowledge. This view, together with the imputation of agency involved in the dramatic account, are, of course, not susceptible of proof, and consequently their adoption must depend on heuristic value. In the author's able hands they may perhaps be justified, since he shows that he is well aware of the danger of introducing dramatic conceptions into scientific interpretation, but in general it is surely preferable to keep to a strict "order of nature" as a working hypothesis.

The book is written in Prof. Lloyd Morgan's usual interesting style in simple non-technical language, although he has a way of using words which is all his own and requires some getting used to.

G. B. BROWN.

Index to Literature of Food Investigation. By AGNES ELISABETH GLENNIE, B.Sc. [Pp. 108.] (London: H.M. Stationery Office, 1930. Price 2*s*. net.)

To food chemists, particularly those concerned with industrial applications, this "index" constitutes an extremely useful abstract journal. The papers abstracted deal principally with food preservation and food transport. Entries are classified into separate sections dealing with "meat, pig-flesh, poultry and game, fish, eggs, dairy produce, fats and oils, fruit and vegetables, grain, crops and seeds, theory of canning, theory of freezing and chilling, bacteriology, mycology, engineering, and miscellaneous." The abstracts are concise and to the point, the practical aspects obviously being borne chiefly in mind. A review of the more noteworthy advances during 1928–9 is included, extending to 14 pages. Possibly it would have been an added convenience, to facilitate ease of reference, to include an author index in each number, as is done in most abstract journals. The impetus for the publication of this "index" came from the Imperial Research Conference of October 1927, which emphasised the desirability of the research institutions of the Empire being kept abreast of modern developments in preservation and transport of food. The cost of the publication is met from the Empire Marketing Fund.

L. J. H.

BOOKS RECEIVED

(Publishers are requested to notify prices)

Topology. By Solomon Lefschetz, Professor at Princeton University. Colloquium Publications, Vol. XII. New York: American Mathematical Society, 501 West 116th Street, 1930. (Pp. ix + 410.) Price $4.50 net.

Four Place Tables of Logarithms and Trigonometric Functions. Unabridged Edition. Compiled by E. V. Huntingdon, Professor of Mechanics in Harvard University. London: George Allen & Unwin, 40 Museum Street. (Pp. 32.) Price 2*s.* net.

Concerning the Aurora Borealis. By James Halvor Johnson, Berkeley, California. U.S.A.: Gazette Press, 1930. (Private address of author, Turner Terrace, San Mateo, California.) (Pp. 29.)

Atlas Céléste. Par E. Delporte, Astronome à l'Observatoire royal de Belgique. Cambridge: at the University Press, 1930. (Pp. 59.) Price 5*s.* net.

Man and the Stars. By Harlan True Stetson, Director of the Perkins Observatory, Ohio Wesleyan University. London: McGraw-Hill Publishing Co., 6 Bouverie Street, E.C.4. (Pp. xiii + 221, with 25 illustrations.) Price 12*s.* 6*d.* net.

Textbook on Spherical Astronomy. By W. M. Smart, M.A., D.Sc., John Couch Adams Astronomer in Cambridge Observatory and University Lecturer in Mathematics. Cambridge: at the University Press, 1931. (Pp. xi + 414.) Price 21*s.* net.

Electrolytic Conduction. By F. H. Newman, D.Sc., A.R.C.S., F.Inst.P., Professor of Physics, University College of South-West of England, Exeter. London: Chapman & Hall, 1930. (Pp. xii + 441.) Price 25*s.* net.

Concordance de l'Arrangement Quantique, de Base, des Électrons Planétaires des Atomes, avec la Classification Scalariforme, hélicoïdale, des éléments chimiques. Par Charles Janet. Beauvais, France: Imprimerie Departementale de l'Oise, 71 Rue de Pazia. (Pp. 52.)

The Physics of Solids and Fluids, with Recent Developments. By P. P. Ewald, Th. Pöschl, and L. Prandtl. Authorised Translation by J. Dougall, M.A., D.Sc., F.R.S.E., and W. M. Deans, M.A., B.Sc. London and Glasgow: Blackie & Son, 1930. (Pp. xii + 372, with 93 figures.) Price 17*s.* 6*d.* net.

The Rotation of the Galaxy, being The Halley Lecture delivered on May 30, 1930. By A. S. Eddington, M.A., D.Sc., LL.D., F.R.S., Plumian Professor of Astronomy in the University of Cambridge. Oxford: at the Clarendon Press, 1930. (Pp. 30.) Price 2*s.* 6*d.* net.

Radiations from Radio-active Substances. By Sir Ernest Rutherford, O.M., D.Sc., Ph.D., F.R.S., Cavendish Professor of Experimental Physics in the University of Cambridge; James Chadwick, Ph.D., F.R.S., Fellow of Gonville and Caius College, Cambridge; and C. D. Ellis, Ph.D., F.R.S., Fellow of Trinity College, Cambridge. Cambridge: at the University Press, 1930. (Pp. xi + 588, with 138 figures.) Price 25*s*. net.

Colloid Science applied to Biology. A General Discussion held by The Faraday Society, September–October 1930. (Pp. 663–865.) Price 12*s*. 6*d*. net.

The Periodic Law Chart. By W. H. Barrett, M.A., Harrow School. Comprising four separate tables: (*a*) Periodic Table and Atomic Numbers (after Böhr), (*b*) Periodic Table and Atomic Weights (after Mendeleef), (*c*) Periodicity of Atomic Volumes (after Lothar Meyer), (*d*) Melting-Points and Atomic Numbers. Size 6 ft. × 2 ft. 2 in. Unmounted, and packed in cardboard roll, 5*s*. 6*d*. net. Mounted on strong white linen, in cardboard roll, 9*s*. 6*d*. net. Mounted on linen, varnished, on rollers, 15*s*. 6*d*. net.

Mecanique des Fluides. Par M. Henri Villat. Paris: Gauthier-Villars et Cie, 55 Quai des Grands-Augustins. (Pp. vii + 170.) Price, 50 fcs.

The National Physical Laboratory. Collected Researches. Vol. XXII, 1930. London: Published by His Majesty's Stationery Office, Adastral House, Kingsway, W.C.2, 1930. (Pp. 417.) Price £1 net.

The Chemical Investigation of Plants. By Dr. L. Rosenthaler, Lecturer in the University of Berne. Authorised Translation of the Third, improved, and enlarged German edition, by Sudhamoy Ghosh, D.Sc., F.R.S.E., Professor of Chemistry, School of Tropical Medicine and Hygiene, Calcutta. London: G. Bell & Sons, 1930. (Pp. viii + 197.) Price 12*s*. 6*d*. net.

Systematic Inorganic Chemistry from the Standpoint of the Periodic Law. By R. M. Caven, D.Sc. (London), F.I.C., Professor of Inorganic and Analytical Chemistry in the Royal Technical College, Glasgow, and G. D. Lander, D.Sc. London and Glasgow: Blackie & Son, 1930. (Pp. xviii + 510.) Price 9*s*. net.

Artificial Organic Pigments and their Applications. By Dr. C. A. Curtis. Translated from the German *Kunstliche Organische Pigmentfarben*, by Ernest Fyleman, B.Sc., Ph.D., F.I.C. London: Sir Isaac Pitman & Sons, Parker Street, Kingsway, W.C.2, 1930. (Pp. viii + 291.) Price 21*s*. net.

Fundamentals of Organic Chemistry. By Harry F. Lewis, Professor of Organic Chemistry, Institute of Paper Chemistry, Appleton, Wisconsin. London: McGraw-Hill Publishing Co., 6 Bouverie Street, E.C.4, 1930. (Pp. viii + 390.) Price 13*s*. 9*d*. net.

Natural Terpenes. By John W. Baker, D.Sc., Ph.D., A.R.C.Sc., A.I.C., Lecturer in Organic Chemistry in the University of Leeds. London: Methuen & Co., 36 Essex Street, W.C. (Pp. viii + 169.) Price 6*s*. net.

An Introduction to Chemistry. A Pandemic Text. By John Arrend Timm, Assistant Professor of Chemistry, Yale University. With a Foreword by John Johnston, Director of Research, United States Steel Corporation. London: McGraw-Hill Publishing Co., 6 Bouverie Street, E.C.4. (Pp. xviii + 561.) Price 17*s*. 6*d*. net.

Laboratory Exercises in General Chemistry. By John A. Timm, Ph.D., Assistant Professor of Chemistry, Yale University, and Orion E. Schupp, Jr., Ph.D., formerly Laboratory Assistant in Chemistry, Yale University. London: McGraw-Hill Publishing Co., 6 Bouverie Street, E.C.4. (Pp. ix + 136.) Price 6*s*. 3*d*. net.

A Textbook of Practical Physical Chemistry. By K. Fajans, Professor of Physical Chemistry in the University of Münich, and J. Wüst. Translated from the German by Bryan Topley. With a Preface by F. G. Donnan, LL.D., F.R.S. London: Methuen & Co., 36 Essex Street, W.C. (Pp. xv + 233, with 74 figures.) Price 15*s*. net.

The Development of Physiological Chemistry in the United States. By Russell H. Chittenden, Professor of Physiological Chemistry in the Sheffield Scientific School of Yale University. New York: Chemical Catalog Company, 419 Fourth Avenue at 29th Street, 1930. (Pp. 427.) Price $6.00.

A School Course of Chemistry. By J. R. Partington, M.B.E., D.Sc., Professor of Chemistry at East London College, University of London. London: Macmillan & Co., St. Martin's Street, 1930. (Pp. x + 388.) Price 4*s*. 6*d*. net.

British Chemicals and their Manufacturers. The Official Directory of the Association of British Chemical Manufacturers. London: Association of British Chemical Manufacturers, 166 Piccadilly, W.1., 1931. (Pp. 405.)

The British Chemical Plant Manufacturers' Association, Affiliated with the Association of British Chemical Manufacturers, Official Directory of Members, 1931, with a Classified List of their Manufacturers and Services. London: British Chemical Plant Manufacturers' Association, 166 Piccadilly, W.1. (Pp. 151a.)

The Manufacture of Artificial Silk (Rayon), with Special Reference to the Viscose Process. By E. Wheeler, M.B.E., A.C.G.I., F.I.C. With a Foreword by Sir William J. Pope, K.B.E., D.Sc., LL.D., F.R.S. Being Vol. I of a Series of Monographs on Applied Chemistry, under the Editorship of E. Howard Tripp, Ph.D. Second and Revised Edition. London: Chapman & Hall, 11 Henrietta Street, W.C.2, 1931. (Pp. xviii + 177, with 73 figures.) Price 12*s*. 6*d*. net.

Practical Physical Chemistry. By Alexander Findlay, Professor of Chemistry, University of Aberdeen. Fifth Edition, Revised and Enlarged. London: Longmans, Green & Co., 1931. (Pp. xii + 312, with 114 figures in the text.) Price 7*s*. 6*d*. net.

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